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RESEARCH NOTES

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The General Linear Model: A New Perspective on Some Familiar Topics[1]

James Fennessey

ABSTRACT

This paper presents an attempt to explicate some common features of several superficially diverse techniques of data analysis and to indicate how the logic of a single abstract model is relevant to each. The specific purposes are: (1) to show in a relatively intuitive fashion the mathematical relationships between analysis of variance, regression analysis, and analysis of covariance, as special cases of the general linear model; (2) to suggest types of practical applications of analysis of covariance in social research; and (3) to indicate some generally overlooked parallels between the general linear model, especially as formulated in analysis of covariance, and other methodological topics, including Lazarsfeld's elaboration procedure for the $2 \times 2 \times 2$ table, ecological correlations, compositional effects, and others. A final section points out some logical problems which arise in the attempt to apply any of the techniques of this class.

Multiple regression,[2] analysis of variance,[3] and analysis of covariance[4] are procedures with which many sociologists are at least somewhat familiar. However, the three are by no means equally well known, nor are they equally frequently employed in actual research. A consequence, as well as cause, of this uneven acquaintance is a widespread lack of awareness among practicing researchers that these three techniques can be (and in the more mathematical treatments usually are) derived from a single general set of propositions known as the "general linear model." Most

[1] The research reported here was conducted while the author was a predoctoral Fellow supported by a Public Health Service Fellowship 5-F1-MH-29, 193-02 from the National Institute of Mental Health. Doris Entwisle, Leon Gleser, Leo A. Goodman, Clinton Herrick, Nancy Karweit, and especially Robert A. Gordon provided helpful comments on an earlier version of this paper. Although the suggestions made, including some by an anonymous reviewer for the *Journal*, were perceptive and generous, only the author should be held responsible for any shortcomings of the paper in its present form.

[2] Some recent examples of the use of multiple regression analysis are: O. M. Galle and K. Taeuber, "Metropolitan Migration and Intervening Opportunities," *American Sociological Review*, XXXI, No. 1 (February, 1966), 5–13; J. S. Coleman *et al.*, *Equality of Educational Opportunity* (Washington, D.C.: U.S. Office of Education, 1966), chap. iii; R. L. Hamblin and C. R. Smith, "Values, Status, and Professors," *Sociometry*, XXIX, No. 3 (September, 1966), 183–96.

[3] Some recent examples of the use of analysis of variance are: E. Burnstein and R. Zajonc, "The Effect of Group Success on the Reduction of Status Incongruence in Task-oriented Groups," *Sociometry*, XXVIII, No. 4 (December, 1965), 349–62; D. Entwisle, "Developmental Sociolinguistics: A Comparative Study in Four Subcultural Settings," *Sociometry*, XXIX, No. 1 (March, 1966), 67–84; J. R. Wood and Eugene A. Weinstein, "Industrialization and Occupational Evaluation," *American Journal of Sociology*, LXXII, No 1 (July, 1966), 47–57.

[4] The only published research thus far discovered which made explicit use of analysis of covariance is D. J. Bogue and D. L. Harris, *Comparative Population and Urban Research via Multiple Regression and Covariance Analysis* ("Studies in Population Distribution," No. 8 [Oxford, Ohio: Scripps Foundation for Research on Population Problems, 1954]). Another work, published since the present paper was drafted, is Robert A. Gordon, "Social Level, Social Disability, and Gang Interaction," *American Journal of Sociology*, LXXIII, No. 1 (July, 1967), esp. 56–57. See also O. D. Duncan, "Social Stratification in a Pennsylvania Rural Community" (Bulletin 543, Pennsylvania State College, School of Agriculture, Agricultural Experiment Station, State College, Pa.), 1951.

of the treatments which describe this general linear model are in terms that render them inaccessible to researchers without a good deal of formal training in statistics (or mathematics). The primary purpose of this paper is to call attention to this general model, so that sociologists planning or conducting research will be able to consider and utilize all three techniques with more flexibility and efficiency.

At the present time, the potential application of analysis of covariance in particular tends to be overlooked. This may be a result of the often sketchy treatment that the technique receives in most elementary texts.[5] Quite recently, there have been several studies which have employed analytical procedures which are actually identical to analysis of covariance,[6] but without so labeling them. In the belief that such applications can be performed more intelligently when the investigator is able to refer to the literature on analysis of covariance, I shall devote one section of this paper to an illustration of several research situations where analysis of covariance is appropriate.

Having shown the applicability of analysis of covariance in straightforward research situations, I shall go on to indicate how several other important methodological topics can be profitably conceptualized as isomorphic in logical structure to the general linear model. For example, the technique of elaboration of the relations in a $2 \times 2 \times 2$ table, as presented by Lazarsfeld and others, represents a direct application of the linear model logic to the case of dichotomous dependent variables. For several other topics as well, notably the ecological correlation fallacy, the study of compositional effects, the construction of a "standardized" index, and even the percentaging of cross-tables, the logic of linear models is useful in a less direct, though no less central, way.

Beyond the possible benefits to the experienced researcher of seeing the common logic in these various techniques, those who train future sociologists might find it pedagogically helpful to subsume all of these topics, which conventionally are taught separately and without any stress on the interconnections between them, under a single general heading.

I. THE GENERAL LINEAR MODEL

Whenever a solution is sought to the problem of expressing the contribution of one or more influences to the behavior of a variable, some form of the general linear model is potentially applicable. The major substantive distinction between regression analysis, analysis of variance, and analysis of covariance is in the differing levels of measurement of the explanatory variables. The traditional division of the problem into separate topics is mainly a result of the fact that differing computational procedures have been adopted for convenience when the work must be performed manually. With the rapid advances in computer technology and general availability of computer facilities nowadays, this consideration no longer governs. However, the intellectual approach to the topics has not been revised among many of those who actually apply the techniques.

For the remainder of this section, I shall present a brief discussion of the abstract form of the general linear model, along with some explanation of the assumptions that are made. Some use will be made of terminology from linear algebra, but, for those unfamiliar with them, these terms are explained in footnotes. The material in this section is largely derived from several standard works, especially from Scheffé and from Graybill.[7] Each of these, in addition to

[5] Among the elementary texts, one of the clearest treatments of analysis of covariance is that by H. M. Blalock, Jr., in *Social Statistics* (New York: McGraw-Hill Book Co., 1960), chap. xx.

[6] See, for example, B. Eckland, "Academic Ability, Higher Education, and Occupational Mobility," *American Sociological Review*, XXX, No. 3 (October, 1965), 735–46. This technique is described as a "dummy variable regression," but the procedure is actually indistinguishable in its logic or computations from analysis of covariance.

[7] H. Scheffé, *The Analysis of Variance* (New York: John Wiley & Sons, 1959), p. 5 and pp. 192–

presenting a more formal and complete treatment of the ideas here, also provides a section on the relevant aspects of linear algebra and matrix theory.

Consider the following equation and definitions, which constitute one way to express the general linear model:

$$Y_i = \beta_0 X_{0i} + \beta_1 X_{1i} + \beta_2 X_{2i} + \ldots + \beta_P X_{Pi} + \mu_i , \quad (1)$$

in which

- Y_i is the ith observed value of some random variable, and is referred to as the "dependent" variable.
- X_{pi} is the known value for the pth "predictor" variable on the ith observation. Note that p ranges from 0 to P, and so there are $P + 1$ predictor variables in the equation. It is necessary that no single predictor variable be a linear combination of other predictor variables.
- β_p is a parameter of the system which is to be estimated from the data if possible.[8]
- μ_i is a random variable with the following properties: the expectation of μ_i is 0; and the expectation of $\mu_i\mu_j$ is 0 when i is unequal to j and is σ^2 when i equals j.

The properties of μ_i stated above imply that the expected errors of prediction of the Y values have zero mean and equal variance, and are pairwise uncorrelated. As far as the X's are concerned, if X_p *were* equivalent to a linear combination of several other X_q's, then knowledge of X_p would add no information for predicting Y over and above that contained in the X_q's. Thus, if the predictability (information) were to be apportioned between X_p and the X_q's, it could be divided in an infinite number of ways without altering the prediction error, and so no unique solution for the parameters would be possible. Let n denote the total number of observations (assume n is greater than $P + 1$).

Although equation (1) resembles the usual multiple regression equation, it is different in several ways. First, the X_{pi} are defined not as random variables but as fixed quantities. Also, no assumption has been made about the form of the distribution of the μ_i. The Gauss-Markoff Theorem[9] guarantees that, under the assumptions stated, the estimates obtained by a linear least-squares procedure will be unbiased and of minimum variance among all linear unbiased estimates, whatever the form of the distribution of the μ_i. The estimates of the β_p and of the μ_i will be denoted b_p and e_i, respectively. Only if an attempt is to be made to establish confidence intervals or to test hypotheses about the values of the parameters will it be necessary to assume any particular form for the distribution of the μ_i. In practice, if any form is assumed, almost invariably the μ_i are assumed to be distributed according to the normal distribution curve.

Since nothing has been assumed about the X_{pi} except that they are known fixed quantities and that none is a linear combination of others, it is clear that they may be variables which take on only a few values, or even a single value (constants). If such is the case, they are conventionally referred to as "dummy variables" or "counter variables."[10] For instance, in most of the models considered in this paper, the variable X_{0i} is a dummy variable with the

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93; also F. Graybill, *An Introduction to Linear Statistical Models, Volume I* (New York: McGraw-Hill Book Co., 1961). Other useful sources are: K. Brownlee, *Statistical Theory and Methodology in Science and Engineering* (New York: John Wiley & Sons, 1960), pp. 503–38; W. G. Cochran and G. M. Cox, *Experimental Design* (New York: John Wiley & Sons, 1957), pp. 45–58; see also R. A. Bottenberg and J. H. Ward, Jr., *Applied Multiple Linear Regression* (Texas: 6570th Personnel Research Laboratory, Aerospace Medical Division, Air Force Systems Command, Lackland Air Force Base).

[8] In this paper, the case in which the β_p are random variables will not be taken up.

[9] For a discussion of the Gauss-Markoff Theorem, see Scheffé, *op. cit.*, pp. 13–19; see also Graybill, *op. cit.*, pp. 114–17.

[10] See D. B. Suits, "Use of Dummy Variables in Regression Equations," *Journal of the American Statistical Association*, LII (1957), 548–51; see also Scheffé, *op. cit.*, p. 5.

constant value 1. Thus, b_0 is always multiplied by 1.

If all the explanatory variables are nominally or ordinally[11] measured (hereafter to be referred to as "categoric explanatory variables") and if the additional assumption is made that the μ_i are normally distributed, the model of equation (1) is appropriate for analysis of variance. If all the explanatory variables are measured on an interval or ratio scale (hereafter to be referred to as "continuous explanatory variables"), the model can be applied in a multiple regression analysis. Finally, if at least one of the explanatory variables is categoric and at least one is continuous, the situation is that of analysis of covariance. In other words, each of the three procedures constitutes a particular subtype of the general linear model.

II. THE DERIVATION OF ANALYSIS OF VARIANCE, REGRESSION ANALYSIS, AND ANALYSIS OF COVARIANCE FROM THE GENERAL LINEAR MODEL

Since the applicability of the linear model is least obvious when all the predictor variables are categoric, it seems well to begin by presenting rather briefly the mathematical relationships and manipulations by which the general linear model can be applied to analysis of variance. Recall that in the statement of the general linear model, the only assumptions made about the X_{pi} were that they are known fixed numbers and that no one of the X_p is a linear function of any of the remaining ones. For the mathematics of the model, their relaion to the observed scientific variables is rrelevant. In other words, the scientific exlanatory variables may be connected to he predictor variables of the mathematical quation only by some non-obvious rule. 'he importance of this fact will become learer as we progress.

Analysis of variance: a hypothetical example.—Suppose that a questionnaire which includes a vocabulary test has been administered to n people. Suppose also that the interest of the researcher is to determine what influence, if any, a respondent's sex, race, and town of residence have on his vocabulary test score. The vocabulary test score, denoted Y, is a continuous dependent variable which is to be accounted for by knowledge of several categoric explanatory variables (frequently referred to as "factors"). This is a typical situation which might arise in the course of a survey analysis and is one for which analysis of variance is an appropriate technique.[12]

In this example (suppose that there are five towns and three racial categories) there are $2 \times 3 \times 5 = 30$ cells, that is, thirty distinct combinations of levels on the three factors. Assume that in the data there is at least one observation in each cell, but that the number of observations is not necessarily the same in one cell as in another, and moreover that the number of observations in the cells is not proportional to the product of the marginal frequencies. In more technical terms, the design is fully crossed, complete, but not proportional. Under these conditions, the computations become quite lengthy unless approximate methods are used.[13] However, the approach based on the linear model is mathematically identical to the lengthy method and is quite practical with the aid of an electronic computer.

In actual research, varying numbers of replications from one cell to another may arise from the fact that the data were collected on a representative sample and it

[11] See Warren Torgerson, *Theory and Methods Scaling* (New York: John Wiley & Sons, 1958), . 13–40, for a discussion of types of measurement.

[12] It should be mentioned that what has been described is actually a "fixed effects" (Model I) situation—that is, interest is focused only on the observed values of the explanatory variables. The alternative is a random effects situation in which the observed values of the explanatory variables are regarded as sampled from a distribution of values.

[13] See Cochran and Cox, *op. cit.*, pp. 81–82; see also Brownlee, *op. cit.*, pp. 522–26; G. W. Snedecor, *Statistical Methods* (5th ed.; Ames: Iowa State College Press, 1956), pp. 385–88.

simply happened, for instance, that there was a larger number of Negro females in town A. Unequal cell sizes may also arise from technical failures to obtain complete or usable data on some of the intended respondents. These situations are quite common, especially in non-manipulative studies.

Before continuing this section, it seems best to mention a notational complication that arises when one treats analysis-of-variance problems according to the linear model. In the general linear model, the subscripts on Y ran from 1 to n, where n was the total number of observations in the study. For the application to analysis of variance, it is clearer to identify the parameters and the dependent variable by a series of subscripts which directly relate them to a configuration of the factors. Thus, to denote the vocabulary score of an individual, we write Y_{ijkm}, where i ranges from 1 to 2 to denote the level of the sex factor, j ranges from 1 to 3 to denote the level of the race factor, and k ranges from 1 to 5 to denote the level of the town factor. The last subscript, m, ranges from 1 to n_{ijk}, where n_{ijk} is the number of observations in the ijkth cell. Thus, the grand total number of observations, n, is the sum of the n_{ijk} over all three subscripts.

Analysis of variance: preliminary formulation.—Recall that in the three-factor analysis of variance we conventionally allow for the possibility of three two-way interactions and one three-way interaction. Thus, the model for this particular example could be written:

$$Y_{ijkm} = u + s_i + r_j + t_k + (sr)_{ij} + (st)_{ik} + (rt)_{jk} + (srt)_{ijk} + e_{ijkm} \,. \qquad (2)$$

This equation presents the observed vocabulary score of an individual, Y_{ijkm}, as being made up of several fixed components plus a random component. The fixed component u is common to all respondents. The fixed components s_i, r_j, and t_k represent the main effects of the ith level of the sex factor, the jth level of the race factor, and the kth level of the town factor, respectively. There are also fixed components due to the interactive effects of combinations of these factors. These components are denoted by the terms $(sr)_{ij}$, $(st)_{ik}$, $(rt)_{jk}$, and $(srt)_{ijk}$. In denoting the interaction terms, the symbol $(sr)_{ij}$ is used, for instance, to represent the value of the sex-race interaction effect coefficient in the five ij cells (one for each town) and does not imply any multiplication. The random component is denoted e_{ijkm} and is assumed to have the properties described for μ_i in the general linear model.

The fixed components in equation (2) are simply alternative symbols for the parameters in a general linear model. There are seventy-two such parameters in this example as presently formulated. They must be estimated from the data, if possible. The question then arises: in this sort of situation, what rule shall be used to define the known, fixed, linearly independent X_{pi} of the general linear model? One approach would be to define seventy-two dummy variables, each of which can take only two values: 1 if the parameter corresponding to a particular level of a factor or to a particular interaction term is present in a given observation, and 0 if the parameter is absent. With this definition, it is possible to set up for each respondent a vector of dummy variables (1's and 0's) which designates the exact configuration of influences operating on him.

As should be obvious, however, not all of these dummy predictor variables are free to take values independently of one another. In fact, some of the predictor variables in this equation can be expressed as linear combinations of other predictor variables, and this violates one of the assumptions made earlier. For example, if for a particular person the variable corresponding to maleness has the value 1, then the variable corresponding to femaleness for that person must have the value 0. Conversely, if the variable for maleness is 0, then the variable for femaleness must be 1. In other words, if we denote by X_0 the variable corresponding to u (which vari-

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As should be obvious, however, not all of these dummy predictor variables are free to take values independently of one another. In fact, some of the predictor variables in this equation can be expressed as linear combinations of other predictor variables, and this violates one of the assumptions made earlier. For example, if for a particular person the variable corresponding to maleness has the value 1, then the variable corresponding to femaleness for that person must have the value 0. Conversely, if the variable for maleness is 0, then the variable for femaleness must be 1. In other words, if we denote by X_0 the variable corresponding to u (which vari-

able always has the value 1), denote by X_1 the variable for maleness, and by X_2 the variable for femaleness, then $X_2 = X_0 - X_1$. That is, X_2 is a linear combination of X_0 and X_1. More generally, if there are q levels to a factor, then $q - 1$ variables are sufficient to represent its main effects, using the model of equation (2). So, in this initial formulation using seventy-two variables, there will be some redundancies present, both in the main effects and in the interactions.

In terms of the $n \times P + 1$ matrix of predictor variables for all observations, there will be some columns (variables) which are linear combinations of sets of other columns (variables). In other words, although there are seventy-two columns and more than seventy-two rows (assuming n, the total number of observations, >72), the rank of the matrix[14] is less than seventy-two. An elementary theorem of matrix algebra states that the rank of a matrix is equal to the maximum number of unknown variables which can be uniquely determined from the matrix. Thus it is not possible to estimate the seventy-two parameters indicated in equation (2). This redundancy of the variables in the initial formulation is the major mathematical distinction between the general linear model and its applied form in analysis of variance. The elimination of the redundancy is straightforward, however, and its logic can be understood with little mathematical training.

Analysis of variance: elimination of redundancy.—There were seventy-two parameters in the original formulation, but the rank of the matrix was only thirty, so no more than thirty parameters can be estimated. Also, although we wish to give an interpretation to the parameters of equation (2), such an interpretation implies certain linear relations among the parameters beyond those thus far explicitly stated. Because these relations must hold if the model is actually to be defined as we intend, some of the seventy-two parameters of equation (2) can be expressed in terms of other parameters. Making use of these restrictions (relations) thus reduces the number of parameters which must be estimated from the data. In the example here, it will be seen that exactly thirty parameters must be estimated.

To make the parameters interpretable in the intended way, and at the same time to make the system of equations solvable, the implicit linear relations among the parameters are expressed as a series of restrictions stating that the sum of the effect coefficients (parameters) for any given factor or interaction term is zero.[15] Thus, for the main effects, the restrictions are: $s_1 + s_2 = 0$, which implies that $s_2 = (-1)s_1$; $r_1 + r_2 + r_3 = 0$, which implies that $r_3 = (-1)r_1 + (-1)r_2$; $t_1 + t_2 + t_3 + t_4 + t_5 = 0$, which implies $t_5 = (-1)t_1 + (-1)t_2 + (-)t_3 + (-1)t_4$. The summation-to-zero property for the effect coefficients of the interaction terms holds over values of any subscript with the other subscript(s) held constant. For example,

$$(sr)_{11} + (sr)_{12} + (sr)_{13} = 0$$
$$(sr)_{21} + (sr)_{22} + (sr)_{23} = 0$$

Summation over second subscripts for each value of first subscript.

$$(sr)_{11} + (sr)_{21} = 0$$
$$(sr)_{12} + (sr)_{22} = 0$$
$$(sr)_{13} + (sr)_{23} = 0$$

Summation over first subscripts for each value of second subscript.

These summation-to-zero restrictions make it possible to represent many of the seventy-two terms in equation (2) by using

[14] The rank of a matrix is defined as "the largest order, r, of the nonvanishing minors of the matrix" (see Ross A. Beaumont and Richard Ball, *Introduction to Modern Algebra and Matrix Theory* [New York: Holt, Rinehart & Winston, 1954]).

[15] See, for example, Brownlee, *op. cit.*, pp. 409–16.

combinations of other terms, and so the redundant parameters can be removed. In this way, the model can be ***reparametrized,*** that is, written with fewer terms than before. As noted above, $s_1 + s_2 = 0$ implies $s_2 = (-1)s_1$. Thus, if the dummy variable representing maleness is allowed to assume values of $+1$ and -1, it alone can represent the entire main effect of sex. For the other factors, if the dummy variables corresponding to $q - 1$ levels of each factor are allowed to assume values of $+1$, 0, and -1, the other main effects can each be represented using $q - 1$ terms.

Several additional terms can be eliminated from the interaction effect terms. For example, although in equation (2) there were six terms to represent the sex-race interaction, only two terms used in combination are needed when the summation-to-zero restrictions are taken into account. This is true because all six terms of the sex-race interaction can be represented as simple linear combinations of the two terms, that is, each term multiplied by an appropriate dummy variable. Any two terms could be used, just as any one of the levels of the main effects could have been retained or deleted. As an illustration, we have used $(sr)_{11}$ and $(sr)_{12}$. The equivalents are:

Old	*New*
$(sr)_{11}$	$(+1)(sr)_{11} + (0)(sr)_{12}$
$(sr)_{12}$	$(0)(sr)_{11} + (+1)(sr)_{12}$
$(sr)_{13}$	$(-1)(sr)_{11} + (-1)(sr)_{12}$
$(sr)_{21}$	$(-1)(sr)_{11} + (0)(sr)_{12}$
$(sr)_{22}$	$(0)(sr)_{11} + (-1)(sr)_{12}$
$(sr)_{23}$	$(+1)(sr)_{11} + (+1)(sr)_{12}$

This last equivalence is derived from $(-1)\,(-1)\,(sr)_{11} + (-1)\,(-1)\,(sr)_{12}$.

Carrying out the same procedure for the other interaction effects, we find that with the summation-to-zero restrictions the model can be rewritten using only the thirty dummy variables indicated in equation (3), which has thirty terms (from 0 through 29). Each X_p equals either $+1$, 0, or -1, depending on the cell.

Using the formulation of equation (3), all persons in the 111 cell, for example, have the indicated vector of dummy variables after the reparametrization. Similarly, all persons in cell 134, for example, have the indicated vector of dummy variables. The values of the dummy variables for persons in the other cells can be determined by the same sort of procedure.

$$\begin{aligned} Y_{ijkm} = {} & u(X_0) + s_1(X_1) + r_1(X_2) + r_2(X_3) + t_1(X_4) + t_2(X_5) + t_3(X_6) + t_4(X_7) \\ & + (sr)_{11}(X_8) + (sr)_{12}(X_9) + (st)_{11}(X_{[10]}) + (st)_{12}(X_{[11]}) + (st)_{13}(X_{[12]}) \\ & + (st)_{14}(X_{[13]}) + (rt)_{11}(X_{[14]}) + (rt)_{12}(X_{[15]}) + (rt)_{13}(X_{[16]}) \\ & + (rt)_{14}(X_{[17]}) + (rt)_{21}(X_{[18]}) + (rt)_{22}(X_{[19]}) + (rt)_{23}(X_{[20]}) \\ & + (rt)_{24}(X_{[21]}) + (srt)_{111}(X_{[22]}) + (srt)_{112}(X_{[23]}) + (srt)_{113}(X_{[24]}) \\ & + (srt)_{114}(X_{[25]}) + (srt)_{121}(X_{[26]}) + (srt)_{122}(X_{[27]}) + (srt)_{123}(X_{[28]}) \\ & + (srt)_{124}(X_{[29]}) + e_{ijkm}\,. \end{aligned} \tag{3}$$

Cell 111

u	s	r	t	(sr)	(st)	(rt)	(srt)
+1	+1	+1 0	+1 0 0 0	+1 0	+1 0 0 0	+1 0 0 0 0 0 0 0	+1 0 0 0 0 0 0 0

Cell 134

u	s	r	t	(sr)	(st)	(rt)	(srt)
+1	+1	−1 −1	0 0 0 −1	−1 −1	0 0 0 −1	0 0 0 −1 0 0 0 −1	0 0 0 −1 0 0 0 −

The model described above, and reflected in equation (3), is a *full* model. That is, it makes use of all the information available in the data. For any given configuration of data, there is in fact an infinity of full models, each of which implies distinct assumptions about the substantive processes which are operating. The particular set of restrictions employed here to complete the definition of the model are the ones used most frequently in experimental research, but only because they happen to describe the kind of process which is thought to be operating. In many survey situations, on the other hand, it might be more appropriate to introduce a different set of restrictions. For instance, instead of saying that the unweighted sums of the effect coefficients are zero, we might want to say that it is a certain weighted sum which must be zero.

It should also be noted that a model need not make use of all the information which the data can provide. In fact, the usual hypotheses of analysis of variance are tested by comparing the adequacy of a model which ignores some of the information in the data with the adequacy of the full model.

In order now to consider testing the usual analysis-of-variance hypotheses about the significance of the main and interaction effects, it is necessary to assume that the μ_i (and consequently, the e_{ijkm}) are distributed normally. If no distributional form is assumed for the μ_i, then only point estimates of the β_p (i.e., the effect coefficients) and/or linear functions thereof can be obtained.

The actual procedure for testing the various hypotheses involves the calculation of a series of separate sets of estimates and separate sums of squared residuals. In other words, for a series of models, each different from the others in the parameters it includes, the parameters are estimated and the sums of the e^2_{ijkm} are calculated. In brief, the object is to see if a significant loss in predictive accuracy is incurred by ignoring predictive information from certain of the explanatory variables. The predictive accuracy is inversely related to the sum of squared residuals. These error sums of squares (*ESS*) are sums of squared differences between the observed values of *Y* and the values predicted on the basis of the model used in that equation. The first equation makes use of all thirty terms as indicated in equation (3). The *ESS* associated with this equation is the smallest possible with the given data, since it makes use of all possible predictive information. In succeeding equations, various subsets of the thirty parameters (each subset containing exactly the dummy variables which represent a particular main effect, interaction effect, or combination thereof) are set to equal zero, which is equivalent to ignoring the predictive information from those dummy variables. The *ESS* for each of these equations is then calculated. To determine whether the difference between the *ESS* for the thirty-term equation and the *ESS* for another equation is significant, a conventional *F*-ratio test is carried out on a statistic which adjusts each *ESS* for its degrees of freedom.[16] If the *ESS* from the equation in question is significantly larger than that from the thirty-term equation, the conclusion is that the information contained in the deleted terms does truly improve the accuracy of prediction.

There are standard computer programs which carry out just this sort of procedure, calculating the series of equations and the *F*-ratios for a number of hypotheses specified by the investigator.[17]

[16] See Bottenberg and Ward, *op. cit.*, pp. 125–26; or Cochran and Cox, *op. cit.*, p. 53.

[17] For example, a FORTRAN program for the IBM 7094 has been written at the Health Sciences Computing Facility of U.C.L.A. It is described in W. J. Dixon (ed.), *BMD Biomedical Computer Programs* (Los Angeles: Health Sciences Computing Facility, Department of Preventive Medicine and Public Health, School of Medicine, University of California, January, 1964). The title of the program is BMD05V, "The General Linear Hypothesis." Another program which can be employed for this sort of analysis is a general multiple regression program in use at the Department of Social Relations, Johns Hopkins University. The program is called CROMP.

This sketchy presentation of the analysis of variance via the general linear model has been intended more as an introduction than as a full discussion. Further material is available in the references, especially those cited in this section.

Regression analysis.—Application of the general linear model to multiple regression analysis is considerably simpler than that to analysis of variance. In fact, it is generally in the context of regression analysis that sociologists become acquainted with linear models. In regression analysis, as in analysis of variance, if the normality of errors assumption is made, one can analyze the variance due to the explanatory variables, ascertain its significance, and proceed in the same manner as indicated for the analysis-of-variance situation, and can also test hypotheses about specific values of the parameters.

Note that in regression applications the subscript on the dependent variable, and those on the independent variables which indicate particular observations, run from 1 to n. In regression analysis, the X_{pi} are continuous variables, which can (at least theoretically) take on any real values, and there is no reason to presume that the value of X_p is the same for any two observations. Also, in regression applications, there is ordinarily no problem of linear dependence between two or more predictor variables, and so usually no reparametrization is needed.

There is a problem in the fact that the X_{pi} are assumed by the linear model to be known, *fixed* quantities, not random variables. To circumvent this obstacle, the X_{pi} are regarded as particular realizations of the random variables from a multivariate distribution. Then the parameters and confidence intervals are estimated *conditional* upon these particular values of the X_{pi}.[18] If it is desired to estimate confidence intervals under repeated sampling of all variables—as is usually the case in social research—the assumption of normally distributed μ_i must be made, and also it is necessary to assume that the X_{pi} are distributed in such a way that their marginal distributions do not involve the β_p or the σ^2 at all.[19] In such situations, the formulas used to estimate the confidence interval are different, and the interval somewhat wider, than for fixed X_{pi}.

Analysis of covariance.—We turn now to a discussion of the general linear model and analysis of covariance. To understand this technique, first consider a two-factor analysis of variance, with one factor labeled G and the other factor labeled H. Assume for the sake of the presentation that there is no interaction and that the number of cases is the same in every cell. Under these conditions, the estimated relation between Y and a particular combination of levels of G and H can be written as follows:

$$Y_{ijm} = u + g_i(+1) + h_j(+1) + e_{ijm}\,, \quad (4)$$

where i ranges 1 to I, j ranges 1 to J, and m ranges 1 to n_{ij}.[20]

Suppose now that the problem is modified, and interest centers instead on a continuous variable F as explanatory of Y. In symbols, the model could be written:[21]

$$Y_i = b_0 + b_1(f_i - \bar{f}.) + e_i\,, \quad (5)$$

where i ranges 1 to n. The $h_j(+1)$ in equation (4) and the $b_1(f_i - \bar{f}.)$ in equation (5) are identical as formal terms (although of course not necessarily in numerical value); the only differences are notational conventions in the subscripts. Also, under these conditions, b_0 is simply an alternate

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symbol for the constant term. There is thus no reason why the term $b_1(f_i - \bar{f}.)$ cannot be inserted into equation (4), replacing the $h_j(+1)$. To resolve the notational differences, we need only rewrite as follows: $b_1(f_{im} - \bar{f}..)$. The reader who has followed this far will realize that the resulting equation

$$Y_{im} = u + b_1(f_{im} - \bar{f}..) + g_i + e_{im}\,, \qquad (6)$$

where i ranges 1 to I, and m ranges 1 to n_i, contains one predictor variable which is a true continuous variable and another set of I predictor variables which represent the levels of a categoric factor. This is exactly what is done in the simplest form of analysis of covariance.

In the one-factor analysis of variance, $\bar{Y}_{i.} = u + g_i$. In the analysis of convariace, $\bar{Y}_{i.} - b_1(\bar{f}_{i.} - \bar{f}..) = u + g_i$. In simple regression analysis, if each observation were to be assigned to one of I subsets, then $\bar{Y}_{i.} = b_0 + b_1(\bar{f}_{i.} - \bar{f}..)$. But in analysis of covariance, $\bar{Y}_{i.} - g_i = b_0 + b_1(\bar{f}_{i.} - \bar{f}..)$.

What all this indicates is that analysis of covariance has a unique duality. On the one hand, attention can be directed to the impact of a categoric variable, G, while a continuous variable, F, is being controlled (the word "control" here implies that certain assumptions must be true if the procedure is to be appropriate). Conversely, covariance analysis can also be used when interest is on the impact of a continuous variable, F, while a categoric variable, G, is being controlled (again with certain assumptions required). Hubert Blalock has pointed out this duality.[22]

The first case considered by Blalock is the estimation of the effects of a categoric variable, while allowing for the effects of some continuous variable (usually referred to as a "covariate" or a "concomitant variable"). The second case is the converse of this and occurs when interest centers on the relation between two continuous variables, while controlling for the effects of some categoric variable by obtaining relationships within categories.

Mention of the calculation of relationships within categories brings up one major problem that often arises in the execution of an analysis of covariance. In equations (5) and (6) we have used a single constant, b_1, to denote the slope of the regression line of Y upon F. It is quite possible, however, that the slopes will be different for each category of G (i.e., more generally, for each cell of the design). If this should be the case, interpretation becomes problematic. The existence of different slopes in each cell is equivalent to what would be called an "interaction effect" in analysis-of-variance terminology. Whenever there is such an effect, the model becomes substantively less appropriate, though still mathematically solvable, and so some compromise must be made. We shall return to this point later.

Note that what has been presented here is the simplest possible example of analysis of covariance. Actually, there is no reason why analysis of covariance cannot be carried out with several categoric variables and several continuous variables in the same equation. Electronic computers make the computations a routine matter; any difficulties that may emerge are in making interpretations of the complex results.

III. FOUR TYPES OF APPLICATION FOR ANALYSIS OF COVARIANCE

W. G. Cochran has suggested several possible uses for analysis of covariance.[23] Of these, two seem particularly promising in sociological research. The first use is to *increase the precision* of a study by extracting the variation in the dependent variable which is due to irrelevant predictor variables. The second use is to *increase the accuracy* of a study by removing bias from the estimate of the influence of an explanatory variable which is associated or

[22] Blalock, *op. cit.*, pp. 360 ff.

[23] W. G. Cochran, "Analysis of Covariance: Its Nature and Uses," *Biometrics*, XIII (1957), 261–81.

confounded[24] with another explanatory variable. By crossing this dichotomy with that proposed by Blalock, we obtain four general cases where analysis of covariance can be useful. In each of them the only basic requirement is that at least one explanatory variable be categoric and at least one be continuous. This section of the paper will illustrate each of the four cases by means of a hypothetical situation in which the dependent variable is, as before, the score on a vocabulary test, denoted Y. The explanatory variables will be town of residence, which is categoric (with five categories), and annual salary, which is continuous.

Actually, of course, since analysis of variance and regression analysis also derive from the same general model, they too can be used for the same four kinds of strategic purposes (assuming the appropriateness of the data). Although I shall not attempt to amplify this point here, it is one which illustrates the kind of epistemological gains which can arise from focusing on the common foundations of the three techniques.

The first case to be considered will be that where interest centers on the effects of town of residence upon vocabulary score. Assume, for the present, that the distributions of annual salary are the same in each town. This is equivalent to saying that there is no association or confounding of salary with town. The estimated equation for an individual observation can be written:

$$Y_{im} = u + b_1(s_{im} - \bar{s}_{..}) + t_i + e_{im}\,, \qquad (7)$$

where s_{im} denotes the individual salary, and t_i denotes the effect of the ith town, and where m runs from 1 to n_i, n_i being the number of observations in town i. Note that a single slope, b_1, has been used for all of the towns. Without this assumption, full interpretation cannot be made.

Since it has been assumed that the salaries are distributed identically in each town, then the average salary in each town equals the over-all average of the salaries; $\bar{s}_{1.} = \bar{s}_{2.} = \bar{s}_{3.} = \bar{s}_{4.} = \bar{s}_{5.} = \bar{s}_{..}$ Since this is true, the average vocabulary score in each town, $\bar{Y}_{i.}$, can be written:

$$\bar{Y}_{i.} = u + t_i\,, \qquad (8)$$

since $b_1(\bar{s}_{i.} - \bar{s}_{..}) = b_1(0) = 0$ and since the sum of the e_{im} will always be zero (because any non-zero error will be absorbed by the parameters).[25]

Thus the estimate of the t_i obtained in this situation is the same as would have been obtained without using any covariate in the equation. But—and this is the benefit of this particular application of analysis of covariance—if the covariate had not been used, the error term (which is used as the denominator in the F-ratio tests) would have been considerably larger than it is when the covariate is used. If no covariate were used, the variation due to individual differences in salary would have been incorporated into the error term, so that the new error term, denoted e^*_{im}, would be equal to $[b_1(s_{im} - \bar{s}_{..}) + e_{im}]$. By using the covariate, we obtain a smaller denominator in the F-ratio and so can more easily obtain a given level of significance. The precision of the analysis has been increased. Recall that if the calculated F-ratio is less than the critical value in some given analysis, the strictly correct conclusion is that we have failed to demonstrate any effects of town on vocabulary score and not that such effects are actually absent. Strictly speaking, the researcher should always be aware of the possibility that his data are insuffcently sensitive; in practice, however, many workers tend to blur these fine distinctions. For this reason, any technique which improves the utilization of a given batch of data is an important practical aid.

[24] If the observed values of two predictor variables are not orthogonal, the variables are said to be "associated" or "confounded." If both variables are random variables, the term "association" is used; otherwise, they are said to be "confounded." In either case, the point is that better than chance accuracy can be obtained by using some linear function of one to predict the other.

[25] See Johnston, *op. cit.*, p. 113.

As the second of the four classes of situation, suppose that interest still centers on the influence of town upon vocabulary score but that the assumption is not made that the distributions of salary are the same in each town. In this case, the average vocabulary score in each town could be written:

$$\bar{Y}_{i.} = u + b_1(\bar{s}_{i.} - \bar{s}_{..}) + t_i . \quad (9)$$

If the covariate had been ignored, this would have been

$$\bar{Y}_{i.} = u + t_i^* ,$$

where

$$t_i^* = [b_1(\bar{s}_{i.} - \bar{s}_{..}) + t_i] .$$

Thus, for some towns, t_i^* would overestimate t_i, and for others it would underestimate it. In this second class of situations, use of the covariate not only continues to improve the precision of the analysis but also increases the accuracy by eliminating a distortion in the estimated values of the t_i.

The situations considered so far are the classic ones, in which the continuous variable is regarded more or less as an interference, a nuisance variable, which obscures the study of the influence of the categoric variable upon the dependent variable. But, as mentioned above, analysis of covariance is equally applicable when interest is on the continuous variable. Consider again the same variables as in the preceding example, but suppose now that interest is focused on the influence of salary upon vocabulary scores.

It can be demonstrated that using town as an additional explanatory variable, and extracting its contribution to the variance of the vocabulary scores, leads to an improvement in the precision of estimate of b_1, the slope parameter. This, again, is under the assumption that all the within-town slopes are equal. Moreover, if the distributions of salary for each town are not the same, using town as a predictor not only improves the precision but also removes bias which would otherwise be present in the estimate of b_1. The situation can be represented clearly by means of graphs indicating some idealized configurations of data.

Figure 1 presents an idealized form of the situation in which the two explanatory variables are not associated or confounded, that is, the distribution of salary levels is the same in all towns. According to the graphed data, towns A and B tend to raise the vocabulary score, while towns C, D, and E tend to lower it. The slope of the line relating salary to vocabulary score is the same in all five towns. In particular, the graph is scaled so that the slope, b_1, is unity. Imagine now that the graph were squeezed to the left, until all the points were to lie along the ordinate. Obviously, there would be considerable variation in these points and variation among those from a single town also. But, if the points were "adjusted" for the influence of salary level, all those in town A, for example, would fall at a single point on the ordinate, as indicated by the notation $u + t_1$. Thus it is seen that, when interest is on the effect of the categoric variable, the use of the covariate increases precision. This is the same point that was made in the earlier algebraic argument.

Suppose now that the interest is on estimating the slope. The precision of estimate of the slope, b_1, depends upon how widely scattered the points are from the line defined by that slope in relation to their total scatter. In the idealized data shown in Figure 1, if the appropriate t_i is subtracted from every observation, the points all come to lie exactly on the line defined by b_1. Thus, using town as an additional predictor, the slope is estimated with perfect precision in this artificial example.

Figure 2 illustrates an idealized form of the situation in which there is a difference in the distribution of salary from one town to another. According to the graph, the average salaries in each town are: town A = \$5,400; town B = \$5,400; town C = \$5,500; town D = \$5,600; and town E = \$5,600. Note that the actual town

effects are the same as in Figure 1, and that the within-town slopes are again identical and all equal to unity. In Figure 2 it is clear that if the over-all slope were estimated, the estimate would not be 1; similarly, as described in the earlier algebraic argument, if the individual town effects were estimated by a simple one-factor analysis of variance, the t_i obtained would be incorrect. In fact, the way the data lie the apparent effect of town on vocabulary score would be almost entirely washed out.

Problems of interpretation.—Thus far, a single set of variables has been used to illustrate four classes of situations in which analysis of covariance can be of aid. In each of these situations, the slopes of the lines relating salary to vocabulary test score were assumed to be identical for all five towns.[28] Let us now consider the implications if the slopes are not the same.

When the slopes are not the same, but the distribution of salaries is identical for each town, the procedure becomes somewhat different, but the equations can still be solved for estimates of the effect of each town, and of the slope within each town. To obtain a slope within a town, we simply calculate as if the town were the entire sample. To obtain the t_i, we simply carry out the one-factor analysis of variance on the residuals from each within-town regression line. Under these conditions, the t_i and b_{1i} obtained are unbiased, but they no

[28] In practice, of course, the observed slopes need be only approximately equal, since sampling fluctuations operate to shift them slightly. Bottenberg and Ward, *op. cit.*, give a discussion of tests for equality of slopes on pp. 110–15.

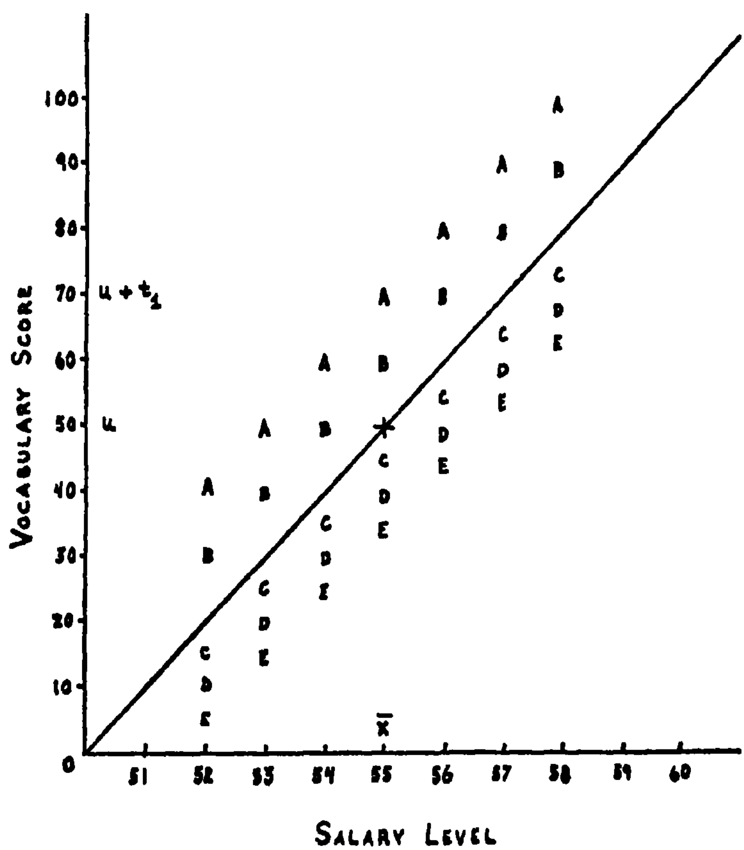


FIG. 1

longer tell the whole story. The several b_{1i} cannot be legitimately combined into a single average b_1, since this does not reflect reality. Nor can an F-ratio test be legitimately carried out on the over-all effect of town, since part of the effect of the town is through its changing the slope, and this portion will be ignored when the slope is controlled out.

The mathematics of the procedure changes in a relatively minor way if the slopes are unequal, but the linear model which has been used does not then truly portray what is going on in the data. Nevertheless, the analysis-of-covariance logic is useful, since without it there is no way to discover the true character of the data. This same reasoning is applied in analysis of variance, when interaction is a possibility. With unequal slopes, as with interaction, the interpretability is limited to providing descriptive statements of what can be expected under certain combinations of factor levels. The scientific implication is that new variables must be constructed or discovered (whether by transformations of present variables or by measuring new properties of the respondents) if a linear model is to be applied.

If there is both a difference in the slopes from one town (cell) to the next, and a difference in the distributions of salary for each town (confounding of distribution), it is mathematically possible to obtain unbiased estimates of the t_i and the b_{1i}, provided there are at least three observations in each town (that is, enough to provide a least-squares estimate of two parameters in each town). To determine the t_i, one can

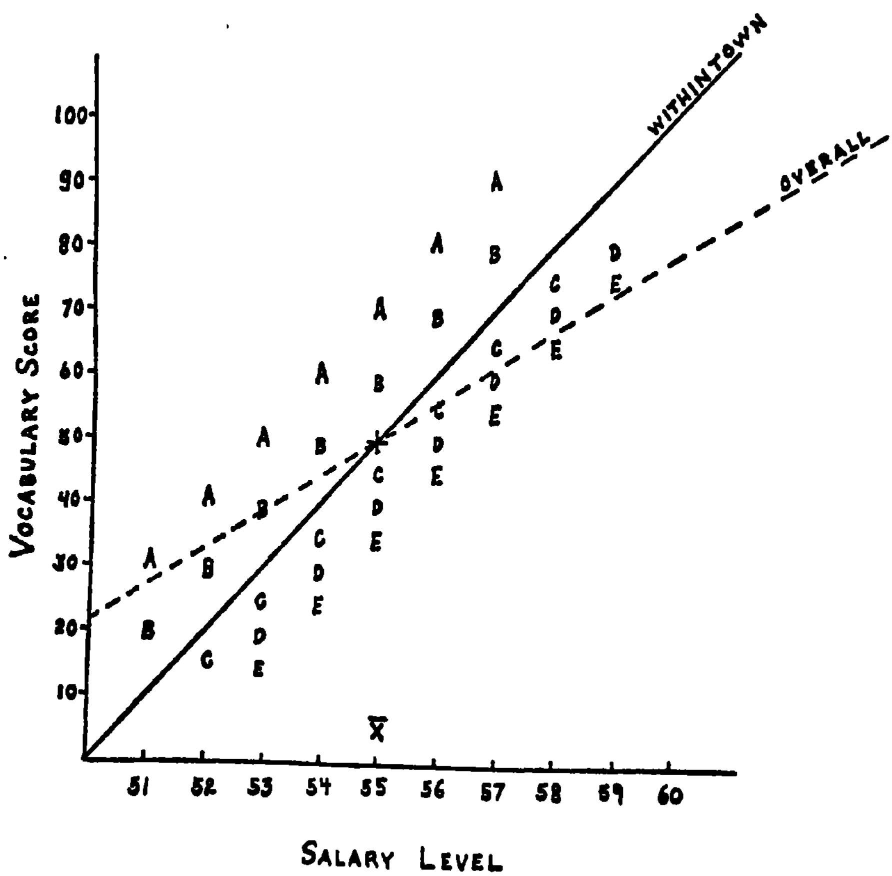


FIG. 2

extend each of the separate regression lines through a perpendicular erected at the grand average of the salary distributions $\bar{X}_{..}$. Differences in these intercepts provide one reasonable basis for estimating the t_i. In this case, however, one can never be sure that, if the distribution of salaries in town A had been the same as in town C, the slope in town A would not have become the same as the slope in town C. In other words, curvilinearity in the regression function may have produced the distinct slopes. Thus, as before, the interpretation is severely restricted.

Neither the procedures just described nor those suggested for the case where the slopes are different but the distributions of X are identical are exactly equivalent to the simultaneous inclusion of all the dummy variables and the covariate in a single model. Moreover, it is only the latter procedure which can be strictly described as analysis of covariance. However, the logic of the procedure is the same for all four situations, and it is that similarity which is our primary concern in this discussion.

Authors who have treated analysis of covariance have issued cautions regarding its use if the within-cell slopes are different from one another. This caveat is identical to the one against making substantive interpretations in ordinary analysis of variance if there is significant interaction.[27] They have also, however, warned about the use of analysis of covariance if the "treatment affects the covariate."[28] This last condition can be rephrased as referring more generally to any association or confounding between the categoric factor and the continuous variable. In this case, the advice requires some qualification. In many situations, as long as equal slopes can be assumed, it is perfectly legitimate to proceed when there is an association between the categoric explanatory variable and the continuous one.[29] A certain amount of power is lost, as can be seen by considering an extreme case, in which there is no overlap in scores on the covariate between certain cells. Under these circumstances, there is no way of ascertaining the linearity of the slope for points in the interval which is not covered in any cell. The linearity then rests on an extrapolation, which is not only risky but is risky to an unknown degree. On the other hand, fairly small degrees of association (as measured perhaps by a correlation ratio) do not seriously lower the power of the technique any more than fairly small associations between continuous variables in multiple regression make it not applicable.

Any technique which is designed to separate out the influence of several variables becomes less effective when the variables covary to a considerable extent. Conversely, if the variables vary completely independently of each other, the technique will be optimally efficient, but its only utility will be to increase the precision of estimate. One must use any technique while recognizing its limitations, and realize that discovering that a set of data cannot answer the research question is preferable to using those data as support for an erroneous answer.

In this section, the illustrations have used only the simplest possible forms of analysis of covariance. In practice, it is quite likely that more variables will be involved. The aim here is to point out that analysis of covariance complements the other two more widely known techniques of linear statistical analysis, and together with them provides the investigator with a single general approach he can use in a variety of situations.

An alternative approach to analysis of covariance.—As background for the next section of this paper, I shall discuss briefly analysis of covariance from a different perspective, one which parallels the traditional approach to analysis of variance. Analysis

[27] See Scheffé, *op. cit.*, p. 94.

[28] See Cochran and Cox, *op. cit.*, p. 90; Bottenberg and Ward, *op. cit.*, pp. 85-95; Brownlee, *op. cit.*, p. 330.

[29] See Scheffé, *op. cit.*, p. 198.

of variance has a fundamental theorem which, in simplest form, can be expressed:

$$\sum_{i=1}^{I}\sum_{m=1}^{n_i}(Y_{im}-\bar{Y}_{..})^2 = \sum_{i=1}^{I}\sum_{m=1}^{n_i}(\bar{Y}_{im}-\bar{Y}_{i.})^2 + \sum_{i=1}^{I} n_i(\bar{Y}_{i.}-\bar{Y}_{..})^2 .$$

Or, letting SS denote the "sum of squares," $SSY_{\text{total}} = SSY_{\text{within}} + SSY_{\text{between}}$. This would be the formulation for a one-factor analysis of variance. In general, the total variance can be legitimately partitioned into a number of components which are independent and additive (as long as the predictors are non-confounded). For our purposes, the simplest formula will serve. Analysis of covariance has a similar formula in which cross-products replace squares. This formula includes the analysis-of-variance formula as a special case (that where $X = Y$). In this formulation of analysis of covariance, attention is concentrated more on the behavior of the sums of products than on the structure of the linear model.

The analysis-of-covariance theorem can be written:

$$\sum_{i=1}^{I}\sum_{m=1}^{n_i}(X_{im}-\bar{X}_{..})(Y_{im}-\bar{Y}_{..}) = \sum_{i=1}^{I}\sum_{m=1}^{n_i}(X_{im}-\bar{X}_{i.})(Y_{im}-\bar{Y}_{i.}) + \sum_{i=1}^{I} n_i(\bar{X}_{i.}-\bar{X}_{..})(\bar{Y}_{i.}-\bar{Y}_{..}),$$

or, letting SP denote the "sum of products," we have $SPXY_{\text{total}} = SPXY_{\text{within}} + SPXY_{\text{between}}$.

In words, this says that the total sum of products can be partitioned into components, one component being the sum of products within categories of some third variable which will be labeled A; and another component being the sum of products between categories of A.[30]

To say that there is a non-zero sum of products between categories of A (the second term on the right-hand side of the formula) is equivalent to saying that the distribution of the X is not the same in all the categories. If it were the same, then all the $(\bar{X}_{i.} - \bar{X}_{..})$ would equal zero, and so each term of the sum would have to equal zero. Whenever, in the earlier discussion of analysis of covariance, it was stated that the distribution of the covariate was the same in every cell, this was equivalent to saying that the $SPXY_{\text{between}} = 0$.

IV. THE ANALYSIS OF COVARIANCE AS PARADIGM

As mentioned at the beginning, the general linear model, and especially the analysis of covariance, is well suited to serve as a logical paradigm for several topics in sociological research methods. The analysis of covariance is unique as an application of the general linear model in that it invites the construction of several separate graphs describing the relationships between two continuous variables, X and Y, where each graph depicts the relationship between X and Y for just the observations from one of the several categories of a third, categoric variable, which might be denoted by A. Analysis of covariance thus makes intuitively clear the notion of *conditional* relationship, that is, a relationship between two variables which holds for a fixed value of some third variable. Note that this conditional relationship is distinct from the conditional relationship referred to in discussions of hypothesis testing, where what is meant is the relationship between the dependent and explanatory variables, conditional upon the particular observations made.

[30] See O. D. Duncan, R. Cuzzort, and B. Duncan, *Statistical Geography* (Glencoe, Ill.: Free Press, 1961), p. 65 *passim;* see also, E. F. Lindquist, *Design and Analysis of Experiments in Psychology and Education* (Boston: Houghton Mifflin Co., 1953), pp. 319–23.

The same logic of conditional relationships applies in analysis of variance; here, however, the variable which corresponds to X in the analysis of covariance has only a discrete set of levels of effect on Y, rather than a neat linear relationship with Y, thus making the notion of conditional relationship less apparent. In multiple regression problems, the logic of the conditional relationship again applies. The partial regression coefficient $b_{yk.j}$ is an estimate of the linear relationship between Y and X_k after X_j (itself used only in a linear fashion) has been allowed to extract all the variance it can from both of them. The partial correlation coefficient is an estimate of the predictive accuracy that could be expected using the relationship. These partial statistics have as one of their components the weighted averages of the conditional statistics between Y and X_k for each of an extremely large number of categories of X_j. The conditional relationships are assumed to be identical in the population, and any observed discrepancies between them are ascribed solely to sampling fluctuations.

In other words, if the categoric variable is conceived as continuous, analysis of covariance becomes regression analysis. The assumption of equal conditional relationships, as mentioned just above, would be made (which corresponds to the equality of slopes in analysis of covariance), but there would be no direct way of checking on this equality. A possible indirect check would be an investigation of the homoscedasticity and randomness of the residuals. There would also be no possibility of discovering non-linear effects of the explanatory variable (except by including some new variable which is a non-linear function of the explanatory variable), since multiple regression assumes linearity of effects. If, on the other hand, the covariate were regarded as categoric, the procedure would reduce to analysis of variance with two factors. In this case, it would be possible to test the linearity of influence of the two factors, and the presence of interactive effects. Thus, there is a common abstract mathematical model for analysis of variance, regression analysis, and analysis of covariance; and in addition, the analysis of covariance also can serve as a substantive paradigm for the other two techniques.

Note that the analysis-of-variance model uses up several degrees of freedom in order to make estimates of the various effect coefficents. Not all of these degrees of freedom are used in analysis of covariance, and even fewer are used in multiple regression analysis. Thus multiple regression has the greatest power, in the sense that it leaves the largest number of degrees of freedom available for the residual variance estimate. This tends to make that estimate smaller and, in any case, brings it to a point in the F-ratio tables where smaller values are required for significance.

This last remark illustrates a fundamental point. If more restrictions (assumptions) are made about the data, there will always be a gain in the precision of estimates of the parameters required, and fewer parameters will be required. Conversely, every test of an assumption, or every assumption not made, reduces the maximum possible precision and increases the number of parameters which must be estimated. Thus, in many situations, it is problematic whether the use of a certain assumption will give enough gain in precision to offset a possible loss due to the poor fit of the data to the assumption.

It may have seemed that the decision to make, or not make, this or that assumption is more or less arbitrary. This is emphatically not the case. The present paper concentrates on similarities and differences among the various subtypes of the general linear model, but this should not be interpreted as implying that any subtype can be successfully applied to any given set of data. The validity of the conclusions from any of these procedures is only as good as the *evidence* (either from the data or a priori) that the assumptions all are true. In order not to seem too gloomy, however, it should be mentioned that some recent studies have indicated that certain assump-

tions can be met only approximately and still have the procedure yield valid results. This topic is known as the "robustness of a model" vis-à-vis its several assumptions.

Having made these last observations on the conventional applications of the linear model, we are ready to turn to a consideration of its relevance to other methodological topics.

Lazarsfeld's technique of elaboration.—Lazarsfeld's technique for the elaboration of relationships among attribute (categoric) variables[31] can be shown to be a slight conceptual extension of analysis of covariance but is identical in the basic logic. Lazarsfeld's basic equation, applied to a $2 \times 2 \times 2$ table, as illustrated below, is as follows: Define

$$[XY] = \frac{ad - bc}{n^2}$$

$$\begin{array}{cc|c|c|} & & \multicolumn{2}{c}{Y} \\ & & H & L \\ \hline X & H & a & b \\ \hline & L & c & d \\ \hline \end{array} \qquad a + b + c + d = n\,; \qquad (10)$$

then if there are two separate 2×2 tables, one containing all observations in which some third variable T is $+$, and the other containing all observations in which T is $-$,

$$[XY] = \frac{n_{T+}}{n}(XY \cdot T+) + \frac{n_{T-}}{n}(XY \cdot T-) + \frac{n^2}{(n_{T+})(n_{T-})}(XT)(YT). \qquad (11)$$

If each term is multiplied by n and the terms are grouped, we obtain

$$n[XY] = [n_{T+}(XY \cdot T+) + n_{T-}(XY \cdot T-)] + \frac{n^3}{(n_{T+})(n_{T-})}(XT)(YT). \qquad (12)$$

This last equation begins to resemble the one for analysis of covariance discussed above on page 16, $SPXY_{total} = SPXY_{within} + SPXY_{between}$. Actually, it can be shown that, if numerical values of 1 and 0 are assigned to the high and low categories of variables X, Y, and T, and if the standard formula on page 16 is applied, the resulting terms are identical to those obtained from using equation (12). (The author has available on request a detailed proof of the numerical equivalence of the $SPXY$ formula and Lazarsfeld's equation as written above.) As far as I have been able to determine, Lazarsfeld has never presented his technique along these lines. Yet the left-hand side of equation (12) is a total sum of cross-products; the first term on the right-hand side (in brackets) is the weighted sum of the within-category cross-products; and the second term on the right-hand side is the between-categories cross-product. Lazarsfeld's procedure meets all the requirements of the general linear model except those relating to the distribution of the μ_i. Contrary to the requirement for the linear model, the expected value of $\mu_i\mu_j$ is not zero.[32] This failure to meet all the assumptions, as well as some additional complications which arise in the conceptualization and treatment of interactive effects, implies that one should not try to apply computing procedures from the ordinary linear model indiscriminately to situations in which there is a categoric dependent variable. In fact, the handling of such situations is a topic which has received considerable attention in the more technical statistical literature. On the other hand, these differences do not in any way vitiate the logical analogy (that is, the similarity in the way the processes are conceived to be operating) between the

[31] See P. F. Lazarsfeld, "The Algebra of Dichotomous Systems," in H. Solomon (ed.), *Studies in Item Analysis and Prediction* (Stanford, Calif.: Stanford University Press, 1961); see also, B. M. Russet *et al.*, *World Handbook of Political and Social Indicators* (New Haven, Conn.: Yale University Press, 1964), pp. 323–24.

[32] See the properties of μ_i as described in Sec. I above.

general linear model and Lazarsfeld's procedure.

Merely demonstrating the mathematical equivalence of the Lazarsfeld equations and those for analysis of covariance is of little value unless it also clarifies practical or theoretical problems of substantive interest. The Columbia University methodologists have described several possible configurations that the data in a $2 \times 2 \times 2$ table might take. They have coined the terms "marginal relationship," "partial relationship," "conditional relationship," "contingency effect," "intensifier effect," and "suppressor effect." These terms can be consistently interpreted in ordinary analysis-of-covariance terminology.

The partial and the marginal relationships correspond to the distinction between the "within-cells" relationship and the "between-cells" relationship. If the distribution of X is the same for both $T+$ and $T-$, then $(XT) = 0$, and so the "between" relationship is zero. In other words, the marginal relationship is zero. This question of the presence or absence of a marginal relationship constitutes the second of two major choice points in the analysis. The first such major choice point is that at which the question of equality of relationships within each category of the control variable is answered. In covariance analysis, this is the question of equal slopes; in analysis by Lazarsfeld's technique, the corresponding question asks whether the cross-product $(XY \cdot T+)$ is the same as the $(XY \cdot T-)$. If the slopes—and in the Lazarsfeld method, the cross-product statistics—are the same, then one can legitimately talk about the separate effects of X and T on Y. It should be mentioned that the use of the word "equal" here is meant to be approximate. One of the limitations of the model is that significance tests for these questions have only recently begun to be discussed in the sociological literature.[33]

If the two statistics are not equal, there have been several names applied to the various configurations which the difference might take. The term "contingency effect" seems to apply to the situation in which one of the cross-product terms is (approximately) zero and the other is non-zero. This is a substantively interesting situation, just as it would have been in conventional analysis of covariance. Mathematically, however, it is merely an instance of an interaction effect between X and T. The notion of "intensifier effects" is also an instance of an interactive effect, but in this case there is a non-zero cross-product statistic in both categories of T. For the intensifier effect, one of these statistics must be considerably larger than the other, although of the same sign. Finally the "suppressor effect" notion is still another instance of an interactive effect. In this case, the two cross-products statistics are of opposite sign and of such relative size and weighting (determined by the number of cases in each category) as to approximately cancel each other out at the zero-order level. This again is a substantively interesting case of interaction, but it has no mathematical distinctiveness.

One virtue of drawing attention to the logical similarities between the Lazarsfeld analysis and the general linear model is that researchers dealing with conventional analysis-of-variance data may—as a result of recalling the isomorphism—be prompted to examine and conceptualize any interaction which appears, using one or more of the descriptive headings just mentioned.

There is no point in overworking the comparison. The elaboration technique developed by Lazarsfeld has been useful in leading to the discovery and labeling of several data patterns which occur fairly often in sociological research and which are theoretically interesting precisely because they indicate the need for a model which takes into account the joint effect of certain combinations of the variables meas-

[33] One of the few treatments accessible to sociologists is: Leo A. Goodman, "On the Multivariate Analysis of Three Dichotomous Variables," *American Journal of Sociology*, LXXI, No. 3 (November, 1965), 290–301.

ured in present-day studies. But, in view of the fact that the logic of the model, that is, the basic assumption about the relation between the data and the mathematics, is the same as that of the general linear model except for the assumed properties of the error term, it seems that it would be more efficient to learn this logic once and then study the various applications, rather than being presented with it several times, each time with the use of a different terminology.

Ecological correlation.—A second topic which can be shown to be a special case of analysis of covariance is that of ecological correlation. An ecological correlation is a correlation between two variables which are measured on aggregates—for example, on census tracts. The difficulty that arises in the use of such correlations (or in the corresponding regression coefficients) can be expressed as follows: the investigator often attempts to use a statistic which actually measures the "between" relationship to serve as an indicator of the "within" relationship. Such use is in general unwarranted, since the two components are in general independently determined by the data. Yet, in the literature, there is no lack of such correlations on aggregated data being used as indicators of individual relationships.

The tendency to make this error probably arises because the variable of location, for example (which census tract the respondent is in), is not thought of as a possible causative factor. When it is said that location may be a causative factor, this should be understood to mean that location may be highly (even perfectly) correlated with, and thus serve as an indicator of, other unmeasured variables which do exert a causal influence. Once the researcher is made aware of this possibility, he should then have no trouble in avoiding the mistake of assuming that the "between" relationship is a substitute for the "within" relationship.

Leo Goodman has suggested a procedure by which ecological relationships can, under some special assumptions, be made to serve as unbiased indicators of the individual relationships.[34] The procedure recommended by Goodman does in fact derive from a conceptualization based on the linear model. It is our intent here merely to review, with somewhat different emphasis from the original, how the linear model logic is involved in his discussion.

The example which Goodman uses (the same one used by Robinson in his pioneering paper[35] on ecological correlation) supposes that there are two variables which, at the individual level, are dichotomies: white–non-white and literate-illiterate. Let X denote the white–non-white dichotomy, and let Y denote the literate-illiterate dichotomy. Suppose that marginal frequencies are available on each of these dichotomies for each of k census tracts and for the total population in a city. An equation can then be set up stating whether a given individual is literate or illiterate, on the assumption that some numerical values, say 1 and 0, are assigned to the levels of X and Y:

$$Y_{ji} = b_0 + b_{1j}(X_{ji} - \bar{X}_{..}) + t_j + e_{ji} \,. \quad (13)$$

In this equation, Y_{ji} is 1 if the ith person in the jth tract is literate, and is 0 if he is not. Also, X_{ji} is 1 if this person is white and is 0 if he is non-white. The equation allows for a possible effect of the tract in which the person lives, namely, t_j. It also allows for the possibility of unequal relations between race and literacy in each tract by its provision for distinct b_{1j}.

The question now is to determine what assumptions must be made in order that this equation can be used in the example described to estimate individual relationships. The necessary restrictions can be phrased in various ways. If each tract can be assumed to be a *random sample* from

[34] Leo A. Goodman, "Some Alternatives to Ecological Correlation," *American Journal of Sociology*, LXIV (1959), 610–25.

[35] W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, XV (1950), 351–57.

the *same population* of tracts, then the estimates of the relation can be pooled to give an unbiased over-all estimate. The random sample condition is equivalent to saying that there is no relationship between tract and literacy except that which arises from the variation in proportion white. In other words, all $t_j = 0$. The same population condition is equivalent to saying that the relation between race and literacy in each tract must be the same, that is, all $b_{1j} = b_1$.

If both of these assumptions are met, equation (13) can be rewritten:

$$Y_{ji} = b_0 + b_1(X_{ji} - \bar{X}_{..}) + e_{ji} \,. \quad (14)$$

This leads to the following equation for the tract marginals:

$$\bar{Y}_{j.} = b_0 + b_1(\bar{X}_{j.} - \bar{X}_{..}) \,. \quad (15)$$

The parameters in equation (15) are the same as those in equation (14), and so if the parameters for (15) can be estimated, they can be plugged back into (14). Then, by the use of these parameters, the cell entries of the 2×2 table for the total city can be generated and a measure of association computed. Goodman shows that, with the given assumptions, the parameters are estimable, and he suggests some shortcuts in computation.

The next practical question is to find methods by which it can be determined whether the assumptions of no interaction (equal relationships within tracts) and of no effect of tract per se on literacy are reasonable. Goodman suggests that if the percentages of X and Y for each tract are plotted on a scattergraph, and they show a trend toward linearity, there is probably no sizable interactive effect. In other words, the relation within each tract is probably the same except for some random fluctuation. A second condition suggested in the article is that Y must be much more heavily dependent on X (race) than on A (tract). This amounts to saying that the fit of the points in the scattergraph should be fairly close to a single straight line—in other words, that the t_j should be close to zero. Consequently, the deviations of the estimated tract averages on Y (where knowledge of X is assumed in making the estimate) from the observed tract averages should all be small. If they are not, the estimates for the individual relationship are imprecise though unbiased. Goodman suggests this criterion in his remark that the over-all average on Y, which is known, should be close to the estimated over-all average.

Goodman notes that such conditions in actual research would be "very special." This again illustrates the role of restrictive assumptions in data analysis. They can frequently provide justification for inferences which are otherwise unobtainable, but, to attach confidence to the results, one must have substantial evidence that the assumptions themselves are appropriate.

In this discussion of Goodman's paper, several incidental points have not been treated. The objective was to indicate that here, as in other instances, the analysis-of-covariance model slightly extended can easily serve as a framework within which to organize discussion of a problem. Both for an exposition of the basic fallacy in the use of ecological correlations and for the presentation of a technique which circumvents the limitations of ecological correlation under certain special circumstances, the formulation in terms of the analysis-of-covariance–linear-model logic permits a coherent and compact treatment.

Compositional effects.—Another topic which can be subsumed under the same mantle is that of compositional effects.[36] In

[36] See P. Blau, "Structural Effects," *American Sociological Review*, XXV, No. 2 (April, 1960), 178–93; J. A. Davis *et al.*, "A Technique for Analyzing the Effects of Group Composition," *American Sociological Review*, XXVI, No. 2 (April, 1961), 215–25; J. S. Coleman, "Relational Analysis: The Study of Social Organizations with Survey Methods," *Human Organization*, XVII, No. 4 (1958), 28–36; P. F. Lazarsfeld and A. H. Barton, "Qualitative Measurement in the Social Sciences: Classification, Typologies, and Indices," in D. Lerner and H. Lasswell (eds.), *The Policy Sciences* (Stanford, Calif.: Stanford University Press, 1951), esp. pp. 187–91.

this case there is observation of individual scores on some explanatory variable X, and, likewise, observation of individual values of some dependent variable, Y. In addition, each individual is classified as a member of one of a series of aggregates—here, usually structured sociological groups. The compositional effects are the effects due directly or indirectly to some characteristic of this group. The unique aspect here is that group membership is not used simply as an indicator of some unknown characteristic of the group, as was done earlier in the ecological correlation situation. Instead, the group variable that is used is some statistic from the distribution of observations on X for that group. This parameter is usually the mean, occasionally the variance or some other statistic. Naturally, it is equally possible to use a statistic relating to some other variable, Z, to indicate the "compositional" effect, as long as the statistic which is used characterizes the group as a totality. Davis *et al.*, mention in their article the parallel between analysis of covariance and compositional effects. They also point out that the same abstract problem has been treated negatively as the fallacy of ecological correlation. My purpose is simply to emphasize this parallel and suggest that use of it might be advisable in order to present an integrated treatment of these several topics.

Blau's article, which appeared before the one by Davis *et al.*, does not mention the parallel with analysis of covariance and is in general much more concretely oriented, considering only attitudinal explanatory variables. Thus, the difference in terminology between Blau's "structural effects" and Davis *et al.*'s "compositional effects" indicates a wider scope of applicability for the latter. Davis *et al.* are also more explicit and systematic in discussing the several distinct configurations of compositional effects which correspond more or less closely to the data configurations discussed earlier in connection with the Lazarsfeld elaboration technique. This seems to indicate something about the methodological gains in scope and coherence which can be achieved by conceptualization in terms of a covariance type of model.

Direct and indirect standardization.—Another technique which can be conceptualized in terms of the ideas of the linear model is that of standardization.[87] Suppose the researcher wishes to assign some index value to the fertilities of several nations, so that they may be compared. Let $\bar{Y}_{k.}$ be the measure of fertility for each of K nations. It is known that several variables, notably age, affect the fertility level of an individual woman. Letting Y_{ki} denote the individual fertility of the ith woman in the kth nation (which is assumed to be known), we might consider a linear model, in which u denotes some additive constant, f_k denotes the slope of the relation between fertility and age in the kth nation, and n_k denotes the influence of the kth nation. The equation for an individual woman's fertility could then be written:

$$Y_{ki} = u + f_k(a_{ki} - \bar{a}_{..}) + n_k + e_{ki}\,, \quad (16)$$

where a_{ki} denotes the age of the woman, and $\bar{a}_{..}$ denotes the grand average of all the ages.

This equation is formally identical to equation (13), which was the basic equation for the ecological correlation situation in terms of the linear model. However, an objection can be raised immediately to equation (16) on the grounds that the relation between age and fertility is not properly represented by a straight line. Fertility typically is low among the very young and very old, and highest among those approximately 15–30 years of age. In order to make the model fit more closely, various non-linear functions (say, the square) of age might be used. Another possibility would be to partition the ages into a set of discrete categories, each of which would

[87] For a discussion of standardization see G. W. Barclay, *Techniques of Population Analysis* (New York: John Wiley & Sons, 1958), pp. 161–66; M. Rosenberg, "Test Factor Standardization as a Method of Interpretation," *Social Forces*, XLI, No. 4 (1962), 53–61.

have its own distinct influence on fertility. This latter method is the one adopted in practice in this sort of situation. With some further changes in subscripts, equation (16) can be reformulated:

$$Y_{kji} = u^* + f_{kj}(+1) + n_k + e_{kji}, \qquad (17)$$

where u^* is a new additive constant, and where age categories are denoted by subscripts j. This equation is similar to the initial formulation of analysis of variance with one major exception: the main effect of age is deliberately left unseparated from the possible interactive effect between age and nation. Note that at this stage the model does allow for a possible interactive effect between age and nation, and also allows for the possibility of differing distributions of ages from one nation to the next. Thus, the equation for the kth nation is:

$$\bar{Y}_{k..} = u^* + (f_{k1})(p_{k1}) + \ldots + (f_{kJ})(p_{kJ}) + n_k, \qquad (18)$$

where p_{kj} is the proportion in the kth nation who are in the jth age category.

If there is reason to do so, the assumption can be made that $f_{1j} = f_{2j} = \ldots = f_{Kj} = f_j$ for all values of j. In other words, a single set of effect coefficients (analogous to a single slope) can be applied to all the nations. If this is done, the resulting value for $\bar{Y}_{k..}$ rests on the assumption of non-interactive effects of age, but it does allow the shape of the age distribution in a nation to be separated from the main effect of the nation in determining its fertility. Thus, using the single set of f_j, one could call the obtained n_k the indexes of intrinsic fertility for each nation, independent of age distribution.

This technique, in which a "standard" set of age-specific fertility rates (effect coefficients) is applied to the age distributions of every nation, is called *indirect* standardization. Note that it is applicable even if the age-specific fertility rates for one or more nations are unknown.

If, on the contrary, it cannot be assumed that all of the $f_{kj} = f_j$, then substantively this interactive effect, which is in the present formulation confounded with the effect of differing age distributions, ought to be noted and available for separation or inclusion in the summary measure of the effect of nation on fertility. As with any interactive effect, the decision is somewhat arbitrary. To unconfound the effects of age distribution and the age-nation interactive influence, a single age distribution is used for all the nations. Thus, in this case, the equivalent assumption is that $p_{kj} = p_j$ for all values of k. The distinct sets of f_{kj} are applied to a single "standard" age distribution. This technique is called *direct* standardization.

Note that direct standardization can be applied if the actual age distributions in each nation are unknown, but that it requires knowledge of the age-specific fertility rates for each nation. Direct standardization permits the determination of an interactive effect, if there is one, but obscures the effect of differing age distributions. Indirect standardization, conversely, obscures any possible interactive effects but permits the determination of the effect of differing age distributions. Both of these techniques thus are optimum only if some knowledge is missing. When knowledge is complete (i.e., the age-specific fertility rates and the age distribution both are known), the ideal procedure would be to obtain separate components due to the main effect of nation, the interactive effect of age and nation, *and* the effect of differing age distributions.

This discussion illustrates how an alternative formulation of the two-factor categoric explanatory variable model can be used to present direct and indirect standardization. It should be noted that, before a numerical solution could be obtained, there would have to be a reparametrization of this model, as was the case with analysis of variance. Two useful points are brought out incidentally by this discussion: (1) if the relation between two continuous variables, X and Y, is non-linear, one useful strategy is to categorize X and estimate a

set of distinct coefficients; (2) both methods of standardization lose some potential information, and perhaps the ideal method, if all of the explanatory variables are known, would be to conceptualize the fertility of a nation as a vector quantity, not fully expressible in a single number.

At least with the present sort of approach, the user is alerted to the limitations of any scalar measure of national fertility.

Finally, it is worth mentioning that, although the example and the terminology of direct and indirect standardization were taken from demography, there is nothing to prevent the same logic from applying in any context where a single index is to be constructed representing an aggregate quantity.

Percentaging of cross-tables.—In every situation in which there is an explicit or implicit comparison between two or more sets of data with respect to the strength of some relationship within the sets, the same sort of logic is applicable. Hans Zeisel's discussion of the rationale of percentaging cross-tables,[38] and his proposed rules for determining the appropriate direction in which to calculate percentages, can be understood as another context in which the logic of the linear model is relevant. The major reason for percentaging a table is to make it (either explicitly or implicitly) more easily comparable with some other table. In fact, even when there is only one table at hand, the reason for percentaging it is so that the reader can relate it more readily to tables of similar form that he may have encountered in other contexts.

If an explanatory variable (measured by a dichotomy) is denoted X, and a dependent variable (also measured by a dichotomy) is denoted Y, and if there are two tables (perhaps, for example, one for males and one for females), labeled Table A and Table B, Zeisel's rule for percentaging is as follows:

TABLE A

		X +	X −
Y	+	p_A	q_A
	−	$1-p_A$	$1-q_A$
		100%	100%

TABLE B

		X +	X −
Y	+	p_B	q_B
	−	$1-p_B$	$1-q_B$
		100%	100%

The procedure being applied here is actually a special case of direct standardization. Equivalently, we have assumed a standard population for each table of 200 observations, with half (100 observations) in each of the two categories of X. Like any other direct standardization, this procedure makes the implicit assumption that any observed differences in the marginal distributions of X between Table A and Table B are irrelevant to the purposes at hand. Thus the marginal distribution of X is intentionally excluded in the interests of providing a simpler presentation of the interesting aspects of the data.

Conceptualization of percentaging in terms of a linear model helps to make clear the sense in which Zeisel can say that percentaging is a crude substitute for a multiple correlation analysis.[39] Both percentaging and correlation analysis are ways of statistically "controlling" for the effects of other variables in order to study a relationship between X and Y. In a correlation analysis, however, one controls for other variables by adjusting both X and Y for the possible effects on each of them of all the other variables; in percentaging, one equivalently assumes that all the remaining variables which influence Y are unrelated to X (which is the same as saying that all the variables which affect X are unrelated to Y), and so these other variables will not bias the estimate of the relationship between Y and X.

This approach should also make somewhat clearer Zeisel's caution about an exception to his rule.[40] As he says, if there is to be an inference from a sample to a popu-

[38] Hans Zeisel, *Say It with Figures* (4th ed.; New York: Harper & Row, 1957), pp. 5–41.

[39] *Ibid.*, p. 16.

[40] *Ibid.*, pp. 30–36.

lation, and if the sample Y has a marginal distribution different from its marginal distribution in the population, then the percentages must not be calculated according to the independent variable. The reason for this is that, under these conditions, the process of sampling can be conceptualized as a spurious influence which has an effect not only on the distribution of Y but also on the distribution of X. Thus, there is a single variable related to both X and Y, and so the percentaging assumption is not met.

Other topics involving the linear model.—There is a variety of other techniques in which the linear model is more or less directly implicated. However, rather than further lengthening this paper by going into detail, I shall simply mention a few other areas where the linear model is relevant. Factor analysis, for example, conceptualizes each observed score on one of a large number of variables as being a linear combination of some smaller number of underlying factors.[41] Discriminant analysis, another little known but potentially useful technique in sociological research, likewise involves the basic idea of the linear model.[42] In this technique, the objective is to find the linear combination of a number of (predictor) variables which will maximize the distances between two or more subsets of a batch of data and simultaneously minimize the variance within subsets. Coleman's technique for the multivariate analysis of attribute data[43] (i.e., where the dependent variable is a dichotomy) employs a novel set of parameters, the transition rates of a continuous time, discrete state stochastic process. But, having proposed these parameters as the focus of interest, Coleman's suggested method of estimating them is once again to assume them to be linear combinations of a number of components,[44] each component deriving from one of the explanatory variables.

V. CONCLUSION

Many research situations, although requiring different specific computational techniques, nevertheless share some basic logical characteristics. The class of such situations can be described as that in which several variables (weighted by some coefficients) are used simultaneously to estimate a single variable.

The utilization of the linear model rests on several assumptions about the characteristics of the processes which generate the data. In terms of the scientific logic, these requirements may be phrased as follows:

1. The relationship between any explanatory variable, X_k, and the dependent variable, Y, does not depend on the values of any other explanatory variable or set of explanatory variables.

2. Either (*a*) all pertinent explanatory variables have been included in the model or (*b*) none of the pertinent variables which is not included in the model bears any relationship (any association) with any of the explanatory variables which are included in the model.

3. None of the explanatory variables can be predicted exactly (or even almost exactly) by using any linear combination of the other explanatory variables.

4. Data are available on a sufficient numbers of cases, where "sufficient" has to be defined in terms of the particular situation.

In the scientific context, a person interested in studying some particular question must undertake a mental analysis of several factors simultaneously in order to design research which will validly and efficiently

[41] See H. H. Harman, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1960), p. 13.

[42] See J. G. Bryan, "The Generalized Discriminant Function: Mathematical Foundation and Computational Routine," *Harvard Educational Review*, XXI, No. 2 (Spring, 1951), 90 ff.

[43] J. S. Coleman, *Introduction to Mathematical Sociology* (Glencoe, Ill.: Free Press, 1964), chap. vi.

[44] For a discussion of the optimum weighting procedure, see R. P. Boyle, "Causal Theory and Statistical Measures of Effect: A Convergence," *American Sociological Review*, XXXI, No. 6 (December, 1966), 843–51.

address the question. The design problem requires that a model be chosen (1) which is theoretically appropriate, (2) for which data can be obtained, and (3) whose statistical structure will permit analysis to produce interpretable results. The label "operational definition of variables" actually obscures the complexity of much of this process by treating it as a unitary task. In fact, the various possible operational definitions of a single variable quite regularly have consequences both for the details of the theoretical appropriateness of the model and for the statistical properties which can be assumed.

This sort of activity is a central but frequently overlooked portion of research design. Unfortunately, it is also one about which it is difficult to generalize. Thus, while recognizing the interdependent character of the decisions, let us focus on one point in the process at which a set of operationally defined variables has been tentatively adopted. Let us also agree that the theoretical relationships among these variables are at least implicitly understood, that is, there is the material for a theoretical model at hand.

In this situation, the statistical and theoretical properties of the model intertwine and can, to some extent, be treated as one. The point is that, if the statistical requirements do not match the theoretical ones, at least the efficiency, and possibly the validity, of the design will be lost. Several questions can be asked about the design, each deriving from one of the assumptions of the linear model.

The first question to be answered, prior to any computations, concerns the postulated *causal sequencing* among the explanatory variables. Any given pair of explanatory variables may be: (*a*) causally unrelated (though perhaps correlated in the sample); (*b*) directly related in one of the two possible directions; or (*c*) indirectly related, again in one of the two possible directions. The matter of setting up such sequences and then dealing with them has only recently begun to receive attention in sociology,[45] although there is a literature from other disciplines, notably economics and biology.[46]

Usually there will be several plausible structures among the many possible arrangements of the explanatory variables. There will also be some arrangements which can be ruled out a priori. Even with the use of electronic computers, something simpler than an exhaustive testing of every remaining possibility is needed. At this point, the investigator's judgment, based on his familiarity with the previous evidence and relevant theory, is the final guide. Several models may have to be tried, with the hope that one(or, at worst, a few) of them will prove to fit the data more accurately than the rest.

The next question involves the sort of inference that is intended. Are the conclusions to be used merely as descriptive of the observed data and other situations exactly like the observed data (fixed effects)? Alternatively, the observations may be regarded as a sample from which inferences are to be made to a population (random effects). Naturally, in a given study, it is possible that some of the variables will be regarded as samples, while others will be considered as exhaustive of the possibilities. This sort of question has been discussed by Campbell and Stanley as the internal versus the external validity of a piece of research.[47]

A third question—the first which depends on the character of the data—is:

[45] See, for instance, H. M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964); also, O. D. Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, LXXII, No. 1 (July, 1966), 1–16.

[46] In economics, any of the econometrics texts will provide an example of this sort of approach. For example, Johnston, *op. cit.* In biology, Sewall Wright has pioneered this sort of approach. His writings are somewhat scattered, but one recent example is his "The Interpretation of Multivariate Systems" in O. Kempthorne (ed.), *Statistics and Mathematics in Biology* (New York: Hafner Publishing Co., 1954).

[47] D. T. Campbell and J. C. Stanley, "Experimental and Quasi-experimental Designs for Re-

what type of measure has been used on the dependent variable, Y? If Y is continuous, all of the techniques of the general linear model are potentially available. If Y is categoric, the situation is more complicated, but many situations can be handled by techniques such as Lazarsfeld's elaboration, or Coleman's multivariate analysis of attribute data, or discriminant analysis.

The next question is whether it is appropriate to make the assumption, basic to the linear model, that relations between the dependent variable and the explanatory variable are (*a*) linear in the parameters and (*b*) additive, that is, no interactive influences between explanatory variables. These assumptions may have to be tested from the data. Thus the first actual use of the data is likely to be a test that the pattern is consistent with an additive model. This test basically involves examining the conditional relationships between Y and X_k for various levels of X_j. If the conditional relationships are equal for all values of X_j, then X_k and X_j can be said to be non-interactive in their effects.

If, for some pairs of variables, the conditional relationships are not all equal, a compromise must be made. Sometimes a new explanatory variable can be defined which will account for the interactive effect *and* which also has substantive meaning. In other cases, although the mathematics will still permit the separation of the interactive component, the substantive theory cannot accommodate it. In this case, the interpretation of the numerical output is severely restricted.

It should be noted that, although the discussion has been phrased in terms of pairs of variables, this would be only the simplest possible interaction effect. One must also consider the possibility of interactive mechanisms involving three, four, or more variables, depending upon the total number of explanatory variables in the analysis. It should also be mentioned that, if the explanatory variables are continuous, there is an infinite number of ways in which the relation between Y and X_k can depart from linearity. If non-linearity is suspected, either some functional forms may be tried or the explanatory variable may be partitioned into a series of categories.

Another question concerns the handling of non-orthogonality among the explanatory variables. In practice, orthogonality is usually found only by coincidence or by deliberate manipulation of the choice of observations. In experimental situations, orthogonality can be and usually is guaranteed; in observational studies, it is seldom obtained. An operational definition of non-orthogonality might be that use of some linear function of X_j improves (over chance) the prediction of X_k. Depending upon the specific causal model postulated, and upon the degree of non-orthogonality (also referred to as "confoundedness" or "multicollinearity," depending upon the context), different conclusions are made and different procedures recommended.

VI. SUMMARY

This last section has recapitulated some of the recurring ideas discussed in the treatment of specific techniques. Although the ideas have been presented in a very schematic fashion, it is hoped that the logical similarities among the many techniques have been made somewhat clearer. Aside from the aesthetic and pedagogical justification for appreciating these similarities, there is also the consideration that the same mathematical computations are used in many of these techniques. For this reason, a relatively few generalized computer routines can usually be combined in various ways to provide a variety of usable programs.[48]

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search on Teaching," in N. L. Gage (ed.), *Handbook of Research on Teaching* (Chicago: Rand McNally & Co., 1963), pp. 175 *passim*.

[48] See A. G. Beaton, *The Use of Special Matrix Operators in Statistical Calculations* (Princeton, N.J.: Educational Testing Service, Research Bulletin RB-64-51, 1964).

Intersociety and Intrasociety Correlations of Occupational Prestige[1]

J. Michael Armer

ABSTRACT

The possibility that recent evidence of international similarity in occupational prestige may be a spurious result of oversampling of students and other "modern" respondents in non-industrialized society studies is discussed and examined with survey data from Hausaland, a traditional, non-industrial society in northern Nigeria. Correlations between NORC prestige evaluations and evaluations of "modern" and "traditional" Nigerian respondents fail to support the alternative methodological explanation and provide additional support for the thesis of international occupational prestige uniformity. The need for comparisons among respondents from communities varying in size and complexity in non-industrial societies is indicated as a further test of the possibility of spuriously high international correlations, and the need for theoretical efforts to account for intersocietal uniformity in occupational prestige structures is emphasized.

In 1956, Inkeles and Rossi reported an extremely high correspondence between the occupational prestige structures of Germany, Great Britain, Japan, New Zealand, the U.S.S.R., and the United States —the six industrial nations for which comparative data were then available.[2] The authors suggested that the high correlations might reflect "a relatively invariable hierarchy of prestige associated with the industrial system," despite differences in their sociocultural systems.[3] In a subsequent article on "Industrial Man," Inkeles cites the earlier findings and argues that "there are definitely some common values about the occupational realm shared not only within particular countries but within all modern, large-scale, more or less industrial societies."[4] However, more recent studies of occupational prestige ratings in non-industrial societies have found evidence of correlations with industrial society ratings equally as high as those reported previously by Inkeles and Rossi.

The question raised and partially examined in this paper is whether the similarities in ratings might be due to overrepresentation of students or "industrial men" in the non-industrial society studies rather than to the existence of common occupational prestige structures across all societies. A test of this alternative methodological explanation is made by comparing prestige evaluations of separate "modern" and "traditional" samples of respondents in the highly traditional, non-industrial Hausa society of northern Nigeria.

PREVIOUS EVIDENCE

In a recent review of cross-cultural occupational prestige research, Hodge, Treiman, and Rossi present consistently high coefficients of determination (r^2) ranging from a low of 0.62 to a high of 0.95 between the 1963 NORC evaluations for the United States and evaluations in twenty-

[1] This research is part of a larger study of value transition in a developing society supported by the Foreign Area Fellowship Program and the National Science Foundation. Appreciation is due to Robert Youtz for assistance in data analysis and to Gerald King and Robert A. Ellis for comments on a previous draft.

[2] Alex Inkeles and Peter H. Rossi, "National Comparisons of Occupational Prestige," *American Journal of Sociology*, LXI (January, 1956), 329–39.

[3] *Ibid.*, p. 339.

[4] Alex Inkeles, "Industrial Man: The Relation of Status to Experience, Perception, and Value," *American Journal of Sociology*, LXVI (July, 1960), 1–31.

three other industrial and non-industrial nations.[5] The mean coefficient of determination for countries with modernized, industrial structures (indicated by Gross National Product per capita of $450 or more) is 0.84, while for less economically developed countries (all having GNP levels of less than $300) it is 0.82.[6] The lack of any apparent association between magnitude of the correlations and level of industrialization led Hodge and his associates to conclude that "the data so far suggest that occupations stand roughly in the same order of popular evaluation across a wide variety of nations of varying levels of industrialization and varying cultural backgrounds. This new evidence requires an explanation which stresses social-structural features that all societies, whether industrialized or not, have in common."[7]

While suggesting this thesis of a universal occupational prestige structure, Hodge *et al.* are careful to note the possible alternative explanation of sampling bias. They state that "because the samples for the underdeveloped countries often consist mainly of students in Western-type schools . . . the correlations are high relative to those which would obtain had prestige scores been derived from a sample of the general population."[8]

Among studies which have not relied exclusively on student samples, Tiryakian's investigation of occupational prestige evaluations of Filipinos in a suburb of Manila and four rural communities is one of the earliest.[9] However, Inkeles argues that Tiryakian's high correlations indicate that evaluations common to industrial nations "may also be shared in countries not so highly industrialized but already incorporated into or influenced by currents of modernization."[10] Thus, it is questionable whether they may be considered as evidence of a universal occupational prestige thesis. Presumably, this objection would also be raised against earlier evidence based on a stratified probability sample of males age twenty to twenty-nine in six large cities in Japan.[11] Also, a study conducted with a probability sample of 260 residents of Santiago, Chile, in 1963 may fail to provide an adequate case, "inasmuch as Chile is a country in a stage of industrial development intermediate between Indonesia . . . and the nations in which the studies discussed by Inkeles and Rossi were conducted. . . . It should also be noted that Chile is industrializing rapidly and is indeed the kind of 'centralized national state' that Inkeles and Rossi describe."[12]

In each of these few studies in which non-student samples were included, the reported occupation evaluations agree closely

[5] Robert W. Hodge, Donald J. Treiman, and Peter H. Rossi, "A Comparative Study of Occupational Prestige," in Reinhard Bendix and Seymour Martin Lipset (eds.), *Class, Status and Power* (2d ed.; New York: Free Press, 1966), pp. 309–21. The authors recomputed all correlations using 1963 NORC occupational evaluations for the United States. The twenty-three countries and corresponding coefficients of determination were as follows: Australia (.89), Belgian Congo* (.63), Brazil* (.76), Canada (.93), Chile* (.84), Denmark (.86), Germany (.81), Ghana* (.83), Great Britain (.83), Guam (.90), India* (.75), Indonesia* (.81), Ivory Coast* (.83), Japan* (.84), Netherlands (.86), New Zealand (.95), N. Rhodesia* (.86), Norway (.90), Poland (.62), Philippines* (.90), Sweden (.74), Turkey* (.91), U.S.S.R. (.79). Countries included in the "less developed" category are asterisked.

[6] Hodge *et al.*, *op. cit.*, p. 320. The authors note that, in addition to lower economic development, the non-industrial nations all retain important non-Western, indigenous populations and cultures.

[7] Hodge *et al.*, *op. cit.*, p. 310.

[8] *Ibid.*, p. 320.

[9] Edward A. Tiryakian, "The Prestige Evaluation of Occupations in an Under-Developed Country: The Philippines," *American Journal of Sociology*, LXIII (January, 1958), 390–99.

[10] Inkeles, *op. cit.*, p. 8.

[11] Research Committee, Japan Sociological Society, "Social Stratification and Mobility in Six Large Cities of Japan," in *Transactions of the Second World Congress of Sociology* (London: International Sociological Association, 1954), II, 414–31.

[12] Roy E. Carter, Jr., and Orlando Sepulveda, "Occupational Prestige in Santiago de Chile," *American Behavioral Scientist*, VIII (September, 1964), 20–24, esp. 24.

with the NORC results, but this may be due primarily to the intermediate status of the countries in terms of modernization or industrialization. Evidence from more traditional and undeveloped societies is needed for a more rigorous test of the thesis that occupational prestige evaluations are shared cross-culturally, regardless of industrial level. Indeed, in India, which clearly fits the traditional, non-industrial description, Cook reports evidence of a low correlation (0.21) between occupational evaluations of postgraduate male students and illiterate village men. However, it is impossible to tell from the reported data whether the low correlation results primarily from education, rural-urban residence, or other uncontrolled differences between the two groups.[13]

Although there is very little systematic evidence supporting the proposition that high correlations of previous research may be largely a result of sampling designs favoring educated "modern" or "Westernized" segments of the population in traditional, non-Western societies, neither is there sufficient evidence of high agreement between occupational prestige evaluations of such subpopulations.[14] As evidence of high correlations between occupational evaluations of subpopulations is obtained, greater support may be claimed for the view which postulates international uniformity in occupational evaluations. To the extent that further evidence from subpopulations indicates divergencies from standard occupational evaluations, then interpretations postulating the existence of competing occupational stratification hierarchies, at least in early periods of industrialization and exposure to modern, "Industrial Man" ideas, must be further entertained, refined, and investigated. The present study examines evidence bearing on this problem by comparing occupational prestige evaluations of selected subpopulations of youth within the ancient mud-walled city of Kano in northern Nigeria.

RESEARCH SETTING AND SURVEY

Hausa society in northern Nigeria is a particularly appropriate setting for the present investigation, since it is well known as an African society in which the traditional culture and sociopolitical organization were largely preserved throughout the colonial period.[15] It was here that the British inaugurated their policy of "indirect rule" and conscientiously endeavored to minimize changes in the established order by restricting Western education, Christian mission activity, and European settlement throughout most of the colonial period. Although highly complex in its own right, Hausa society has remained relatively undeveloped and "traditionalistic."[16] In short, it represents a society where traditional evaluations and standards may be expected to continue to have a strong influence on the assessment of occupations and therefore to accentuate any differences in the prestige evaluations of more or less educated and "modern" segments of the population.

In the spring of 1965, interviews were conducted with a sample of 591 boys,

[13] David R. Cook, "Prestige of Occupations in India," *Psychological Studies* (Mysore, India), VII (January, 1962), 31–37.

[14] For industrial societies, there is considerable evidence of high consensus on the occupational prestige evaluations of persons in different social positions. See, e.g., the analysis of North-Hatt data on this point by Albert J. Reiss, Jr., *Occupations and Social Status* (Glencoe, Ill.: Free Press, 1961), pp. 162–90.

[15] Although there are many ethnic groups and cultural influences in Hausaland, the single adjective "Hausa" is used for the sake of convention and convenience when referring to the society, people, or occupations in this study.

[16] "Traditionalistic" is used here in the sense described by Hoselitz, in which the society purposefully seeks to maintain as much of the traditional way of life and values as possible while not necessarily forsaking social and economic benefits of selective modernization. See Bert F. Hoselitz, "Tradition and Economic Growth," in Ralph Braibanti and Joseph J. Spengler (eds.), *Tradition, Values and Socio-Economic Development* (Durham, N.C.: Duke University Press, 1961), pp. 83–113.

which comprised all seventeen-year-old boys who could be located in a 16 per cent probability area sample of the 128 wards of the city. Interviews were conducted in the Hausa language during the school holiday period at the homes of respondents by local secondary-school boys of slightly older age.

Information on a variety of matters, including background characteristics of respondents and their evaluations of the prestige of selected occupations, was obtained through structured interview questions. A list of sixteen occupations was constructed to include examples of major occupational categories, prestige levels, and traditional as well as modern titles. The rating procedure consisted of showing respondents a card on which a picture of a hill had been drawn with five levels marked off corresponding to higher and lower "social positions of all the people in this city." Occupations were called off by the interviewer and respondents were asked to "point to its position on the hill." The ratings accorded each occupation on the five-point scale were then averaged to give a prestige score between 1.00 and 5.00.

TABLE 1

OCCUPATIONAL PRESTIGE RATING PERCENTAGES, MEANS, AND STANDARD DEVIATIONS FOR TOTAL SAMPLE OF KANO BOYS, 1964

Occupations	Prestige Rating						N	Mean	S.D.
	1 (High)	2	3	4	5 (Low)	No Answer			
Government and native-authority officials	72	16	6	1	3	1	590	1.47	0.92
Doctors	43	30	18	5	2	0	591	1.93	1.02
Lawyers	38	34	16	7	3	0	591	2.03	1.08
Mallams	34	23	18	15	8	1	590	2.40	1.31
Office clerks	12	27	35	19	4	0	591	2.78	1.05
Primary-school teachers	14	23	35	20	5	1	590	2.80	1.10
Store clerks	12	23	38	20	4	1	590	2.81	1.04
Market traders	11	20	27	26	14	1	590	3.12	1.20
Factory workers	7	17	27	34	13	2	589	3.29	1.13
Building contractors	7	15	30	30	16	1	590	3.34	1.14
Policemen	6	13	29	35	14	5	586	3.39	1.11
Tailors	6	12	28	32	20	0	591	3.46	1.15
Farm owners	10	15	17	21	34	0	591	3.54	1.38
Butchers	3	7	16	28	43	3	588	4.01	1.11
Leatherworkers	5	5	11	28	49	1	590	4.10	1.15
Praise-singers and drummers	2	5	8	25	56	7	584	4.31	1.00
Mean	......	......	......	......	......	......	......	3.05	1.12

RESULTS

The prestige hierarchy.—Prestige ratings of the total sample of Kano boys are reported in Table 1, which will be discussed before the degree of agreement between occupational evaluations for different subpopulations is investigated. Occupational prestige scores range from a high of 1.47 for government and native-authority officials to a low of 4.31 for praise-singers and drummers, whom the Hausa frequently consider akin to beggars, except for those who are more or less permanently attached to a government official or other important person. The mean prestige score of 3.05 is very close to the midpoint of the prestige scale and divides the occupations into "white collar" and "blue collar" categories. Indeed, a principal

feature of the prestige hierarchy is the clear tendency for administrative and professional occupations to be ranked highest, followed by clerical, skilled, and semiskilled occupations, in accord with occupational hierarchies in industrial societies. At the bottom of the hierarchy are the culturally "unclean" and "despised" occupations of butcher, leatherworker, and praise-singer, which follow the field analysis of Hausa stratification by Smith.[17]

The extent to which the hierarchy actually agrees with urban-industrial structures can be suggested by rank-order correlation with a list of similar occupational titles drawn from the NORC data. As Haller and Lewis point out, such correlations are affected by the tendency to eliminate from comparison those occupations without clearly translatable titles (i.e., "those . . . which can be expressed in the language and terminology of each of the societies under comparison").[18] In the present study, all sixteen occupational titles are retained in the comparisons, including the non-Western occupational titles of mallam (religious leader and Islamic scholar), market trader, and praise-singer. In fact, there is no case of a perfect correspondence between an occupational title in this study and in the NORC data, but in all cases, an attempt has been made to select the best comparison category available from the NORC list of occupations. These are listed in Table 2, along with Hausa titles, rank orders, and prestige scores (based on the

TABLE 2

PRESTIGE RANK ORDER AND SCORE FOR HAUSA OCCUPATIONS AND SELECTED NORC EQUIVALENTS

Hausa			NORC		
Rank	Score	Occupations	Rank	Score	Occupations*
1	91	Government and native-authority officials	4	74	Officials and administration, state and local
2	82	Doctors	1.5	89	Physicians and surgeons
3	79	Lawyers	1.5	89	Lawyers and judges
4	72	Mallams	5	71	Clergymen
5	65	Office clerks	7	69	Clerical n.e.c.
6	64	Primary-school teachers	3	78	Teachers n.e.c.
7	64	Store clerks	9.5	67	Retail salesmen
8	58	Market traders	9.5	67	Retail salesmen
9	54	Factory workers	13	55	Laborers in non-specified, non-durable manufacturing industry
10	53	Building contractors	6	70	Managers—construction business, self-employed
11	52	Policemen	8	68	Policemen and detectives
12	51	Tailors	12	60	Tailors and tailoresses
13	49	Farm owners	14	53	Farm owners and tenants
14	40	Butchers	11	63	Meatcutters (except slaughter plants and packinghouses)
15	38	Leatherworkers	15	51	Craftsmen—shoemakers or laborers in leather products
16	34	Praise-singers and drummers	16	39	Laborers—personal services

* The NORC occupational titles are taken from the list of occupations and prestige scores extrapolated by Otis Dudley Duncan as described in his "A Socioeconomic Index for All Occupations" in Albert J. Reiss, Jr., *Occupations and Social Status* (New York: Free Press, 1961), chap. vi. The list of occupations and prestige scores is presented in Appendix B, pp. 263–75.

[17] M. G. Smith, "The Hausa System of Social Status," *Africa*, XXIX (July, 1959), 239–51, and *Government in Zazzau, 1800–1950* (London: Oxford University Press, 1960), pp. 250–53.

[18] A. O. Haller and David M. Lewis, "The Hypothesis of Intersocietal Similarity in Occupational Prestige Hierarchies," *American Journal of Sociology*, LXXII (September, 1966), 211. Haller and Lewis also discuss several other methodological weaknesses of previous research.

NORC scoring range from 20–100) for each occupation.

Despite the fact that Hausaland represents one of the most traditional societies of Africa, that the respondents represent a cross-section of both students and non-students, and that occupations are far from equivalent in both lists, the rank-order correlation (ρ) between the Hausa and NORC prestige hierarchies is $r_s = .89$, indicating very high agreement. Although rank positions are in general agreement, actual prestige scores are often quite divergent. For example, leatherworkers rank fifteenth on both lists, but have substantially different prestige scores. The degree of agreement between actual prestige scores for the Hausa and NORC occupations is indicated by a Pearson coefficient of correlation of $r = .81$. The coefficient of determination (r^2) equals .65, which would place it among the lower coefficients reported by Hodge *et al.* but still indicative of high agreement with NORC evaluations.[19] In sum, it is apparent that Kano boys assess occupational prestige in much the same way as respondents in the host of other societies reported by Hodge.

Comparison of subsample ratings.—The question has been raised whether the high occupational prestige correlations between non-industrial and modern, industrial societies may be largely the result of biased sampling which overrepresents "modern," educated elements of the populations—the "industrial men" of non-industrial societies. An answer to the question requires fur-

TABLE 3

OCCUPATIONAL PRESTIGE RANK-ORDER COMPARISONS BY LEVEL OF EDUCATION

Hausa Occupations	Respondent Level of Education			NORC Study
	None	Some Primary	Some Secondary	
Government and native-authority officials	1	1	1	4
Lawyers	2	3	2	1.5
Doctors	3	2	3	1.5
Mallams	4	4	7	5
Primary-school teachers	5	7	9	3
Store clerks	6	6	8	9 5
Office clerks	7	5	4	6
Traders	8	9	5	9.5
Factory workers	9	8	11.5	13
Tailors	10	12	13	12
Building contractors	11.5	11	6	7
Policemen	11.5	10	10	8
Farm owners	13	13	11.5	14
Butchers	14	15	15	11
Leatherworkers	15	14	14	15
Praise-singer and drummers	16	16	16	16
(*N*)	(312)	(210)	(69)	....

NOTE.—Intercorrelations (ρ): no education—some primary $r_s = .97$; no education—some secondary $r_s = .89$; some primary—some secondary $r_s = .89$. Correlations with NORC: no education $r_s = .86$; some primary $r_s = .85$; some secondary $r_s = .84$.

[19] It should be pointed out that the r^2 computed above is not perfectly comparable to those computed by Hodge *et al.*, since they used 1963 NORC scores rather than the earlier scores and because they have eliminated farming occupations from all their computations. Actually, if the assumption of interval data is going to be made (or ignored), a more appropriate measure of similarity between the two sets of occupational prestige scores would be Robinson's A. For the data in Table 2, $A = .85$. See William Robinson, "The Geometric Interpretation of Agreement," *American Sociological Review*, XXII (February, 1957), 17–25.

ther evidence as to whether those who are high and low in education (or high and low in "modernity" or "Westernization") share occupational prestige evaluations with each other and with NORC evaluations.

Occupational prestige rank orders between Kano boys who have had (1) no formal education, (2) one to seven years of primary-school education, and (3) one or more years of secondary-school education are compared in Table 3. The three rank-order correlation coefficients between the evaluations of the subsamples are .89, .89, and .97. The highest correlation, of .97, is between the evaluations of those with no education and those with some primary-school education. Boys with some secondary-school education rate mallams substantially lower and market traders and building contractors higher than those with either no education or primary education only. Otherwise, the rank orders are highly similar.

TABLE 4

OCCUPATIONAL PRESTIGE RANK-ORDER COMPARISONS BY TRADITIONAL OR MODERN SELF-IDENTIFICATION

HAUSA OCCUPATIONS	SELF IDENTIFICATION: Traditional	SELF IDENTIFICATION: Modern	NORC STUDY
Government and native-authority officials	1	1	4
Doctors	2	3	1.5
Mallams	3	5	5
Lawyers	4	2	1.5
Primary-school teachers	5	4	3
Store clerks	6	6	9.5
Office clerks	7	7	6
Traders	8	8	9.5
Factory workers	9	10	13
Farm owners	10	12	14
Tailors	11	13	12
Building contractors	12	9	7
Butchers	13	15	11
Policemen	14	11	8
Leatherworkers	15	14	15
Praise-singers and drummers	16	16	16
(*N*)	(172)	(34)	......

NOTE.—Intercorrelation (ρ): traditional—modern $r_s = .94$. Correlations with NORC: traditional $r_s = .80$; modern $r_s = .90$.

Correlations of the subpopulation rank orders with NORC prestige evaluations are also uniformly high ($r_s = .84$–$.86$) although not as high as correlations of the rank orders with each other. The lower correlations with NORC evaluations may be partially attributed to lack of occupational comparability. In sum, youth with varying levels of education perceive basically identical occupational prestige hierarchies which also correspond quite closely with NORC rankings. No evidence is found to support the suggestion that general reliance on student samples in previous research may be responsible for spuriously high correlations.

A test may also be made of the possibility that sampling bias which favors more "modern" segments of the population in underdeveloped societies is responsible for the high occupational prestige correlations with industrial society evaluations. Instead of an objective index of "modern" or "traditional" segments of the population, a more direct, subjective index is provided by responses to the following interview question:

> Some boys have adopted many of the new ways of the Europeans, and some boys have stuck fast to the traditional ways of our forefathers. In thinking about your own feelings, beliefs, and behavior, would you say that you have:
>
> ___ 1. Adopted very much from the new ways of the Europeans
>
> ___ 2. Adopted quite a bit from the new ways of the Europeans
>
> ___ 3. Adopted only a little from the new ways of the Europeans
>
> ___ 4. Stuck fast to the traditional ways of our forefathers.

Respondents selecting the first response alternative are considered subjectively "modern" and those selecting the fourth alternative as subjectively "traditional." Respondents selecting either of the two middle categories are considered subjec-

tively intermediate and are excluded from subsequent analysis.

Comparisons of occupational prestige evaluations of "modern" and "traditional" subsamples are presented in Table 4. The data indicate that boys who are subjectively modern accord virtually identical occupational prestige ratings as those who identify themselves as highly traditional ($r_s = .94$). Where differences in prestige evaluations occur between the two groups, modern boys tend to perceive prestige evaluations closer to the NORC hierarchy than do traditionals. Occupational prestige correlations of modern and traditional boys with the NORC evaluations are $r_s = .90$ and $r_s = .80$, respectively. Although the level of agreement with Western occupational prestige structures is slightly lower for traditional boys, correlations of both rank orders with the NORC evaluations of the sixteen occupations are high and do not support the explanation of sampling bias as an important source of high intersociety correlations in prestige evaluations.[20]

CONCLUSIONS

Occupational prestige evaluations of Kano subsamples differing in educational status and subjective modernity are found to correlate highly with each other and with NORC evaluations. This was found despite the tradition-oriented character of Hausa society and the lack of close comparability in occupational titles. The evidence suggests that previously reported high correlations between industrial society prestige structures and prestige evaluations of samples in traditional, non-industrial societies cannot be attributed to sampling bias favoring students or "Westernized" segments of the population. These findings further weaken the claim that a distinctive and uniform occupational prestige structure is associated with the industrial system or modernization in general. The evidence strengthens the suggestion by Hodge *et al.* that a common occupational prestige system is shared in greater or lesser degree across all societies, whether industrialized or not.

On the other hand, sampling bias has not been completely eliminated as a possible alternative explanation of high intersociety correlations. Haller and Lewis have recently called attention to the tendency of studies in underdeveloped societies to sample exclusively within urban settings, and they argue that high correlations are "probably due to similarities in the evaluation of occupational roles which emerge in dense populations with complex divisions of labor; in short, they may be due to urbanization in general rather than industrialization in particular."[21] Support for this interpretation is suggested by Cook's low correlation between illiterate village men and postgraduate males in India,[22] as well as by Haller and Lewis' evidence of differing levels of correlation between NORC occupational prestige evaluations and the evaluations of their samples differing in urbanization experience.

Since Haller and Lewis had to rely on the proportions of their student samples from non-farm backgrounds as their index of urbanization, the reported correlations may also be explained in terms of farm or non-farm background effects per se, rather than exposure to simple or complex divisions of labor. However, high correlations between prestige rankings of respondents from farm and non-farm families in the present study ($r_s = .96$) and in Tiryakian's study ($r_s = .94$) tend to discount

[20] Intercorrelations (ρ) between occupational prestige hierarchies of all four self-identified subsamples and the NORC data are also consistently high, as is shown in the following matrix.

Subjective Subsamples	Codes	2	3	4	NORC	(N)
Moderns	1	.96	.97	.94	.90	(84)
Intermediate moderns	2	..	.97	.89	.88	(116)
Intermediate traditionals ..	3	..	..	.95	.87	(217)
Traditionals ...	4	..	..	..	.80	(172)

[21] Haller and Lewis, *op. cit.*, p. 212.

[22] Cook, *op. cit.*, p. 34.

this alternative and leave the Haller-Lewis interpretation standing.[28] Further research employing large, representative samples from several communities of varying size and complexity in non-industrial, non-Western societies is needed in order to test this remaining alternative interpretation and to provide a sound basis for evaluating the Hodge thesis of a common occupational prestige structure shared in all societies.

While subsequent research involving more circumscribed samples may find less agreement in occupational prestige evaluations than the present or previous studies, the evidence is clear that a high degree of intersocietal uniformity does exist in the evaluation of occupational prestige by large segments of traditional as well as modern societies. The importance of this finding is more fully recognized when the uniformity is compared to the great variation between societies in the evaluation of other widespread social categories, such as religious, ethnic, and sex categories. As yet, however, no theoretical explanation has been advanced which adequately deals with this unusual cross-cultural uniformity. A major implication of these results is the great need for such a theory based on an analysis of social and social-psychological processes and conditions shared much more widely than just among modern industrial or semi-industrial nations. Moreover, an adequate theoretical interpretation should be able to explain discrepancies in occupational prestige evaluation which appear but tend to have little effect on over-all similarities between prestige structures. In summary, the evidence reported above discounts several major methodological explanations, calls attention to remaining methodological limitations, and emphasizes the need for theoretical efforts to account for the persistent intersocietal uniformity in occupational prestige.

UNIVERSITY OF OREGON

[28] Tiryakian, *op. cit.*, p. 395.

Developmental Sociolinguistics: Inner-City Children[1]

Doris R. Entwisle

ABSTRACT

In spite of the massive efforts aimed at early education of the culturally deprived, little solid information is available to guide these programs. It is assumed that cultural differences are important in language development and in cognitive development, but documentation of these assumptions is surprisingly sparse. Some data presented in this paper suggest, contrary to expectation, that inner-city children are more advanced on certain language measures than suburban children at the time of school entrance.

Linguistic data relate to many problems of sociological theory, such as the discreteness of social stratification and the integration of ethnic groups into the social system, and they are particularly informative vis-à-vis culture transmission and differences in socialization practices. Linguists studying language in its social context have worked mainly on dialect geography, however; and, so far, few data pertain to the interaction of language and social setting. As Hymes[2] points out, little is known about how personal and community beliefs and values impinge on the use of language, especially as this relates to language acquisition by children.

By age four the child has somehow learned most of the structural features of his native tongue, and by age eight he has learned a great deal about substitution properties of words. He knows that "little," for instance, will fill the same slot in a sentence as "big," even though he cannot define the word "adjective." The major development of language and verbal concepts occurs prior to age eight, so formal schooling may have little impact on it. The child's subculture may be the *only* substantial environmental influence because the young child's exposure to language is primarily auditory.

WORD ASSOCIATIONS AS DATA

Obtaining free associations to word stimuli is one way to study language development in young children because these associations are closely related to general linguistic competence and to verbal comprehension. This method has sharp limitations —the sample of language is minute—but associations can be secured from many different groups of children and analyzed by machine, permitting cross-cultural study of verbal concepts over a broad age span.[3]

[1] This work was supported by the National Institute of Child Health and Development, grant HD 00921-05, and was also aided by support from the Center for Study of Social Organization of Schools, Johns Hopkins University. The co-operation of staff and pupils in schools of Baltimore City is gratefully acknowledged. In particular, I am grateful to Dr. Orlando Furno, director of research, and to the following principals: Mr. Edwin Cohen, Mrs. Maria D. Hammond, Mr. Donald T. Leuschner, Mr. Wilfred M. Seaborne, Mr. Fuller C. Strawbridge, Mr. Henry N. West, and Miss Mildred A. Winter. The paper has benefited from comments by James Coleman, Robert Gordon, Edward McDill, and Arthur Stinchcombe.

[2] Dell Hymes, "Models of the Interaction of Language and Social Setting," *Journal of Social Issues*, XXIII (April, 1967), 8-28.

[3] Several atlases aid study of responses: For children in kindergarten, first, third, and fifth grades, see Doris R. Entwisle, *Word Associations of Young Children* (Baltimore: Johns Hopkins Press, 1966); for fourth-, fifth-, and sixth-grade children, see David S. Palermo and James J. Jenkins, *Word Association Norms* (Minneapolis: University of Minnesota Press, 1964). Atlases for other groups are listed in bibliographies of these books.

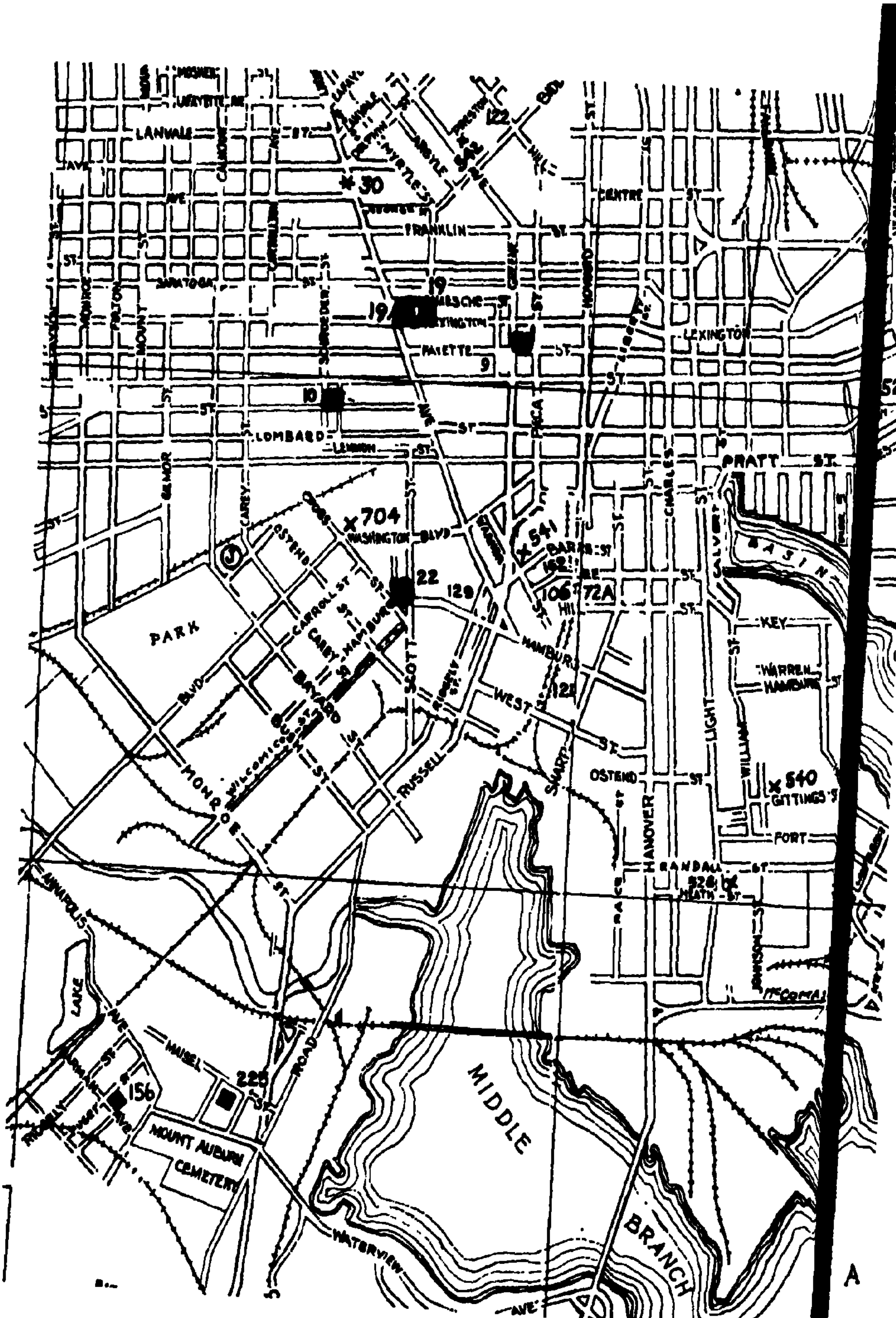

LANVALE
FRANKLIN
CENTRE
SARATOGA
LEXINGTON
FAYETTE
LOMBARD
PRATT ST.
WASHINGTON BLVD
PARK
MONROE
BAYARD
CARROLL ST
OSTEND
HAMBURG
WEST
RUSSELL
SCOTT
PACA
GREENE
HOWARD
LIBERTY
SHARP
HANOVER
CHARLES
LIGHT
CALVERT
BASIN
KEY
WARREN
WILLIAM
GITTINGS ST
FORT
RANDALL
HEATH ST
JOHNSON
LAKE
ANNAPOLIS
ROAD
MOUNT AUBURN CEMETERY
MIDDLE
BRANCH
WATERVIEW
AVE
30
19
9
10
704
541
22
129
72A
540
156
225
A

FIG. 1

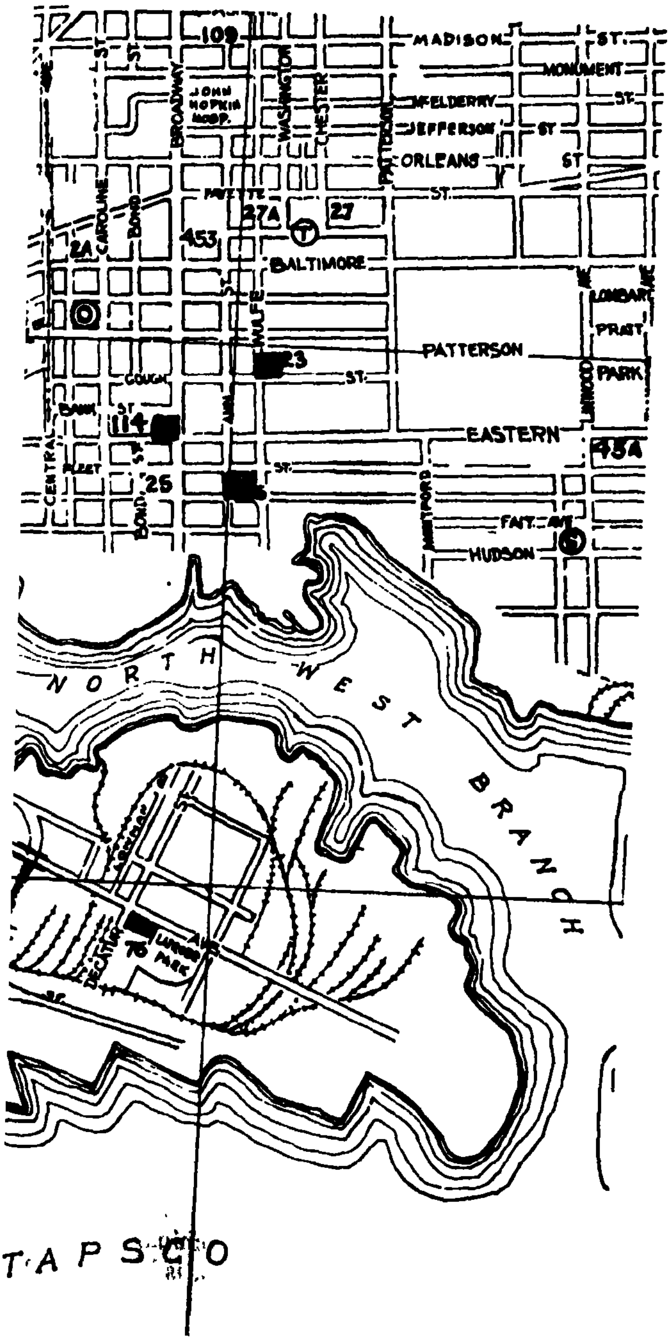


Fig. 1—*Continued*

The validity of associations as indicators of linguistic development derives mainly from the correlation between grammatical sophistication and the appearance of paradigmatic associates and from the gradual emergence of responses typical of adults (therefore mature, by definition). We have gathered associations to common words ("table," "sell," "black," etc.), and so our data concern basic language skills. More refined and elaborate measures, such as sentence length, use of various grammatical constructions, vocabulary size, and so on, might well demonstrate cleavage among groups that are equivalent on the word-association measures, or show differences of another kind from those we find. The reader should bear this in mind.

Prior to 1965 we sampled word associations for children representing various cultural and socioeconomic groups in Maryland and nearby regions of Pennsylvania.[4] There were strong differences between rural and suburban children but negligible differences between suburban blue-collar and suburban upper-middle-class groups. The present study focuses on urban slum groups, both Negro and white, because we wished to see if extreme SES differences (slum vs. suburban) would have impact on language development when smaller SES differences (blue-collar vs. upper-middle class) had not.

METHOD

Data were gathered from children in Baltimore City, Maryland, who resided within census tracts (see Fig. 1) where median family income is as low as $2,400. Elementary schools in the downtown area are very imbalanced racially, so to procure children whose living conditions were similar, schools with one racial group predominating were balanced by nearby schools with the other group predominant. For instance, school 19, all Negro, is balanced by

TABLE 1

DESIGN OF SUBJECTS IN SAMPLE

Subjects	White Children				Negro Children				Grand Totals
	White Int.		Negro Int.		Negro Int.		White Int.		
	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	
Kindergarten:									
No. of children	....	20	....	20	....	20	....	20	80
Average I.Q.*	....	....	....	....	....	....	....	....	
First grade:									
No. of children	20	20	17	20	20	20	9	20	146
Average I.Q.	100.1	79.5	99.4	80.1	98.7	79.9	97.6	79.9	
Third grade:									
No. of children	20	20	19	20	20	20	20	16	155
Average I.Q.	99.7	80.8	100.3	80.5	99.9	80.6	99.6	81.1	
Fifth grade:									
No. of children	20	20	20	20	20	20	20	20	160
Average I.Q.	99.7	80.3	99.8	80.6	99.5	80.7	99.6	80.5	
Total	....	....	....	....	....	....	....	....	541

* No data available.

[4] Entwisle, *op. cit.* See also, Doris R. Entwisle, "Developmental Sociolinguistics: A Comparative Study in Four Subcultural Settings," *Sociometry*, XXIX (March, 1966), 67–84; and "Form Class and Children's Word Associations," *Journal of Verbal Learning and Verbal Behavior*, V (December, 1966), 558–65.

nearby school 10, mostly white, and the neighborhoods around the schools are similar. The design of the sample and the racial composition of the schools are shown in Tables 1 and 2. Negro and white children of average I.Q. (95–105) and low I.Q. (85 or less) were selected by using school records.[5] The entire design was replicated four times: white interviewers with white children, white interviewers with Negro children, Negro interviewers with white children, and Negro interviewers with Negro children. Racial groups are equivalent in terms of I.Q. and grade. Interviewers were all middle class with some college training. The interviewing procedure is reported completely elsewhere.[6]

The ninety-six stimulus words represent the several form classes (nouns, adjectives, verbs, pronouns, adverbs, and miscellaneous words). Nouns, adjectives, and verbs are stratified on frequency, so that there are eight high frequency (over 1,000), eight medium frequency (500–999), and eight low frequency (499 or less) of each according to the Thorndike-Lorge *J*-count. Other form classes do not permit a frequency division, so there are eight words of each kind.[7]

RESULTS

The results consist of lists of free associations. Data derived from these lists, such as frequency of primary responses, number of different responses, and percentages of paradigmatics, are the data analyzed. The "number of observations" in any analysis consists of a response measure for a specific group of children (say, medium-I.Q., fifth-grade, Negro children interviewed by white interviewers) to a set of eight or more homogeneous stimulus words (say, adverbs or high-frequency nouns).

It has been widely observed that, with increasing age, form-class matching of stimulus and response increases, especially over the early school years, so the number of paradigmatics is the principal measure used.[8] (A paradigmatic response to "go" is "run.") Rates of paradigmatic responding for each subsample group are presented in Table 3.[9]

Age trends in paradigmatic responding obviously differ by form class. For in-

TABLE 2

RACIAL COMPOSITION OF SCHOOLS SAMPLED

School	Non-White	White	Total
1	207	3	210
6	1	460	461
10	60	449	509
19 and A	1,447	0	1,447
22	155	620	775
23	0	394	394
114	447	18	465

[5] I.Q.'s for third- and fifth-grade children were mostly a Kuhlmann-Anderson score obtained in the second or fourth grade, respectively. First-grade children are given the Primary Mental Abilities (PMA) test on school entrance, and their I.Q.'s are based on four subtests, omitting the motor subtest.

[6] Entwisle, *Word Associations*.

[7] For a complete description of the stimulus list and its properties, as well as the method of determining form class of stimulus words and response words and frequency of stimulus words, see Entwisle, *Word Associations*.

[8] Roger Brown and Jean Berko, "Word Association and the Acquisition of Grammar," *Child Development*, XXXI (March, 1960), 1–14; Susan Ervin, "Changes with Age in the Verbal Determinants of Word Association," *American Journal of Psychology*, LXXIV (September, 1961), 361–72; Entwisle, *Word Associations*. Also, for elementary school children, restricted and free associations are very highly related (Klaus F. Riegel, Ruth M. Riegel, Helen E. Smith, and Carole J. Quarterman, "An Analysis of Differences in Word Meaning and Semantic Structure between Four Education Levels" [unpublished manuscript, Department of Psychology, University of Michigan, November, 1964]), with close to 80 per cent overlap between free associations and associations given to instructions to produce superordinates, co-ordinates, contrasts, and so on. Thus free associations are highly related to comprehension of the word and ability to place words in appropriate contexts.

[9] In cases where subsamples were not equal to twenty, the response rate was adjusted proportionately.

TABLE 3

PERCENTAGES OF PARADIGMATIC RESPONSES, BALTIMORE CITY CHILDREN, 1965–66

Form Class of Word	White Children				Negro Children			
	White Int.		Negro Int.		Negro Int.		White Int.	
	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.
	First Grade							
High-freq. nouns	65.0	62.5	50.0	66.3	56.9	45.0	54.4	60.0
Med.-freq. nouns	62.5	58.8	56.3	68.7	61.9	44.4	66.9	58.7
Low-freq. nouns	60.6	58.8	50.6	61.9	54.4	48.7	48.7	53.1
Total nouns	62.7	60.0	52.3	65.6	57.7	46.0	56.7	57.3
High-freq. adj.	49.4	36.3	52.5	42.5	40.0	21.3	44.4	28.7
Med.-freq. adj.	44.4	35.0	46.9	36.3	40.0	26.3	37.5	26.9
Low-freq. adj.	43.7	26.9	38.7	26.9	34.4	17.5	37.5	25.0
Total adj.	45.8	32.7	46.0	35.2	38.1	21.7	39.8	26.9
High-freq. verbs	34.4	28.7	31.3	26.9	20.6	28.1	25.0	29.4
Med.-freq. verbs	27.5	21.9	19.4	22.5	15.0	13.7	20.6	21.9
Low-freq. verbs	20.0	20.0	23.1	20.0	16.3	17.5	23.7	23.1
Total verbs	27.3	23.5	24.6	23.1	17.3	19.8	23.1	24.8
Adverbs	28.7	10.6	25.0	19.4	13.7	11.3	18.1	13.1
Pronouns	51.3	28.1	40.0	36.3	33.1	26.9	41.9	35.0
	Third Grade							
High-freq. nouns	69.4	68.7	76.9	70.6	73.7	61.9	66.9	62.5
Med.-freq. nouns	75.0	70.6	76.3	68.1	76.9	64.4	75.0	65.0
Low-freq. nouns	75.6	75.6	90.0	68.7	67.5	66.3	75.0	61.9
Total nouns	73.3	71.7	81.0	69.2	72.7	64.2	72.3	63.1
High-freq. adj.	73.7	77.5	85.0	74.4	76.9	68.7	64.4	59.4
Med.-freq. adj.	64.4	61.3	71.9	63.1	70.0	63.7	62.5	46.9
Low-freq. adj.	53.1	53.7	66.3	55.0	55.0	48.1	53.1	43.1
Total adj.	63.7	64.2	74.4	64.2	67.3	60.2	60.0	49.8
High-freq. verbs	55.6	50.6	55.0	42.5	57.5	39.4	50.0	48.1
Med.-freq. verbs	34.4	30.0	35.0	28.1	35.0	25.6	27.5	22.5
Low-freq. verbs	25.6	33.1	23.1	23.7	31.9	23.7	30.0	28.1
Total verbs	38.5	37.9	37.7	31.5	41.5	29.6	35.8	32.9
Adverbs	44.4	35.6	36.9	43.7	38.1	30.6	31.9	30.0
Pronouns	66.9	65.0	72.5	63.1	75.0	63.1	65.0	58.7

TABLE 3—*Continued*

Form Class of Word	White Children				Negro Children			
	White Int.		Negro Int.		Negro Int.		White Int.	
	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.
	Fifth Grade							
High-freq. nouns	73.7	72.5	75.6	65.6	78.7	75.6	76.3	71.3
Med.-freq. nouns	80.6	75.0	75.6	76.9	80.6	75.0	85.0	81.3
Low-freq. nouns	89.4	84.4	93.7	84.4	88.1	83.1	85.3	83.7
Total nouns	81.3	77.3	81.7	75.6	82.5	77.9	82.3	78.7
High-freq. adj.	93.7	85.0	90.6	88.1	85.6	87.5	91.9	91.3
Med.-freq. adj.	79.4	76.9	86.3	71.9	75.0	75.0	79.4	76.9
Low-freq. adj.	75.0	73.8	78.7	76.3	81.9	72.5	77.5	75.6
Total adj.	82.7	78.5	85.2	78.7	80.8	78.3	82.9	81.3
High-freq. verbs	79.4	64.4	79.4	69.4	66.3	55.6	78.7	75.6
Med.-freq. verbs	49.4	36.9	43.7	38.7	41.9	36.9	52.5	43.1
Low-freq. verbs	53.1	44.4	43.7	51.9	45.0	45.6	64.4	46.3
Total verbs	60.6	48.5	55.6	53.3	51.0	46.0	65.2	55.0
Adverbs	61.9	50.6	57.5	53.1	62.5	44.4	58.1	54.4
Pronouns	77.5	71.9	80.6	72.5	66.9	75.0	73.1	85.6

stance, noun responses to nouns generally increase between kindergarten and fifth grade, but this increment is considerably less than that for adjectives or verbs. An analysis of variance of paradigmatic responses to nouns, adjectives, and verbs reveals a significant interaction ($p < .01$) among form class, grade, I.Q., race of child, and race of interviewer, so results are presented in terms of *individual* form-class analyses.[10]

Responses to adjectives.—The variance analysis for paradigmatic responses to adjectives shows a highly significant grade × I.Q. × race-of-interviewer × race-of-child interaction (see Table 4). At both first and third grades, Negro children are noticeably lower in seven out of eight comparisons. Differences between the races decrease with age, being 6–11 per cent at first grade and no more than 3 per cent by fifth grade.

Third-grade children are most affected by race of interviewers. When races of interviewers and children are mixed, Negro interviewers elicit more paradigmatics from white children than white interviewers elicit from Negro children, with one exception.

Suburban children from Baltimore with similar I.Q.'s were sampled in 1961–63 at two SES levels: (1) "high SES" suburban children whose fathers' average schooling amounted to 13.8 years and whose families

[10] These findings are consistent with earlier work. See, for example, Entwisle, *Word Associations*, "Developmental Sociolinguistics," and "Form Class"; Also, Doris R. Entwisle and Daniel F. Forsyth, "Word Associations of Children: Effect of Method of Administration," *Psychological Reports*, XIII (July, 1963), 291–99; Doris R. Entwisle, Daniel F. Forsyth, and Rolf Muuss, "The Syntactic-Paradigmatic Shift in Children's Word Associations," *Journal of Verbal Learning and Verbal Behavior*, III (February, 1964), 19–29.

had incomes of about $9,200; (2) "low SES" suburban children whose fathers had about 10.5 years of schooling and whose families had incomes of about $6,200. As mentioned earlier, the present study was undertaken partly to extend the variability on the socioeconomic dimension because it was thought that greater deprivation (family income at about $3,000) might yield language deficits, even though no differences had appeared between middle-class and working-class groups. Just the opposite seems to be true for first graders. Rates for first-grade slum children *exceed* rates for first-grade suburban children matched for I.Q. The medium-I.Q. slum children respond to adjectives much like the high-I.Q. (over 130) suburban children, and although the Negro slum children are less advanced than the white slum children, those of average I.Q. are responding at rates close to 10 per cent higher than either the high- or low-SES suburban white groups. The low-I.Q. white slum children are responding at rates close to those for the medium-I.Q. white suburban children, and the low-I.Q. Negro slum children are about the same as low-I.Q. white suburban children.

By third grade, however, medium-I.Q. slum children of both races *lag behind* suburban children. All children appear to attain the same asymptotic rate by fifth grade, so rates of development differ, but eventually all children attain the same level. At later grades, the early advantage of the slum children in terms of paradigmatic response rates to adjectives is lost.

Responses to verbs.—Previous work suggests that verbs continue to develop after fifth grade, but the major portion of development has occurred by then. (Adjective development is practically complete at third grade.)[11] Paradigmatic responses to verbs show somewhat different trends from those observed for adjectives (see Table 5). Although the impact of race differs by age, racial differences do not vary from one I.Q. level to another. Frequency is apparently a more important variable as far as verbs are concerned.

For verbs, first-grade slum children are more advanced than first-grade suburban children at both I.Q. levels. Again, there is a reversal at third grade, and the relative advantage of the slum children disappears. At fifth grade, Negro slum children are about 5 per cent higher than white slum children *or* white suburban children of both I.Q. levels. Generally the same patterns with age and the same relative position of slum children with respect to suburban children are seen whether adjectives *or* verbs are analyzed.

TABLE 4

AVERAGE PERCENTAGES OF PARADIGMATIC RESPONSES TO ADJECTIVES; GRADE × I.Q. × R.I. × R.C. INTERACTION*

Grade	White Children				Negro Children			
	White Int.		Negro Int.		Negro Int.		White Int.	
	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.
1......	45.8	32.7	46.0	35.2	38.1	21.7	39.8	26.9
3......	63.7	64.2	74.4	64.2	67.3	60.2	60.0	49.8
5......	82.7	78.5	85.2	78.7	80.8	78.3	82.9	81.3

* R.I. = race of interviewer; R.C. = race of child.

[11] See Entwisle, *Word Associations*. Also see Palermo and Jenkins, *op. cit.*

Adverbs and pronouns.—Previous work shows that pronouns develop most rapidly between kindergarten and third grade. Adverbs, like verbs, continue to develop up to fifth grade and probably thereafter. A comparison of suburban and slum children on responses to adverbs and pronouns again documents the superiority of first-grade slum children and a falling behind at third grade. White slum children of medium I.Q. (100) closely resemble white suburban children of high I.Q. (130) for both adverbs and pronouns. The relative positions of various I.Q. and racial slum groups are fairly consistent from grade to grade for both adverbs and pronouns. Suburban children are somewhat superior to slum children in paradigmatics to adverbs at fifth grade. With this exception, the findings altogether for pronouns and adverbs are reminiscent of findings for adjectives and verbs, respectively.

Development of high-frequency responses to nouns.—So far, only paradigmatic response rates have been considered. This section concerns primary responses (the single response with the highest frequency) to nouns, which show increments in high-frequency adult responses with increasing age of the child. For instance, "chair," the primary adult response to "table," is more frequent at age ten than at age five. Frequency of primaries is a measure of maturity that is independent of other measures previously considered, and the relative advancement of first-grade slum children is again noted (see Table 6).

Responses that *increase* in frequency between first and third grades in the large suburban samples are arbitrarily defined as "mature," and the prevalence of "mature" responses in first-grade slum and suburban groups can be compared. For instance, the response "pepper" to "salt" increases in frequency from 44 per cent to 55 per cent in suburban children between first and third grades. First-grade slum children give this response 50 per cent of the time, and so in this primary response they are between first- and third-grade suburban children. All the responses listed in Table 6 increase in suburban children between first and third grades except "fly," "table," and "crayon," so all responses except these are mature. For the responses that decrease, "fly" and "table," the slum rates are lower

TABLE 5

AVERAGE PERCENTAGES OF PARADIGMATIC RESPONSES FOR INTERACTIONS INVOLVING VERBS

	First Grade				Third Grade				Fifth Grade			
	White Int.		Negro Int.		White Int.		Negro Int.		White Int.		Negro Int.	
	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.	Med. I.Q.	Low I.Q.
High-freq......	29.7	29.1	25.9	27.5	52.8	49.4	56.3	40.9	47.8	70.0	72.8	62.5
Med.- freq.....	24.1	21.9	17.2	18.1	30.9	26.3	35.0	26.9	50.9	40.0	42.8	37.8
Low-freq........	21.9	21.6	19.7	18.7	27.8	30.6	27.5	23.7	58.7	45.3	44.4	48.7
	White Int.		Negro Int.		White Int.		Negro Int.		White Int.		Negro Int.	
	White C.	Negro C.	Negro C.	White C.	White C.	Negro C.	Negro C.	White C.	White C.	Negro C.	Negro C.	White C.
Average........	25.4	23.9	18.5	23.9	38.2	34.4	35.5	34.6	65.3	60.1	48.5	54.5

than suburban rates, which is consistent with the notion that slum children are giving "more mature" responses. Slum children exceed suburban children at first grade on all other responses except for "color" and "bird." The suburban children are about ten points higher in average I.Q., a difference that would lead one to predict a higher rate for suburban children, rather than the reverse.

Certain low-frequency words ("butterfly," "cocoon," etc.) must be heard infrequently by slum children, so one might guess that the lag displayed by slum children at third and fifth grades stems from reduced exposure to such words. The data do not support this. Suburban children of third and fifth grades are superior in terms

TABLE 6

COMMON RESPONSES TO NOUNS (FOR HIGH-FREQUENCY AND MEDIUM-FREQUENCY STIMULI)

Stimulus	Response	Suburban First Grade	Suburban Third Grade	Slum First Grade White	Slum First Grade Negro
Bird......	Fly	37.2	35.0	25.0	15.0
Chair......	Table	20.8	18.9	15.0	15.0
Color......	Red	12.1	18.9	10.0	20.0
	Crayon	8.6	0.0	10.0	0.0
Flower.....	Rose	6.4	12.9	20.0	15.0
Fly.......	Walk	0.0	17.2	20.0	0.0
Fruit......	Apple	5.7	22.8	20.0	10.0
Hand......	Arm	3.6	18.2	25.0	Finger 20.0*
Man......	Woman	12.9	43.5	25.0	20.0
Music.....	Sing	11.4	12.9	10.0	Dance 20.0*
	Song......	0.0	10.0	15.0	0.0
Ocean.....	Water	19.0	21.8	20.0	15.0
	Sea	10.0	40.0	15.0	0.0
River.....	Water	18.6	21.7	25.0	15.0
Salt.......	Pepper	44.3	56.4	50.0	40.0
Sheep.....	Lamb	15.7	22.5	35.0	30.0
Square.....	Circle	7.1	15.7	10.0	0.0
	Triangle	0.0	6.4	15.0	20.0
Table.....	Chair	36.1	51.4	60.0	30.0
Wing......	Fly	22.5	39.0	25.0	15.0
	Bird	15.0	19.6	15.0	Chicken 10.0*

* High-frequency responses given by Negro children only, with high-frequency response of white children absent.

Negro slum children give some responses that are very different from white children—for instance, "chicken" to "bird," and "dance" to "music." Such differences may signify a different subcultural semantic structure and will be the topic of a future report. The Negro slum children do manifest many of the same mature responses as the white slum children ("triangle" in response to "square"), and the Negro slum children show rates *ahead* of white suburban children for half of these noun responses.

of high-frequency words as well as low-frequency words. Frequency does appear to be a more potent variable for slum children than suburban children, however, since ranges between high- and low-frequency stimuli for slum children are larger. Also, the interactions involving frequency noted with verbs suggest that young slum children may be exposed to the low-frequency stimuli on our list less often than suburban children. Frequency as currently

measured (by counting occurrences of words in children's books) may not be appropriate for estimating frequencies of less common words of inner-city children. It turns out that slum children give the response "roach" six times as often as suburban children, for example.

DISCUSSION

The outcomes of this research we anticipated were: (1) again no difference between socioeconomic groups, or (2) a retardation in the slum groups because of their relatively greater cultural deprivation. Much evidence (paradigmatic responses to adjectives, verbs, pronouns, and adverbs, as well as primary responses to nouns, a measure independent of the others) suggests that first-grade white slum children are *more* advanced in linguistic development than suburban children of the same intelligence level.[12] To find slum children superior is both unanticipated and exciting because it raises important questions about cultural deprivation and also about the role of cultural factors in early linguistic development.

Previous work suggests that degree of urbanization may affect verbal development in young children. Briefly, it was found that rural Maryland children were behind suburban Maryland children; that Amish children residing in nearby areas of Pennsylvania were behind rural Maryland children; and that blue-collar and upper-middle-class suburban Maryland children, who dwelt in areas of similar urbanization, were about the same. Verbal interaction is probably less frequent for rural children than for suburban children but equally likely for blue-collar and higher-status children. Customs of the Amish (little conversation between parents and children, lack of mass media, marked separation from neighbors, etc.) must discourage interaction even more than rural living per se, and this may account for the rural-Maryland Amish difference.

What characterizes inner-city living that could foster rapid early linguistic development? Homes are crowded, and television sets are practically universal. The slum child's verbal environment may be considerably enriched by television, and his almost unrestricted access to this medium may be the most important cause of the verbal acceleration we observe.[13] Also, there is little evidence to cite, but the pressures upon young slum children to become verbally proficient may be very powerful. Lower-class women are often employed, perhaps forcing the young slum child to acquire verbal skills because his needs must be met by persons other than his mother.

Psycholinguists, who have so far concentrated mainly on early childhood up to about age four, increasingly feel that language acquisition proceeds in line with some kind of internal monitoring.[14] To state the idea in an oversimplified way, the child is disposed to accept certain word orders and to ignore the numerous possibilities that do not occur. He seems genetically tuned to decode the sequences he hears. For simple verbal concepts, the verbal interaction required to support language acquisition activities may be more

[12] It seems likely that the "true" I.Q. of slum children testing at 100 may be higher than 100, yet test bias cannot account for the superiority of slum children because a test bias present at first grade could not evaporate by third grade. Third-grade children of average tested I.Q. should also be advanced on measures (verbs and adverbs) still displaying strong I.Q. differentials at age eight if the test-bias argument holds. This is not true; the slum children are behind on all measures by third grade, and then the medium-I.Q. slum children resemble the low-I.Q. suburban children.

[13] A more detailed discussion of television vis-à-vis subcultural differences in linguistic development is contained in Doris R. Entwisle, "Subcultural Differences in Children's Language Development," *International Journal of Psychology* (in press).

[14] See, for example, George A. Miller, "The Psycholinguists: On the New Scientists of Language," *Encounter*, XXIII (July, 1964), 29–37; Eric H. Lenneberg, *New Directions in the Study of Language* (Cambridge, Mass.: M.I.T. Press, 1966); David McNeill, "Developmental Psycholinguistics" (Cambridge, Mass.: Center for Cognitive Studies, Harvard University, 1966 [mimeographed]).

favorable for the very young slum child than for the suburban child.

In addition, the nature of interaction may favor the slum child. Hess and his associates[15] have observed verbal interaction occurring between Negro mothers and their preschool children from four social classes (college educated to welfare) as mothers attempt to teach their children simple conceptual tasks. Upper-status mothers use more speech, and more elaborate speech, than lower-class mothers, who use simple short sentences and gestures. Use of abstract words is directly related to social class level. The lower-class mothers are more apt to limit the range of choices open to the child and the time required to make a choice. Mothers differed little in affective elements of interaction, but greatly in the verbal and cognitive environments they presented to their children. Such class-specific modes of verbal interaction suggest how acquisition of early verbal skills could be aided at the expense of more sophisticated skills. The first-grade middle-class child could be temporarily at a disadvantage because exposure to more complicated models may not be optimum for acquisition of the very simplest concepts. Table 3 suggests this because the first-grade slum children, in spite of their general superiority, are *not* superior on low-frequency verbs.[16] At third grade, where they are generally inferior, the inferiority is much more noticeable for low-frequency verbs (20 per cent) than for high-frequency ones (7 per cent). In addition, at third grade, adverbs are noticeably depressed, especially for Negro children. Pronouns, by contrast, are not depressed at third grade, and seemed to be the most highly developed of any form class at first grade. Pronouns are the most frequent class in spoken conversation, and their relative frequency must be higher for simple short interchanges than for complex utterances. Adverbs, on the other hand, express much more subtle meanings.

It is not clear what subcultural differences in verbal interaction could produce the Negro-white difference in paradigmatic rates. Dialect cleavage may operate when children are being interviewed (especially the youngest children). Middle-class Negro interviewers may differ more from lower-class Negro children than from lower-class white children because the speech of Negro college girls is more like that of the white community than the Negro slum. Dialect cleavage may also operate when the Negro preschooler attends to verbal models (mostly white speakers) presented via television.

Consistency with other data.—Our results are not so unexpected or so isolated as one might at first think. For instance, lower-class children of both races, when tested on comprehension of paragraphs of children's speech, do as well as middle-class children,[17] and lower-class and middle-class preschoolers[18] are equally good in applying morphological rules. Also, John,[19] using form-class matching in a word-association test as an index of maturity for first- and fifth-grade Negro children from lower-lower (slum), upper-lower, and middle-class families, finds no class differences at

[15] See Robert D. Hess and Virginia C. Shipman, "Early Experience and Socialization of Cognitive Modes in Children," *Child Development*, XXXVI (December, 1965), 869–86.

[16] This is consistent also with the observation that sheer exposure to speech at an early age is perhaps just as beneficial as expansions by the parent on incomplete utterances of the child (see Courtney B. Cazden, "Environmental Assistance to the Child's Acquisition of Grammar" [unpublished Ph.D. dissertation, Harvard University, 1965]).

[17] Martin Deutsch and Estelle Cherry-Peisach, "A Study of Language Patterns," *Instructor* (March, 1966). See also Estelle Cherry-Peisach, "Children's Comprehension of Teacher and Peer Speech," *Journal of Speech and Hearing Research* (in press).

[18] Thomas H. Shriner and Lynn Miner, "Morphological Structures in the Language of Disadvantaged and Advantaged Children," *Journal of Speech and Hearing Research* (in press).

[19] Vera P. John, "The Intellectual Development of Slum Children: Some Preliminary Findings," *American Journal of Orthopsychiatry*, XXXIII (October, 1963), 813–22.

first grade or fifth grade, although middle-class children have significantly larger vocabularies by fifth grade. I.Q. differences of from eight to eleven points favor middle-class over slum children in her study.

Linguistic differences associated with social class often fade away when I.Q. is held constant. In a study of speech quality in lower-class and middle-class children, variety of output in a timed speech sample showed a difference between lower-class and middle-class *only* when children also differed in I.Q.[20] Also, socioeconomic and class differences as reported on some parts of the Illinois test are very small and probably attributable to I.Q. bias, according to our re-analysis of some data for first-grade children obtained in Schenectady, New York.[21]

Actually there are few class differences in children's language behavior and general cognitive style that cannot be explained by the association between socioeconomic status and intellectual level. Although recent research points up the importance of environmental influences on the I.Q. a diffuse measure such as I.Q. does not lend itself either to fruitful hypothesis shaping or to fruitful practical recommendations. Little is done either to advance theory or to cope with current social problems by repeating I.Q. surveys of culturally deprived groups because two important questions are ignored: (1) whether there are differences in more specific areas of cognitive functioning, and (2) how such deficits (or advantages) are related to specific features of the environment.

Our work shows that slum children are not retarded in a low-level kind of verbal functioning and, in fact, are probably advanced. To score at an average level on an I.Q. test while being advanced in one area (language) covered by the test might suggest that performance in other areas (number concepts, space perception) is below par. In checking this conjecture by comparing scores on the various subtests of the PMA, the verbal subtest was *not* consistently the highest, nor were there large differences in mental age between subtests. The implication is that smaller cognitive domains need to be investigated. Failure to identify specific areas and types of deficit may be one reason that programs for the culturally disadvantaged have so far had so little impact. The thrust of research needs to be toward considering social class a discrete set of experiences, and cognitive activities discrete processes. It has been repeatedly shown, for instance, that associative strength (measured by free-association response frequency) is an important determiner of recognition time or learning difficulty. If reading primers are constructed on the basis of middle-class response strengths that are inappropriate for lower-class semantic systems, then a further burden is given the lower-class child. This middle-class orientation is often pointed to, but little direct effort has been aimed at defining precisely what a lower-class orientation consists of. If, as our data mildly suggest, it is in the low-frequency verbal concepts and in the verb-adverb segments of language that slum children are most deficient, specific remedial efforts might be concentrated in these areas and directed toward older children in addition to the ones currently being attended to most in social action programs.

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[20] Martin Deutsch, Alma Maliver, Bert Brown, and Estelle Cherry-Peisach, "Communication of Information in the Elementary School Classroom" (Cooperative Research Project No. 908 [Washington, D.C.: Office of Education, U.S. Department of Health, Education, and Welfare, 1964]).

[21] Vito M. Gioia, "A Comparison of Language Abilities in First-Grade Children from Differing Socioeconomic Groups" (unpublished Ph.D. dissertation, St. John's University, New York, 1965).

Ethnic Endogamy—the Case of Mexican Americans[1]

Frank G. Mittelbach and Joan W. Moore

ABSTRACT

A three-generational analysis of marriages involving Mexican Americans in Los Angeles County shows higher rates of exogamy than do earlier studies. Exogamy is higher for women and increases with removal from immigrant status. There is a strong pattern of generational endogamy and a strong suggestion that social distance between generations may be as important as social distance between the ethnic group and the dominant society. Exogamy is more prevalent among higher-status individuals; with some exceptions, occupation appears to be a better predictor of exogamy than generation. Generally, the older the groom, the more "Mexican" the spouse, though the pattern is not the same for brides. The findings have implications for assimilation of the Mexican Americans and for understanding processes of assimilation.

Theorists concerned with problems of social differentiation have generally taken the endogamy rate of a subpopulation as an indicator of the rigidity of boundaries around it. (Boundaries are, of course, due both to prejudice against the group and to the group's internal cohesiveness.) As Merton argues,[2] this is justified theoretically because exogamous marriages disrupt the network of primary-group relationships that underlie the cohesiveness of any population. In some respects, this is a functional as well as a motivational explanation in that the threat of disrupting primary-group relationships acts as a deterrent to exogamous marriages.

Empirical studies of intermarriage between subpopulations in this country have in some cases emphasized the significance of external boundaries imposed by prejudice, as, for example, in the analysis of Negro exogamy. In other research, as in the analysis of upper-class endogamy and endogamy of European ethnic populations, much literature assumes the boundaries internally maintained by in-group solidarity to be the important factor.

This paper examines the Mexican Americans, the nation's second largest disadvantaged minority and a group which has often been portrayed as distinctively unassimilated and unacculturated—that is, one which has unusually strong boundaries around it. Though these boundaries are most frequently attributed to in-group cohesiveness, Mexican Americans have also experienced considerable prejudice and discrimination. In short, analysis of patterns of intermarriage among this group, which shares minority status with Negroes and shares ethnic distinctiveness with other foreign immigrant groups, offers some promise of fruitful insight into the proc-

[1] In preparing this paper we received the support of the Mexican-American Study Project at the University of California, Los Angeles. We express our particular appreciation to Ronald McDaniel for his assistance and contributions. Helpful comments were received from Leo Grebler and Ralph Guzman. The authors, however, take full responsibility for all data, findings, and interpretations. This is a revised version of a paper presented at the American Sociological Association, August, 1966, meeting.

[2] The literature on intermarriage is extensive. For this general discussion we have drawn on many sources, prominently on the classic analyses by Davis and Merton (Kingsley Davis, "Intermarriage in Caste Societies," *American Anthropologist*, XLIII (1941), 376–95, and Robert K. Merton, "Intermarriage and Social Structure: Fact and Theory," *Psychiatry*, IV (1941), 361–74), as well as the recent extensive discussion by John Finley Scott, "Endogamy in Industrial Societies" (unpublished MS, Sociology Department, University of California, Davis).

esses of boundary maintenance and dissolution.

The paper has two purposes. The first is to increase understanding of the current status and future prospects for assimilation of this population, and the second is to amplify general propositions about conditions under which exogamy occurs. The two purposes reinforce each other: Mexican-American patterns of endogamy are best understood in a general context, and the analysis of this unusual case may help extend the theoretical frontiers slightly.

Previous studies of Mexican-American endogamy are comparatively rare, but all show extremely low rates of exogamy, supporting the notion of the group as distinctively unassimilated. Panunzio's study of individuals born in Mexico and marrying in Los Angeles between 1924 and 1933 showed that only 9 per cent of the individuals (and 17 per cent of the marriages)[3] were exogamous.[4] An analysis of Spanish-surname persons in Albuquerque from 1924 to 1940 indicated that only 8 per cent of the individuals (and 15 per cent of the marriages) were exogamous.[5] Finally, a more recent study of Spanish-surname persons in San Antonio marrying between 1940 and 1955 showed exogamy rates of no more than 10 per cent for persons (and 17 per cent for marriages) in any of the years sampled.[6] Considering that many of these persons in Albuquerque and probably in San Antonio are native born of native parents,[7] the image of this minority as distinctively unassimilated gains much support from these studies. And considering that these studies were prepared in different places and times, the notion that the low rate of assimilation is due to unusual strength of in-group bonds also gains much support.

Both social scientists and journalists have tended to emphasize the special strength of Mexican-American ascriptive bonds—especially to kin and "la raza" (diffusely, the ethnic group)[8]—in interpreting the slow rate of acculturation and assimilation in the population. However, this kind of interpretation does not help much in our analysis of intermarriage in contemporary Los Angeles. The Los Angeles patterns are understandable only in the light of general propositions about the process of assimilation. The data, for example, illustrate the effects of the considerable and growing internal differentiation within

[3] Calculations based on the number of marriages and on the number of persons involved in marriages can vary considerably. Studies reporting one without the other add greatly to the confusion in the understanding of intermarriage. The formula for calculating the in-group marriage for persons is $2E/E+N$, where E is the number of in-group marriages and N is the total number of marriages involving the subpopulation in question. We are indebted to Richard Griego for calling our attention to this relationship. See also Hyman Rodman, "Technical Note on Two Rates of Mixed Marriages," *American Sociological Review*, XXX (1965), 776–78.

[4] Constantine Panunzio, "Intermarriage in Los Angeles, 1924–1933," *American Journal of Sociology*, XLVII (1942), 690–701.

[5] Derived from Carolyn Zeleny, "Relations between the Spanish-Americans and the Anglo-Americans in New Mexico" (unpublished Ph.D. dissertation, Department of Sociology, Yale University, 1944), p. 334.

[6] Benjamin Spencer Bradshaw, "Some Demographic Aspects of Marriage: A Comparative Study of Three Ethnic Groups" (unpublished M.A. thesis, Department of Sociology, University of Texas, 1960). John Burma, in "Interethnic Marriage in Los Angeles, 1948–1959," *Social Forces*, LXII (1963), 156–65, also reports on the ethnicity of partners of exogamous Mexican Americans.

[7] Albuquerque contains many members of one of the nation's "charter member" minorities—descendants of Mexican colonists of the seventeenth and eighteenth centuries. These people call themselves "Spanish Americans" to distinguish themselves from the later Mexican immigrants and their children.

[8] For definition of the concept "la raza," see, e.g., William Madsen, *The Mexican-Americans of South Texas* (San Francisco: Holt, Rinehart & Winston, 1964). The phrase refers to the ethnic group, not "race" in a genetic sense. Madsen is one among many who hold to the importance of ascriptive bonds. See also Ruth Tuck, *Not with the Fist* (New York: Harcourt-Brace, 1956). Many of these studies are old or were done in rural areas—unlike contemporary metropolitan life.

the population. They suggest that this differentation has differentially weakened the holding power of the ascriptive bonds of "la raza."

In addition to pointing to the effects of this growing differentiation within the Mexican-American population, the study—whose findings depart so notably from the three cited above—strongly indicates the importance of the environing social system. The contemporary Los Angeles milieu is far less hostile to Mexican Americans and offers far more economic opportunity than any of the other milieus studied. These data thus compel the analyst to give consideration to external-system as well as internal-system effects on boundary maintenance of this group.

DATA AND MILIEU

The data which form the basis for our analysis consist of 7,492 marriage licenses issued in Los Angeles County during 1963 from a total of over 47,000 licenses. These 7,492 licenses include all marriages in which one or both spouses carry a Spanish surname.[9] By the definition adopted, a total of 9,368 Mexican-American individuals were identified. Of these 2,246 (or 24.0 per cent) were first generation or born in Mexico; 3,537 (or 38.2 per cent) were second generation, with one or both parents born in Mexico, and 3,585 (or 38.2 per cent) were third generation, defined as Spanish-surname individuals whose parents were born in one of the five Southwestern states where Mexican Americans are concentrated.[10]

The Mexican Americans hold a very special position in southern California, both as the major local minority and as compared with Mexican Americans elsewhere. In many respects they appear to be a "typical minority." Mexican Americans constituted slightly less than 10 per cent of the total Los Angeles County population in 1960, but more than 80 per cent were born in the United States. Though it is thus no longer an immigrant population, it is one which has been locally defined as a problem for more than two generations. Memories of the zoot-suit riots of the 1940's are still alive, and segregation from both Anglo whites and Negroes is sharp,[11] great enough so that three Los Angeles area high schools are predominantly Mexican American—a factor certainly important in the availability of marriage partners. However, prejudice against the population in Los Angeles is comparatively low and opportunities for status advancement quite high in comparison with other parts of the American Southwest. From the point of view of the environing social system, then, Los Angeles is an environment which facilitates interaction with the larger system.

FINDINGS

A. *Sex and generational differences.*—The over-all rate of exogamy in these data was much higher than anticipated. Forty per cent of the marriages involving Mexi-

[9] To identify Spanish surname, we used the U.S. Census "long" list, which includes more than 7,000 names. However, for classifying persons as first or second generation, nativity (as recorded on the marriage license) was given priority. For details, see Frank G. Mittelbach, Joan W. Moore, and Ronald McDaniel, *Intermarriage of Mexican-Americans* ("Mexican-American Study Project," Advance Report 6 [University of California, Los Angeles, 1966]), Appendix A.

[10] The third-generation Spanish-surname persons with parents born outside the Southwest were defined as non-Mexican-American. This seemed appropriate in light of the relatively high probability that such persons might be of other than Mexican descent. However, analysis of the data using such a reclassification did not importantly affect the rate of endogamy. Exploratory research suggests that name changing is not substantial among Mexican Americans. Also, it can be noted that, among the foreign stock Mexican Americans in our sample population, only 5.8 per cent did not have a Spanish surname. This could be due to intermarriage on the part of one of the parents, name changing, or a result of the fact that the list of Spanish surnames used was incomplete. The relative role of any of these factors is indeterminate, but in our judgment is presently insignificant.

[11] Joan W. Moore and Frank G. Mittelbach, *Residential Segregation in the Urban Southwest* ("Mexican-American Study Project," Advance Report 4 [University of California, Los Angeles, 1966]).

can Americans were exogamous, and 25 per cent of the Mexican-American individuals married outside their ethnic group. Although statistics in this field are notably difficult to find, it is clear that exogamy in Los Angeles today is much higher for Mexican Americans than in the past or in other urban areas.[12] Interestingly, the Mexican-American exogamy rate is roughly that of the Italian and Polish ethnic populations in Buffalo, New York, a generation ago. Both of these populations are now assimilating rather rapidly.[13]

The results for men and women by generation are presented in Table 1. They show the sex gradient to be as expected for a subpopulation occupying a low status.[14] Women are more exogamous than men, with the respective over-all rates for individuals of 27 and 24 per cent. The generational gradient suggests that exogamy will probably increase in the future as relatively more Mexican Americans move out of immigrant status. The most exogamous are third-generation women (32 per cent), and

[12] This conclusion was not arrived at lightly. Demographic factors also bear upon the rates of intermarriage. In particular, the number and proportion of the ethnic group in the total population as well as sex ratios will influence the rates. These factors may be neutral, or may reinforce or cancel each other. For a recent discussion on some of the issues involved, see Paul H. Besanceney, "On Reporting Rates of Intermarriage," *The American Journal of Sociology*, LXX (May, 1965), 717–21. Though the structure of the Mexican-American population differs from time to time and place to place, this did not appear to have any appreciable effect on the fact that the rates for Los Angeles in 1963 were lower compared with earlier periods and other areas. For an analysis of the demographic factors, see Mittelbach, Moore, and McDaniel, *op. cit.*

[13] B. R. Bugelski, "Assimilation through Intermarriage," *Social Forces*, XL (1961), 148–53. The 1930 rates of endogamy were 71 for Italians and 70 for Poles. The 1960 rates were 27 for Italians and 33 for Poles.

[14] For a discussion of this point, see Scott, *op. cit.*

TABLE 1

PERCENTAGE OF IN- AND OUT-GROUP MARRIAGES OF MEXICAN AMERICANS, LOS ANGELES COUNTY, 1963

Spouse	Mexican-American Grooms			Mexican-American Brides		
	Foreign Born, Mexico (1)	Foreign or Mixed Parentage, Mexico (2)	Natives of Native Parentage (3)	Foreign Born, Mexico (4)	Foreign or Mixed Parentage, Mexico (5)	Natives of Native Parentage (6)
Foreign born, Mexico	*51.9*	13.8	6.8	*48.5*	14.5	6.9
Foreign or mixed parentage, Mexico	22.8	*34.5*	23.8	21.8	*36.7*	27.0
Natives of native parentage	12.2	28.4	*38.9*	9.7	23.2	*33.8*
Subtotal, Mexican American	86.9	76.7	69.5	80.0	74.4	67.7
Hispanic,* foreign or mixed parentage	2.9	1.5	0.8	4.0	1.6	1.3
Other†	10.2	21.9	29.8	15.9	24.0	31.1
Total	100.0	100.0	100.0	100.0	100.0	100.0
(*N*)	(1,086)	(1,826)	(1,667)	(1,160)	(1,711)	(1,918)

NOTE.—Generationally endogamous cells are italicized.

* Excludes foreign stock from Mexico; includes foreign stock from Central and South America, the Philippines, and Spain.

† Includes natives of native parentage with Spanish surname with parents born outside the five southwestern states, natives of native parentage without Spanish surnames throughout the United States, and foreign stock outside of Mexico and other Hispanic countries.

the least exogamous are first-generation men (13 per cent). The generational gradient is steady (Table 1, "Subtotal" row). Sex differences are maintained within each generation, with women more exogamous than men. Similar findings were discovered in a recent two-generational analysis of Puerto Rican exogamy, and their interpretation there, as here, was made within a social-class context.[15]

Most significantly, there is a pattern of endogamy within each generation as well as within the ethnic group. It is apparent that individuals of every generational status tend to marry those with the same generational background.

More important, the data indicate that marriage of second- and third-generation Mexican Americans are assimilationist. Both men and women are more likely to marry "Anglos" than to marry immigrants from Mexico. Among third-generation persons, the chances are actually higher that he or she will marry an Anglo than either a first- *or* a second-generation Mexican. Further evidence can be found related to this generalization. In Table 2, persons with foreign or mixed parentage have been sorted out into those with both parents and those with only one parent born in Mexico. Clearly, Mexican-American men and women with both parents born in Mexico are more likely than those with mixed parentage to marry first- and second-generation spouses and less likely to marry third-generation spouses. By this indicator, the social distance between generations of Mexicans is greater than the social distance between some categories of Mexicans and Anglos. This apparently low degree of solidarity inside the ethnic group is not only contrary to much popular opinion but also to general sociological expectation about this group.

This three-generational gradient and the intra-Mexican variations may well exist in

TABLE 2

PERCENTAGE OF IN- AND OUT-GROUP MARRIAGES OF SECOND-GENERATION MEXICAN AMERICANS BY NATIVITY, LOS ANGELES COUNTY, 1963

Spouse	Second-Generation Mexican-American Grooms			Second-Generation Mexican-American Brides		
	Both Parents Born Mexico	Father Only Born Mexico	Mother Only Born Mexico	Both Parents Born Mexico	Father Only Born Mexico	Mother Only Born Mexico
Foreign born, Mexico	17.9	10.7	9.1	18.1	11.9	9.8
Foreign or mixed parentage, Mexico	36.5	31.5	34.0	38.6	34.2	36.6
Natives of native parentage	24.1	33.8	30.0	17.8	28.8	26.8
Subtotal, Mexican American	78.5	76.0	73.1	74.5	74.9	73.2
Hispanic foreign or mixed parentage	1.2	1.2	2.5	2.1	1.5	0.7
Other	20.3	22.8	24.4	23.4	23.6	26.1
Total	100.0	100.0	100.0	100.0	100.0	100.0
(*N*)	(885)	(588)	(353)	(811)	(605)	(295)

NOTE.—For both categories (brides and grooms), χ^2 significant past .001.

[15] Joseph P. Fitzpatrick, "Intermarriage of Puerto Ricans in New York City," *American Journal of Sociology*, LXXI (1966), 395–406. For analysis by occupation paralleling Fitzpatrick, see below.

other ethnic groups, but to our knowledge it has not been previously demonstrated. It gives a hint to the processes that take place as assimilation of a subordinate population progresses. The data suggest, for example, that members of an ethnic group become less "attractive" to the native born as they appear to be more "ethnic." Conversely, they may become more "attractive" as they become more similar to members of the host society. As differentiations between them and members of the host society become fewer, exogamy may be facilitated.

B. *The influence of occupational status.* —Some of the sources of this tendency toward assimilation are suggested by the occupational gradient in Mexican-American marriages. The only indicator of general social-class standing available on Los Angeles marriage licenses is the occupation of the bride and the occupation of the groom (parents' occupations are not reported). Because women's occupations are notoriously poor indicators of their social-class standing, we will here only examine the groom's occupation; that is, both brides and grooms are grouped according to occupational status of the groom. Obviously, this grouping has different implications for the analysis for bride data and groom data. In the former case, we examine the new family's class standing; in the latter, we examine the class standing of *both* the individual (the groom) and that of the new family of procreation. (We examine the new family's standing in both cases because in this society the husband's occupational status generally determines that of the family.) Thus, the kinds of generalizations we can draw from the two sets of data are more limited in the case of women than of men. In both instances, however, they are consistent with findings from earlier studies and, most saliently for our analysis, are also comparable with Fitzpatrick's data for Puerto Ricans.

Within these limitations, the analysis in general reaffirms the social-class context of exogamy (Table 3). Generally, the higher the socioeconomic status of the groom (or the new family of procreation) the greater the rate of exogamy. For Mexican-American women, slightly more than half of those marrying high-status grooms married exogamously, with only 49 per cent marrying Mexican-American men. Once again, as with generation, the gradient is remarkably steady. The most exogamous are women marrying high-status men, agreeing with Fitzpatrick's findings and probably his interpretation that "it is likely that . . . women are marrying up as they marry out."[16] The least exogamous are the women marrying low-status men.

Within each occupational group, exogamy increases as the person is further removed from immigrant status. This is true for both brides and grooms. Within each generational group, exogamy increases steadily with the socioeconomic status of the groom. This is true for each generation. It also holds for first- and second-generation grooms, but an anomaly appears in the third generation. Although the lowest rate of in-group marriage for men appears among the high-status third generation, the percentage of endogamy in the middle-status group far exceeds the endogamy of the lower-status group.

In most cases, Mexican-American women show a higher rate of outmarriage than do men. Data permit us to hold constant the generation of spouses and occupational level of the new family of procreation but not the more salient variable—occupational level of family of orientation. Therefore, we can only speculate with regard to sex patterns. If the observed pattern of greater outmarriage among second- and third-generation low-status men (compared with women marrying low-status men) were also to hold for family of orientation, this would mesh with other findings on sex differences in sociability patterns in lower classes—and particularly in Mexican-American lower classes. Generally, women of lower status levels have more restricted opportunities for forming

[16] *Ibid.*, p. 400.

social relationships outside the narrow limits of kinship and long-standing friendships.

The results tabulated in Table 3 show generally that both generation and occupation are relevant in Mexican-American exogamy, as they were found to be in Puerto Rican endogamy.[17] To gain some insight into whether occupation or generation is more important in influencing endogamy, we begin by ranking the percentages in Table 3 in a new tabulation by generation and by occupation (Table 4). Then, if occupation is more important than generation in exogamy, the rank order will emphasize occupation (with the generation ordered within each occupation, as in col. 3). If generation is more relevant, it will emphasize generation (with the occupation ordered within each generation, as in col. 4). The results show that actual rankings conform much more closely to the hypo-

[17] *Ibid.* Data on occupation were calculated so as to be comparable with Fitzpatrick's.

TABLE 3

IN-GROUP MARRIAGES AMONG MEXICAN AMERICANS BY OCCUPATIONAL STATUS OF GROOM

Occupation Group of Groom and Generation	Grooms		Brides	
	Percent In-Group	*N*	Percent In-Group	*N*
High:*				
Mexican born	66.2	74	57.6	99
Mexican parents	63.1	187	49.3	207
Native of native parents	51.4	138	44.6	177
Total	59.6	399	49.3	483
Middle:†				
Mexican born	87.5	353	75.3	413
Mexican parents	76.0	766	74.1	703
Native of native parents	75.6	870	69.0	1,039
Total	77.9	1,989	71.9	2,155
Low:‡				
Mexican born	88.7	626	87.3	612
Mexican parents	80.4	810	82.1	750
Native of native parents	65.6	593	71.3	624
Total	78.6	2,029	80.3	1,986
All:§				
Mexican born	86.9	1,086	80.0	1,160
Mexican parents	76.7	1,826	74.4	1,711
Native of native parents	69.5	1,667	67.7	1,918
Total	76.4	4,579	73.1	4,789

* Includes professional, technical and kindred workers, managers, officials, and proprietors (except farm).

† Includes clerical, sales and kindred workers, craftsmen, foremen and kindred workers, and farm owners and managers.

‡ Includes operatives and kindred workers, non-household service workers, private household workers, laborers, and farm workers.

§ All marriages include also those where occupation not reported, unemployed persons, students, and others with no occupation.

thetical rank order emphasizing occupation (Table 4, cols. 1 and 2).[18]

We note two departures from the hypothetical ranking among the men and one among the women. Interestingly, both Mexican-American men and women in the low-status, third-generation group marry out more than would be hypothesized from the influence of social class (or occupational status alone). Possibly this group is more acculturated and comfortable with Anglos than occupation alone might suggest. Perhaps the effect of being native born of native-born parents counteracts the cultural isolation of a low-status blue-collar job. It might also be an age-associated pattern. Possibly, low-status third-generation persons marry earlier than middle- and higher-status persons in the same generation. (No firm conclusions could be reached on this last point because cross-tabulations by generation, occupation, and age reduced sample sizes in the cells to small and often insignificant numbers.)

We do not feel these exceptions disturb the general conclusion that occupation is more significant than generation in explaining outmarriage for both Mexican-American men and women. Since only the occupation of the groom is reported, it further supports the suggestion that women's exogamy is probably associated with their upward mobility. For men, the situation is somewhat different. Puerto Rican men apparently marry in a pattern that emphasizes generational primacy rather than the Mexican-American pattern of occupational primacy. This undoubtedly reflects real differences between the Mexican Americans

TABLE 4

RANK ORDER OF OUT-GROUP MARRIAGE RATES BY SEX, OCCUPATION, AND GENERATION

Rank, from Most to Least Outmarrying	Actual				Hypothetical			
	Rank, Males (1)		Rank, Females (2)		Rank if Occupation More Important than Generation (3)		Rank if Generation More Important than Occupation (4)	
	Occupation	Gen.	Occupation	Gen.	Occupation	Gen.	Occupation	Gen.
1	High	3	High	3	High	3	High	3
2	High	2	High	2	High	2	Middle	3
3	Low	3	High	1	High	1	Low	3
4	High	1	Middle	3	Middle	3	High	2
5	Middle	3	Low	3	Middle	2	Middle	2
6	Middle	2	Middle	2	Middle	1	Low	2
7	Low	2	Middle	1	Low	3	High	1
8	Middle	1	Low	2	Low	2	Middle	1
9	Low	1	Low	1	Low	1	Low	1

[18] An analysis of variance of Table 3 for men was prepared and the results as follows:

Source of Variation	Sum of Squares	d.f.	Mean Square
Generation	.041	2	.0205
Occupation	.070	2	.0350
Interaction	.040	4	.0100

These data provide further support for the hypothesis that occupation contributes more to the variation in rates of intermarriage than does either generation or the interaction between these variables for the period under study. However, if the data were to be viewed as a sample from a larger universe (e.g., several years), then this conclusion would not necessarily hold.

of Los Angeles and the Puerto Ricans of New York.

We know that differences between the two groups are substantial. Mexican Americans are a long-established minority in Los Angeles, but Puerto Ricans are relatively new to New York. It is quite possible that Mexican Americans have made more substantial gains in social and occupational status than have Puerto Ricans.[19] Thus, occupational status might be more important, as is suggested by the wide variations in accordance with occupation as shown in Table 3.

There is also some suggestion in the literature that when Puerto Ricans marry "Anglos," this group may, in fact, include many Negroes. The closeness of Spanish and Negro Harlem and the comparatively low social and cultural distance between Puerto Ricans and Negroes would support this interpretation.[20] Marriage with Negroes is insignificant among Mexican Americans.[21] Possibly, then, the intermarriage rate of New York's Puerto Ricans may occur within a basically ghetto context in which linguistic facility is important but in which acceptability by the dominant white system is not necessary. Further, the higher density of the urban areas in New York may allow much greater intermingling of ethnic groups and tend to reduce social distance.

Still another important factor affects our comparison of Mexican Americans and Puerto Ricans. We have three generations of Mexican Americans and only two generations of Puerto Ricans. When we drop out the third generation of Mexican Americans and compare only two generations, one ethnic group to another, we find that generational primacy becomes as important to the Mexican Americans as it is to the Puerto Ricans. This suggests that in addition to the very real differences between the Mexican Americans and the Puerto Ricans, there are important general differences between ethnic groups viewed as two-generational populations and ethnic groups viewed as three-generational populations. Relations between first- and second-generation ethnics are distinctive for most American groups.[22] In time, as a third generation begins to be important in the Puerto Rican population, the social-class content of exogamy may assert itself.

It is plain that within an ethnic population consisting largely of immigrants and their children, the ethnic culture and the kinship relations of the family are closely interwoven. This is true whether a third generation is present (in the case of Mexican Americans) or is not present (Puerto Ricans). With the addition of a third generation, other kinds of social relationships and distinctions undoubtedly become more important. In the relatively open social system of Los Angeles, it is not surprising that occupational status is so significant.

C. *Age at marriage.*—Earlier research has shown that the age of a man or woman at marriage is definitely patterned by ethnic and social-class subcultures.[23] It is

[19] Given the many difficulties of comparing two ethnic groups in different environments, no firm conclusion can be reached on relative social status and economic achievement. Census data for 1960 on occupation and income indicate that in Los Angeles, 19 per cent of the Mexican Americans held white-collar occupations as compared with 17 per cent of the Puerto Ricans in New York. Also, 19 per cent of the Los Angeles Mexican Americans compared with 10 per cent of the New York Puerto Ricans were skilled workers. Moreover, income of Mexican Americans in Los Angeles was considerably above that of the Puerto Ricans in New York.

[20] This interpretation is supported by other reports on Puerto Ricans. See Nathan Glazer and Daniel Patrick Moynihan, *Beyond the Melting Pot* (Cambridge, Mass.: M.I.T. Press, 1963), pp. 129–42, for a summary of this material.

[21] See Burma, *op. cit.*

[22] The Japanese appear to be something of an exception. See Harry Kitano, *The Japanese-American* (New York: Prentice-Hall, in press).

[23] See Bernice L. Neugarten, Joan W. Moore, and John C. Lowe, "Social Age" (unpublished MS, Committee on Human Development, University of Chicago, 1962) for these data and a general discussion of the social and psychological significance of age.

reasonable to expect that Mexican Americans in Los Angeles also show such patterns. The aggregate data, including all cases where neither party had been previously married, indicated that the median age is 22.0 for Mexican-American grooms and 20.3 for brides (Table 5). This is slightly below comparable figures for the nation, although they have not been completely standardized.[24] But the aggregate results conceal important generational differences.

First- and second-generation Mexican Americans tend to be notably older and third-generation spouses notably younger than the average American at time of marriage. This gradient is not entirely unexpected in light of the variations in age composition of the first, second, and third generations. For example, in the Los Angeles SMSA, in 1960, only 13 per cent of the Spanish-surname population born in Mexico and over fifteen years of age was in the marriageable ages from fifteen to twenty-four. By comparison, individuals with native- or Mexican-born parents, respectively, accounted for 36 and 26 per cent of this marriageable age group.[25]

Given the differences in age composition, we find that among persons of every generation, the more "Mexican" the spouse, the older the bride or groom. Without more study one might accept the notion that per-

TABLE 5

MEDIAN AGES OF GROOMS AND BRIDES BY ETHNICITY (NEITHER PARTY PREVIOUSLY MARRIED)

Generation of Bride or Groom	Born Mexico	Parents Born in Mexico	Third or More Generation	Not Mexican American*
	Groom's Median Age†			
Bride:				
First generation	*25.9*	26.1	22.0	23.3
Second generation	24.0	*23.2*	21.3	23.1
Third or more generation	21 8	21 2	*20.8*	21.8
Subtotal, Mexican American	24.9	22.3	21.1	N.A.
Subtotal, not Mexican American	22.4	22.0	21.0	N.A.
Over-all	24.7	22.2	21.1	N.A.
	Bride's Median Age‡			
Groom:				
First generation	*23.7*	22.1	19.4	19.8
Second generation	22 6	*21 0*	19.3	19.5
Third or more generation	20.7	19.8	*19 1*	19.0
Subtotal, Mexican American	22 4	20.6	19.2	N.A.
Subtotal, not Mexican American	21.7	21.5	19.9	N.A.
Over-all	22.3	20.8	19.4	N.A.

NOTE.—Generationally endogamous cells are italicized.
* Excludes persons of non-Mexican Hispanic stock.
† Average age of all Mexican-American grooms, 22.0 years.
‡ Average age of all Mexican-American brides, 20.3 years.

[24] U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States* (Washington, D.C.: U.S. Government Printing Office, 1964), p. 65. Nationally, median age of bride at first marriage in 1963 was 20.4 for women and 22.8 for men.

[25] *1960, U.S. Census of Population* PC(2), 1B, Table 9.

sons who marry spouses close to immigrant status are marrying into an older population and are themselves likely to be older than those who marry native-born Mexican Americans. Further examination of the grooms suggests that this is not entirely sufficient as an explanation.

Table 6 holds both age and generation constant for Mexican-American grooms and examines the distribution of marriages by ethnicity and generation of brides. The data strongly suggest that in any particular generation of grooms, age has a stronger influence on mate selection by Mexican-American men within the ethnic community than on the endogamy rate proper. In every one of the three generations, older Mexican-American men tend to marry women close to immigrant status.

The Mexican-American men who married Anglo women tended to be somewhat younger than the men who married first- and second-generation brides and older than those who took third-generation brides, although differences are minor. Among the brides who married Anglo men, the pattern is more mixed, although for neither brides nor grooms are there strong reasons to believe that the exogamous spouses are necessarily older (Table 5). (The explanation for these patterns might be cultural or demographic. Present data on this issue are not available.)

TABLE 6

PERCENTAGE OF IN- AND OUT-GROUP MARRIAGES OF MEXICAN AMERICANS BY AGE OF GROOMS, LOS ANGELES COUNTY, 1963

Brides	Grooms, Foreign Born, Mexico			Grooms, Mexican Parentage			Grooms, Natives of Native Parentage		
	Less Than 25	25–34	Over 34	Less Than 25	25–34	Over 34	Less Than 25	25–34	Over 34
Foreign born, Mexico	42.1 (49.7)	56.4 (63.1)	61.6 (71.0)	8.0 (10.3)	18.1 (24.3)	24.8 (32.4)	5.8 (8.2)	8.9 (12.9)	14.1 (26.7)
Foreign or mixed parentage, Mexico	23.0 (27.2)	24.8 (27.7)	19.8 (22.8)	30.5 (39.2)	37.7 (50.6)	40.7 (53.1)	23.6 (33.4)	25.8 (37.4)	20.0 (37.8)
Natives of native parentage	19.6 (23.0)	8.2 (9.2)	5.3 (6.1)	39.3 (50.5)	18.7 (25.1)	11.1 (14.5)	41.2 (58.4)	34.2 (49.6)	18.8 (35.5)
Subtotal, Inmarriage	84.7 (100.0)	89.4 (100.0)	86.7 (100.0)	77.8 (100.0)	74.5 (100.0)	76.6 (100.0)	70.6 (100.0)	68.9 (100.0)	52.9 (100.0)
Foreign or mixed stock:									
Hispanic	2.0	3.7	3.4	0.5	2.6	2.6	0.8	0.7	1.2
Other, Outmarriage	13.3	6.9	9.9	21.7	22.9	20.8	28.6	30.4	45.9
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
(*N*)	(444)	(379)	(263)	(973)	(546)	(307)	(1,280)	(302)	(85)

NOTE.—Percentages in parentheses are based on endogamous marriages only.

These findings are somewhat contrary to other studies which concluded that men (apart from Mexican Americans) who marry outside their ethnic groups tend to be older.[26] This is usually taken as a reflection of some fairly complicated changes in the life of the outmarrying individuals who have not followed what is assumed to be the path of least resistance. Rather, it is

[26] See, e.g., John Burma, "Research Note on the Measurement of Interracial Marriage," *American Journal of Sociology*, LVII (1951), 249–55. Burma's findings generally agree with ours on exogamous Mexican-American men.

suggested, they go to considerable trouble to upset a normal pattern of close relationships inside a group and to establish new relationships with the larger community. It may be interpreted as alienation or perhaps maturation, but in either case it is assumed that the process takes so much time that the individual does not marry until he is older than his fellows. We cannot accept this idea as a complete explanation of our results, because the foreignness of the spouse seems to weigh heavily in marriage selection and, further, because whether or not the marriage partner belongs to the ethnic community is even more important.

How can we explain why younger Mexican Americans are as likely to marry exogamously as are older persons? It is possible that the younger people are more antitraditionalist in their orientation than the older persons. One piece of data supports this contention. Preliminary results from field surveys show the prescriptive age of marriage for men to be rather high in this population. This would argue that the young are departing from the norms of the community. It is possible also that other studies which found the rate of exogamy to rise with age dealt with groups where the prescriptive age for marriage is low, where those who defer marriage until economically and socially established are antitraditionalist in their lives. One might also suggest that the opportunities for wide contacts and social mobility are much greater among the young. Our cross-sectional study captures persons in different age brackets. Thus, the twenty-one-year-old Mexican American marrying an Anglo in 1963 may have had quite different experiences from the thirty-one-year-old person marrying a Mexican-born bride in the same year. Relationships with the larger Anglo society have changed considerably in the past decade.

IMPLICATIONS

At the outset, we stated that our paper had two purposes. The first was addressed to the probable future of a particular population—the Mexican Americans. We feel that our findings strongly suggest the assimilative potential of the population when external barriers are comparatively low, though this potential has been generally depreciated. We must not, of course, make the mistake of confusing a cross-sectional three-generational study with a longitudinal study; the internal differences in endogamy rates imply that the assimilative potential is related to nativity and rate of advancement in the group, and both of these may shift.

We also feel that this analysis strongly supports the responsiveness of Mexican Americans, along with other ethnic groups, to milieu—that is, to variations in both prejudice and opportunities, which themselves are associated. The importance of the milieu emerges from the comparison of Los Angeles rates of endogamy with those found in previous studies.

More generally, these data can be interpreted as indicating underlying processes which occur in the breakdown of mechanical solidarity in an increasingly open system. Mexican Americans have been unusual in maintaining strong ethnic boundaries, in part because of many isolating mechanisms associated with their initial contact with Anglo populations. These mechanisms include the structural and emotional effects of two wars fought with neighboring Mexico within a hundred years, both of which impinged particularly heavily upon Texas, an area that still contains a large proportion of the nation's Mexican Americans. General "race" prejudice and severe educational and work segregation have been still other factors. The population is also quite segregated by religion (Catholicism is the prevailing faith in a predominantly Protestant region)[27] and by

[27] However, Catholic *practice* is far more equivocal in the Los Angeles area. Less than half of the marriages considered here, for example, were validated by Catholic ceremony. In other regards, the religious practice of this population in the country falls far short of the Catholic norms (Patrick H. McNamara, "The Role of the Roman Catholic Church in the Mexican-American Community," in

residence,[28] both in rural and urban areas. In New Mexico, even more stringent historical and political circumstances have insured little contact with the surrounding Anglo population; the high endogamy found in Albuquerque is not surprising, despite the fact that the "Spanish" population is actually older than the relatively "new" white settlers from the East and North.[29]

These kinds of isolating experiences were historically accompanied by considerable—almost exclusive—reliance on kin as principal sources of emotional and other support and also by considerable similarity in outlook and style of life. However, the extent of this isolation is generally declining—though only slightly in some parts of the Southwest, more substantially in others, like Los Angeles.

As isolation declines and as both upward and horizontal mobility become more common, there come to be many more varieties in style of life. Most important, primary-group relations decline in functional importance and they even cease to be maintained mechanically, that is, by style of life. Thus, the socializing and identity-maintaining structures of the ethnic group—and particularly the family—are structurally weakened. Intrafamily dissimilarity increases.

This kind of interpretation seems applicable in our case especially to the third-generation families living in the relatively open opportunity structure of the type found in Los Angeles, and helps explain their notable propensity toward assimilative marriage (whereas if ethnic loyalty were maintained, the generation of partner would have little relevance). Increased experience in the larger system—especially rewarding experience—decreases the saliency of the ethnic group as the prime source of identity, in turn weakening the control of the primary group over the social relations of its members. Ascriptive identity decreases in salience.

The particular history of this population warrants further exploration on this and other topics; the peculiar blend of minority and ethnic statuses provides an especially fertile ground for the development of hypotheses concerning the combined effects of a variety of social differences. This study, based on the severely limited data available in marriage licenses, appears to call for increased attention to the group not only as a social problem but as a sociological problem of interest.

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Leo Grebler *et al.*, *The Mexican-American People* [New York: Free Press, in press]).

[28] See Moore and Mittelbach, *op. cit.*

[29] Nancie L. Gonzalez, *The Spanish Americans of New Mexico: A Distinctive Heritage* ("Mexican-American Study Project," Advance Report 9 [University of California, Los Angeles, 1967]).

Who Voted for Hitler? A New Look at the Class Basis of Naziism[1]

Karl O'Lessker

ABSTRACT

No agreement yet exists among social scientists as to the sources of naziism's sudden electoral surge in 1930 and 1932. One widely held view, stressing the importance of the "outcast and apathetic," has been sharply challenged by S. M. Lipset, who argues that electoral support for Hitler was essentially a middle-class phenomenon. But on the basis of a new analysis of the voting returns, I conclude that a combination of former non-voters and traditional Rightists gave naziism its first great success, and the bulk of the middle-class vote went to Hitler only after the Nazis had established themselves as the largest non-Marxist party in Germany.

Ever since Hitler's accession to power in 1933, social scientists have attempted to supply an explanation for one of its more puzzling aspects, namely, the enormous surge in naziism's electoral support between 1928 and 1933—a surge which transformed it in one election alone from the ninth-ranking party in the state, with barely 2.6 per cent of the vote, to the second-ranking party, with 18.3 per cent.

Analysis of the election returns has tended to yield two conflicting interpretations. One is that previous non-voters provided Hitler with his greatest accretions of strength in the utterly critical election of September, 1930; that naziism's sudden appeal was not, at least initially, to any one class but, rather, to the disaffected and apolitical from all strata, who flocked to the polls that September in support of National Socialism. And only in subsequent elections did large segments of the middle class follow suit.[2]

The other interpretation identifies naziism instead as essentially a middle-class movement in 1930 as well as later. The most influential recent statement of this position is Seymour Martin Lipset's, in his book *Political Man*.[3] Arguing that naziism made its most spectacular gains at the expense of the non-Catholic middle-class parties, Lipset dismissed as statistically insignificant the over-all increase in voting participation and also discounted the widely held notion that defections from the very conservative Nationalist party (DNVP) contributed importantly to Hitler's success.

[1] I want to express my gratitude to C. Richard Hofstetter, of the Ohio State University, for his many hours of help in devising an appropriate statistical strategy and performing all the computer operations. I am grateful, too, to Phillip M. Gregg, of Indiana University, who counseled me on some especially troublesome technical points, read the draft manuscript, and made a number of useful suggestions.

[2] An outstanding exponent of this view used to be Reinhard Bendix, most notably in his essay, "Social Stratification and Political Power," in R. Bendix and S. M. Lipset (eds.), *Class, Status, and Power* (1st ed.; Glencoe, Ill.: Free Press, 1953). But that essay has been removed from the current (1966) edition. And in a 1959 article by Bendix and Lipset we find the statement, "Lipset has convinced Bendix that the shift of non-voters to the Nazis occurred only in 1933, whereas in the preceding elections middle-class extremists predominated among Nazi supporters" ("On the Social Structure of Western Societies: Some Reflections on Comparative Analysis," *Berkeley Journal of Sociology*, V [Fall, 1959], 12).

[3] Seymour M. Lipset, *Political Man* (New York: Doubleday & Co., 1960). I have used the Doubleday Anchor edition of 1963, to which all citations will refer.

Coupled with this "class analysis of naziism" (Lipset's phrase) is a more general theoretical statement concerning the dynamics of extremist movements in which he declares that "the most outcast and apathetic sections of the population can be won to political action by extremist and authoritarian parties only *after* such parties have become major movements, not while they are in their period of early rise. . . . The political interests of the apathetic . . . can be awakened only by a mass movement which presents a *simple* extremist view of politics."[4]

The central questions I wish to deal with in this paper are these: Can a closer analysis, using different statistical tools, of the election returns provide grounds for choosing between the two conflicting interpretations of the role of previous non-voters in naziism's triumph? And will that conclusion then tend to support or weaken Lipset's theoretical position on the nature of extremist mass movements?

Let us first review the basic data.[5] In the parliamentary elections of May, 1928, Nazis received only 810,127 out of a total of 30,753,247 votes. But at the next election, two years later, the Nazi vote rocketed to 6.4 million, while the total vote rose to just under 35 million. In July, 1932, Hitler made the greatest single electoral gain of his career, capturing 13.7 million out of a total of 36.9 million votes. Then new elections were held in November of the same year, and to the astonishment of practically everybody the Nazi vote *dropped* by just over 2 million, and the total vote fell off by 1.4 million. But Hitler was appointed chancellor in January, 1933, and in the no longer entirely free election that followed in March, his party's vote soared again by 5.5 million, while turnout increased by 3.9 million. There were no more elections in the Weimar Republic.

With this brief review and the data of Table 1 before us, we can proceed to evaluate the rival interpretations. The really crucial election for analysis is that of 1930 because it was then that the Nazis were

TABLE 1

THE VOTE FOR MAJOR GERMAN PARTIES IN REICHSTAG ELECTIONS, 1928-1933*

Party	May, 1928		September, 1930		July, 1932		November, 1932		March, 1933	
	Total Vote	%	Total Vote	%	Total Vote	%	Total Vote	%	Total Vote	%
Right:										
Nazis	810,127	2.6	6,379,672	18.3	13,745,781	37.3	11,737,010	33.1	17,277,180	43.9
Nationalists	4,381,563	14.2	2,457,686	7.0	2,177,414	5.9	3,019,099	8.5	3,136,760	8.0
Middle:										
Non-Catholic	8,465,174	27.5	7,768,072	22.3	1,934,802	5.2	2,100,656	5.85	1,401,247	3.5
Catholic	4,657,796	15.2	5,185,637	14.8	5,782,019	15.7	5,326,067	15.0	5,498,457	14.0
Left:										
Socialists	9,152,979	29.8	8,575,244	24.5	7,959,712	21.6	7,247,956	20.4	7,181,629	18.3
Communists	3,264,793	10.6	4,590,160	13.1	5,282,626	14.3	5,980,102	16.85	4,848,058	12.3
Other	20,815	.1	....	....	....	....	....	....	....	....
Non-voters	10,471,431	24.4	8,026,441	18.0	7,344,481	16.0	8,929,259	19.5	5,342,433	11.2
Total vote	30,753,247	75.6	34,956,471	82.0	36,882,354	84.0	35,471,745	80.5	39,343,331	88.8
Eligible voters	41,224,678	....	42,982,912	....	44,226,835	....	44,401,004	.....	44,685,764	....

* Adapted from Samuel A. Pratt, "The Social Basis of Nazism and Communism in Urban Germany" (M.A. thesis, Department of Sociology, Michigan State University, 1948), pp. 29–30.

[4] *Ibid.*, pp. 150, 151. Italics, here as elsewhere, in the original.

[5] Summary national election returns are available in Samuel A. Pratt, "The Social Basis of Nazism and Communism in Urban Germany" (M.A. thesis, Department of Sociology, Michigan State University, 1948), pp. 29–30. The district (*Wahlkreis*) returns will be found in appropriate volumes published by the *Reichs Statistisches Amt.*

transformed from a noisy band of extremists into the second largest party in the state (and the largest non-Marxist party—a point of some significance, as we shall see). It was then, too, that the correspondence between increase in turnout and increase in the Nazi vote *appears* to be most marked. Addressing himself to this latter problem, Lipset says:

> Geiger, Bendix, and others who concluded that the Nazis derived their early backing from traditional nonvoters based this opinion on the over-all election figures which showed an enormous increase of Nazi votes simultaneous with the sudden participation of over four million previous nonvoters. But when the changes in the rates of nonvoting and of the Nazi vote are broken down by districts, we actually find a small *negative* rank order correlation of −.2 between the per cent increase in the Nazi vote and the increase in the proportion of the eligible electorate voting. More vividly stated, in only five of the electoral districts where the Nazi gain between 1928 and 1930 exceeded their *average* gain for all of Germany was the increase in the size of the electorate also disproportionately high. In twenty-two of the thirty-five national districts there is a negative relationship.[6]

Taken at face value, these findings would seem to be utterly conclusive: New voters simply could not have played the preponderant role in Nazi success that earlier analysts had claimed for them. But can we take these findings at face value? Two important considerations obtrude. The first is the brute fact of 4.2 million new votes cast, plus a net loss among all other parties combined of 1.3 million votes. The second is a difficulty that Lipset in a sense creates for himself. For not only does he argue that new voters accounted in no significant way for the Nazi gain; he also wants to insist, as I have already noted, that the traditional conservative-party (DNVP) supporters contributed less to Hitler's victory than did the non-Catholic middle-class partisans. But here again, inspection of the 1930 election returns renders this a most questionable proposition, for the DNVP lost no fewer than 1.9 million votes, while the non-Catholic middle-class parties suffered a net loss of only 700,000.

But now what of the rank-order correlation that Lipset offered as his most persuasive piece of evidence against the thesis that new voters made a major contribution to Hitler's cause in 1930? There are a number of objections that can be raised against the methods that Lipset employed to study the results of that election. These would include, principally, his use of percentages rather than the actual numerical changes in turnout and in the Nazi vote and his failure to take into consideration changes in the vote for other parties.[7] If we are to get any sort of comprehensive picture of where the Nazi gains came from, surely it would be prudent to use some more powerful, inclusive, and sophisticated statistical technique than simple rank-order

[6] Lipset, *op. cit.*, p. 150.

[7] This point is important enough to warrant a fairly extended explanation. Phillip M. Gregg put the matter to me this way in a memorandum: "When examining change over time, we sometimes find ratios deceptive in that they can produce results which are influenced more by the base or denominator of the calculating formula than by the absolute changes measured by the numerator. For instance, in districts which had a proportionately small (below the mean for all districts) increase in turnout and a large proportionate increase (above the mean for all districts) in Nazi voters, a ratio would indicate a negative relationship even though the absolute change in turnout was substantial and a majority of the new voters went for the Nazis. Conversely, in districts which had proportionately more new voters than new Nazi voters, a ratio would indicate that increased turnout was related to increased *non*-Nazi voting even though a large number of new voters cast their ballots for the Nazis. In other words, ratios would record correlated joint differences only if the proportional increases and decreases in turnout and Nazi voting were related positively in most voting districts. But confirmation of the hypothesis about the importance of previous nonvoters does not require evidence of this sort. It only requires evidence that absolute increases in turnout were related to absolute increases in the number of Nazi voters. For this reason, net changes 1928 to 1930 are more appropriate than ratios for testing the hypothesis at hand."

correlation for two variables only. By using, instead, multiple correlation with stepwise regression, we can take into account changes in the vote for each party (or group of parties) as well as increase in turnout. And by treating the district-by-district change in the Nazi vote as our dependent variable, we should then be able to determine the cumulative effect of *all* independent variables on the Nazi vote.

For this purpose I considered as inde-

TABLE 2

COEFFICIENTS OF MULTIPLE CORRELATION, R-SQUARED, AND INCREASE IN R-SQUARED FOR EACH INDEPENDENT VARIABLE

Independent Variable	R	R-squared	Increase in R-squared
Nationalists..........	.620	.385	.385
Turnout............	.802	.701	.316
Non-Catholic middle.	.965	.932	.231
All others, combined.	.991	.981	.050

pendent variables net numerical changes in the following: turnout, the vote for each of four major parties (Socialist, Communist, Nationalist, and Catholic Center), and the vote for all other middle-class and regional parties taken together—a total of six independent variables. Table 2 displays the results of this analysis.

The following inferences seem warranted by these findings. The most important single source of new Nazi strength in 1930 came from voters who had formerly supported the conservative, ultranationalist DNVP: in statistical terms, fully 38 per cent of the variance in the Nazi vote can be attributed to this source. But a close second in importance, accounting for some 32 per cent of the variance, were the previous non-voters: that group which, in Lipset's analysis, appeared to be wholly unrelated to the group of new Nazi voters. And third in importance—much weaker than the preceding two but by no means negligible—were those who had formerly voted for non-Catholic middle-class parties; these accounted for perhaps 23 per cent of the total variance.[8] Finally, the decrease in the Socialist vote and increases in the Catholic and Communist votes bore no statistically significant relationship to the increase in the Nazi vote.[9]

A different and more striking result is obtained, however, when we calculate the β weights for each independent variable. These work out as shown in Table 3.

Since the β weights, in Blalock's words, "indicate *how much change* in the dependent variable is produced by a standardized change in [each] of the independent variables when the others are controlled,"[10]

TABLE 3

β WEIGHTS FOR SIX INDEPENDENT VARIABLES

Variable	β Weight
Turnout..............	+1.08
Nationalists...........	−0.82
Non-Catholic middle...	−0.60
Socialists.............	−0.39
Communists..........	−0.36
Catholic Center.......	−0.31

[8] As readers trained in statistics are aware, drawing casual inferences from coefficients of correlation, squared or otherwise, is an exceedingly tricky business. To say that our first three independent variables together "explain" 98 per cent of the variance in the Nazi vote is not at all to say that we can take that percentage of 5.57 million (the total Nazi gain), divide it according to the proportion 38:32:23, and end up with precise numbers of votes attributable to each variable. The most we can claim for a multiple correlation analysis in this context is that it *probably* gives us relative magnitudes of importance among the variables and that it is quite certainly a more reliable measure of intervariable relationships than is the simple rank-order correlation that Lipset used.

[9] I should report, however, that the product-moment coefficients of correlation are not insignificant. Between decline in the Socialist vote and increase in the Nazi vote, $r = -.39$; between increase in the Communist vote and increase in the Nazi vote, $r = .38$. A fair interpretation of both relationships, I think, would be that "radicalization of the electorate" (Bendix's phrase) was occurring on both Left and Right with some degree of simultaneity.

[10] Hubert M. Blalock, *Social Statistics* (New York: McGraw-Hill Book Co., 1960), p. 345.

the permissible inference that one can draw from these findings is that increase in turnout was substantially the largest factor in the Nazi vote gain, with decline in the Nationalist vote second and decline in the vote for non-Catholic middle-class parties third. But whichever of the two statistical measures presented here one chooses to regard as the more reliable for this sort of analysis, one can hardly escape the conclusion that it was a combination of new voters and defecting Nationalists that transformed the Nazi party into a true mass movement in September, 1930.

One other point to be noted about this election is that correlations between the increase in turnout and increases in the vote for the Catholic and Communist parties both indicate a very strong relationship: for the former, $r = .64$; for the latter, $r = .63$. And a multiple correlation analysis of the increase in the Communist vote in 1930 (a subject which I shall return to shortly) shows that increase in turnout and decrease in the Social Democratic vote together account for fully 72.3 per cent of that variance.

We turn now to the election of July, 1932. Lipset's statement that "the Nazis gained disproportionately from the ranks of the center and liberal parties rather than from the conservatives"[11] is amply supported by the evidence. The non-Catholic middle-class parties lost a staggering 5.8 million votes—almost 75 per cent of their 1930 total—while the DNVP held its losses to 280,000, and turnout increased by 1.9 million. The Nazi gain of 7.3 million votes can be wholly accounted for by these three factors. (The Catholic Center, meanwhile, continued to hold its own, winning an additional 600,000 votes, while the Communist increase of 700,000 doubtless involved gathering in the Socialist loss of 600,000 and a fraction of the new voters.)

Granted, then, that in this election the Protestant middle-class sector of the population contributed much the largest portion of Hitler's new electoral triumph, the curious reversal of trends that occurred the following November lends further support to the thesis that in 1930 it was a combination of traditional conservatives and previous non-voters that lent naziism its first great impetus. Referring to the figures in Table 1, we note first that turnout in the November election *declined* by 1.4 million, the Nationalist vote increased by 842,000—and the Nazi vote fell off by just over 2 million. The vote for all middle-class parties, Catholic and non-Catholic, remained virtually unchanged. And another 700,000 Socialist votes appear to have been transferred largely to the Communists.

Now if we assume, as the evidence clearly warrants, that Nationalist gains came substantially at the expense of the Nazis, we still have to explain the "disappearance" of some 1.2 million Hitler supporters. Or to put it another way, who were the voters-turned-non-voters in November, 1932? Two possible answers come to mind. One is that these sudden defectors from the electoral arena were primarily non-Catholic middle-class persons who had abandoned their traditional parties in droves the preceding July to vote for Hitler but who in November felt cross-pressures of sufficient intensity to cause them to abstain.[12] The other possible answer, for which there is at least as much evidence in our studies of voting behavior, is that those who stayed away from the polls in November were predominantly those customary non-voters who had been lured to the polls for the first time in 1930 by the incendiary appeals of naziism. This appears to be the more reasonable explanation. For while cross-pressuring can indeed make for electoral abstention, it is not at all clear that cross-pressuring was any more intense in November, 1932, than it had been in July. But not only was the November contest the second full-scale parliamentary election in only five months: *two* presidential elections had taken place within the same calendar year.

[11] Lipset, *op. cit.*, pp. 144–45.

[12] The literature on the effects of cross-pressuring is copious and familiar. One excellent statement is Lipset's, *ibid.*, pp. 211–26.

It would not then be surprising to have found a widespread election weariness among the German people by the time of the November balloting. We should therefore be inclined to suppose that non-voting would have been most prevalent among those members of the population for whom voting had never been customary—precisely those people who, according to the argument of this paper, had helped Hitler to his first great victory two years earlier.

There is not much to add about the election of March, 1933. With Hitler already installed as chancellor, Nazi formations conducted a campaign of unparalleled brutality and intimidation. The total vote increased by 3.9 million, while the Nazi vote increased by 5.5 million over the preceding November. Lipset reports finding a positive rank-order correlation of .6 between the two trends; my own analysis yields a rank-order correlation of .94. For Lipset, the electoral results of 1933 indicate that the Nazi movement had finally attained the status of "a *mass* movement which presents a *simple* extremist view of politics"—the only kind, he insists, that can awaken "the political interests of the apathetic." Then he adds, "The Nazis did not fit this category from 1928 to 1930; they did, however, after 1930."[13]

The flaws in this part of his argument are obvious enough: if indeed naziism became a true mass movement only *after* the 1930 election, why was the increase in turnout less than half as large in 1932 as it had been in 1930? Why did the Nazis benefit so very much more from the influx of new voters in 1930 than they did in 1932? And, finally, why did the Nazis fail so signally in November, 1932, to capitalize on their dramatic victory of five months earlier? Surely *then* they constituted the kind of mass movement that Lipset says can galvanize the normally apathetic; and yet, as we have seen, turnout declined—almost entirely at the expense of the Nazis—and Hitler suffered an electoral setback that nearly swept him into political oblivion.[14]

In my view, these questions can be answered satisfactorily only if one assumes, with Reinhard Bendix, that in Weimar Germany "the radicalization of the electorate originated among the previous non-participants in party politics, who probably came from various social groups, and that the significant support of the totalitarian movement by members of the middle class and of other social groups occurred subsequently in the hope of relief from economic distress and in the desire to gain from backing the victorious movement."[15] This hypothesis would not only explain far better than Lipset's the electoral changes we have been considering; it would help us to account, too, for a phenomenon earlier alluded to—the sharp upsurge in Communist strength from 1928 to 1930. Reference to Table 1 shows that Communist voters increased in 1930 by 1.3 million, while Socialist strength decreased by 578,000. If we assume, as most observers do and as statistical analysis warrants, that the bulk of the votes the Socialists lost was transferred to the Communists, we still have to account for the additional Communist increment of upward of 748,000 votes. Those new Communist voters, as we have seen, must have come from the pool of previous non-voters. And this fact serves as support for Bendix's hypothesis about the "radicalization of the electorate"—a development not confined to radicalization on the Right. Moreover, it helps substantially to explain the virtual collapse of the non-Catholic middle-class parties in July, 1932, for by this time naziism must have seemed to the distraught middle class to be the one best hope of averting a Communist take-over. And finally, the electoral events of Novem-

[13] *Ibid.*, p. 151.

[14] The definitive history of the period is Karl D. Bracher, *Die Auflösung der Weimarer Republik* (Stuttgart: Ring Verlag, 1954). An authoritative account available in English is Erich Eyck, *A History of the Weimar Republic* (Cambridge, Mass.: Harvard University Press, 1962), especially Vol. II.

[15] Bendix, *op. cit.*, p. 605.

ber, 1932, come more clearly into focus: these same middle-class voters stayed largely with Hitler; about two-fifths of the Nationalist defectors to naziism went back to their traditional party allegiance; another 700,000 Socialists, acting out of *their* fear of a Nazi take-over, moved into the Communist ranks; and 1.2 million of the normally apathetic new Nazi voters—by now, no doubt, hopelessly bewildered and thoroughly bored by the seemingly endless succession of elections—simply stayed home.

CONCLUSIONS

We have seen that an indispensable component of Hitler's first great electoral success, in September, 1930, was an influx of new voters into the political arena. The wholesale defection of middle-class voters to naziism did not take place until after the NSDAP had become the second largest party in the state. And if the supposition is correct that this middle-class shift occurred because naziism appeared to offer the soundest bulwark against communism, we might reasonably infer that it would not have taken place at all but for the enormous boost given to German fascism in 1930 by the "outcast and apathetic" and by the traditional Right.

These findings cast serious doubt on the thesis that only an already *mass* extremist movement can enlist the support of the normally apathetic and apolitical. Without attempting here to present a comprehensively documented counterthesis, I would suggest that an extremist, authoritarian movement like naziism or communism may indeed be able to mobilize the politically apathetic while it is still in a relatively early stage of development—*provided* social conditions are such that the extremist appeal will seem relevant to the immediately perceived needs of these persons. Social conditions of the sort I have in mind would probably include widespread economic distress, deterioration of traditional institutions, failure of a constitutional regime to achieve legitimacy, and frustrated nationalistic aspirations.

Those, at any rate, were conditions that prevailed in the Germany of 1930. And a demagogue of genius capitalized on them to his own unspeakable advantage.

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Collective Behavior: The Sense of a Rubric[1]

Dennis Brissett

ABSTRACT

This paper is a discussion of the relationship between dramaturgical social psychology and the study of collective behavior. Initially, the relevance of the traditional conceptualization of collective behavior is examined with reference to the questions of the dramaturgical model. The contributions of the collective behaviorists to dramaturgical social psychology and general sociology are then specified.

There exists among contemporary sociologists a general hesitancy to adopt the term "collective behavior" as a label for a distinct type of social action. Even those authors writing specifically on subjects traditionally labeled "collective behavior"[2] invariably attempt to apologize for their use of the term. The authors of two such treatises,[3] unable to justify its use, therefore adopt other terms. This uneasiness with the term "collective behavior" seems to warrant critical examination to locate its conditions. On this basis, the uneasiness can perhaps be dispelled, or at least defended.

It is commonplace to observe that each investigator of social life approaches that life from a particular frame of reference. This perspective is, for the most part, circumscribed by the vocabulary with which he chooses to speak about that world. An extremely important consequence of such a vocabulary is the manner in which the terminology clusters certain behaviors together—how, in other words, social action is typed. Such classification is basic to inquiry and, in effect, explicitly recognizes that not all behavior is of the same kind and, further, that theory formulated to explain one kind of behavior will not be applicable to another, and that therefore each kind must be studied separately.

One of the problems with vocabularies is, however, that they tend to persist over time. Therefore, classificatory schemes formulated on the basis of earlier perspectives in sociology still have, in some cases, currency in present-day thinking. One such instance is that typology which has as one of its basic classes "collective behavior." I will attempt to show in this paper the irrelevance of such a typology when one is operating out of an increasingly significant school of thought in contemporary social psychology.[4] It is hoped that the consequent uselessness of the rubric "collective

[1] I am grateful to Robert P. Snow, Ramon Oldenburg, and Gregory P. Stone for comments on this manuscript.

[2] There is another use of the term "collective behavior" which involves equating collective behavior with all social behavior or interaction. This formulation and its attendant problems will not be directly examined in this paper. However, Herbert Blumer, in his examination of collective behavior, discusses this use and also articulates a definition of collective behavior in terms of its being a separate division of sociological inquiry (Herbert Blumer, "Collective Behavior," in Alfred McClung Lee [ed.], *Principles of Sociology* [New York: Barnes & Noble, 1955]).

[3] Kurt Lang and Gladys Lang, *Collective Dynamics* (New York: Thomas T. Crowell Co., 1961); and R. Brown, "Mass Phenomena," in G. Lindsey (ed.), *Handbook of Social Psychology* (Cambridge: Addison-Wesley Publishing Co., 1954).

[4] This is in reference to the dramaturgical school of social psychology referred to by Manford Kuhn in his "Major Trends in Symbolic Interaction Theory in the Past Twenty-five Years," *Sociological Quarterly* (Winter, 1964), pp. 61–84.

behavior" will become apparent in the process of this analysis.[5]

THE TRADITIONAL PARADIGM

The classificatory scheme to be examined here seems to rest upon two crucial distinctions formulated by early sociologists: one had the consequence of defining the boundaries of the discipline; the second, the function of distinguishing, within the subject matter of sociology itself, two major kinds of behavior. The first of these distinctions was that concerning "individual behavior" and "group behavior." By making these two forms of action at least analytically distinct from one another, sociologists were able to claim that the latter was worthy of study in its own right. In group life, unique patterns, unique regularities of action were said to be discernible. In fact, Émile Durkheim[6] went so far as to declare that properties of groups were the only true "social facts" and, therefore, the only proper subject matter for sociology. Indeed, it was believed that a study of group life would yield a more complete understanding of human behavior than would a simple preoccupation with individual action. The human being was seen, then, in the sociological perspective, as a member of various groups and not, as some would have it, a spontaneous, autonomous individual.

The second basic distinction developed by the early sociologists involved a contrast between conscious, purposeful, intentional, sometimes rational behavior for which persons could be held responsible and behavior which was seemingly unintentional, without purpose, and for which persons could not be held responsible.[7] Actually, this distinction, like so many in the social sciences, had a very pronounced one-sided character. Only the former element was explicitly delineated. The latter merely "grew" residually. It was a distinction that was probably best articulated by George Herbert Mead[8] in his discussion of the social self. Here Mead explicitly distinguished between behavior that is circumscribed by the expectations of others (the "me") and behavior that is spontaneous, unpredictable, and not so circumscribed (the "I"). The reflective, purposeful, intentional[9] "me" is construed as the social dimension of the self. It is that element of the self that is built upon the expectations and reactions of others. Mead, it appears, attempted to extend this social dimension as an explanation for all behavior but, reaching an analytical impasse, invoked the spontaneous, uncontrollable "I" to account for behaviors not adequately accounted for by the "me."[10] This, it seems, was the tack generally taken by sociologists in the early twentieth century. A social explanation was

[5] In a recent book, and from a perspective much different than will be taken here, Neil Smelser has attempted to solve many of the problems raised by the relative autonomy of the field of collective behavior. He has done this by defining collective behavior so that it "can be classified and analyzed under the same conceptual framework as all social behavior" (Neil Smelser, *Theory of Collective Behavior* [New York: Free Press, 1963], p. 21).

[6] Émile Durkheim, *The Rules of Sociological Method* (Chicago: University of Chicago Press, 1938).

[7] This distinction is often mistakenly construed as one between a rational and an irrational man. However, the fact of the matter is that the actor who is responsible for his behavior may or may not act in a rational manner. The essential point is that some behavior is imputed by others to be affected by the actor's reflection, while other activity is said merely to occur without there being any imputation of thinking to the actor. It is assumed that, when people reflect, they reflect, at least in part, on the social consequences of their acts and that this reflection is in reference to social expectations of some sort. Their behavior, therefore, is construed as normative.

[8] George H. Mead, *Mind, Self, and Society* (Chicago: University of Chicago Press, 1934).

[9] In line with previous thinking, it does not seem necessary or worthwhile to term this aspect of the self "rational," although some authors have seen fit to do so, and then to criticize Mead for his rationalistic bias.

[10] This is not to say that Mead intended to do this but only to recognize that his work may be interpreted in this way. I am well aware of arguments to the contrary.

sought and, if found, was elaborated to its limits. What then could not be explained by way of the social was ascribed to the psychological and biological realm. It was then quite tautologically asserted that statements of regularities made about the one realm of behavior (the social) could not be applied to the others (the biological and psychological) and vice versa. This distinction between social behavior and other kinds of behavior, as articulated by Mead and elaborated by the more culturally oriented sociologists, seems basically one of distinguishing between normative and non-normative kinds of behavior. The distinction is premised on an idea of a decision-making, socially motivated type of man versus an individual seemingly driven by either psychological, biological, or unknown forces.[11] To be normative, in this context, is not necessarily to be conforming to specific norms but, rather, to be oriented toward the expectations of others.

These two distinctions together form the basis for the following paradigm:

	Normative	Non-normative
Individual	Normal	Abnormal
Group	Cultural	Collective

The question arises now as to whether this particular way of classifying human behavior is indeed of any relevance to the dramaturgical model of present-day social psychology.

To begin with, in the current universe of discourse of the dramaturgical social psychologist, the differentiation between individual behavior and group behavior does not have currency as a working distinction. Rather, the principal concern is with the social relationship, the actor and his other(s). The traditional idea of a group, although partially incorporated into the conceptualization of a social relationship, is not of direct relevance. An attempt is seemingly being made to break down the dualism between the individual and the group in much the same manner that Gabriel Tarde[12] once did. The emphasis of dramaturgical social psychology, particularly in the writings of Erving Goffman,[13] is placed on interaction, not on the characteristics (be these individual or group) of the actors. The seeming historical ignorance of equating "group behavior" with "social behavior" is cast out. The methods of dramaturgical social psychology are not concerned with contrasting the psychological individual with the sociological group. The study of groups, both large and small, is in fact usually apportioned to the area of social organization. The behavior of persons

[11] There arises the immediate problem here of considering the deterministic force of social norms. It is a prevalent idea in sociology that the norms of social organization may coerce or influence an individual's behavior to the extent that he is habitually conforming. In the face of this, the distinction here rests upon three notions: (1) that a norm (particularly conformity to a norm) should be looked upon as having a career. At some time in the career of conformity to certain norms, at least in contemporary society, the person probably had the alternative of not conforming. That this conformity then became regularized is of little relevance in this context, although it may be of extreme significance in others. (2) That habitual conformity to some norms is taken as an indication by others that one is being purposeful and intentional and, moreover, is responsible for his behavior. The ire of being unintentional, purposeless, and irresponsible is evoked only when one deviates from particular norms, that is, when one's behavior is ascribed the label of "non-normative." (3) That persons' awareness and/or acknowledgment of norms should not necessarily be construed as an indication of rigorous adherence to those norms.

[12] Gabriel Tarde, *The Power of Imitation* (New York: Henry Holt, 1903); and Gabriel Tarde, *Social Laws* (New York: Macmillan Co., 1899). Tarde recognized that the behavior of individuals was imitative and therefore inherently social. According to Tarde, "inventions" did not come into existence, did not have social consequences, until they were imitated. Tarde insisted that social life was a multiplication process and not, as Durkheim and others would have it, a transformation process. Therefore, it was unnecessary for Tarde to posit a psychological individual and a transforming group process. Behaving individuals were, by definition, social. All significant human behavior was interaction.

[13] Erving Goffman, *Encounters* (Indianapolis: Bobbs-Merrill Co., 1961); and Erving Goffman, *Behavior in Public Places* (New York: Free Press, 1963).

is itself looked upon as a social production, as something that does not and cannot exist outside of that person's encounters with others. Therefore, the conceptualization of man versus the group and the accompanying idea of differing propositions applying to one but not the other, although a valid manner of approaching human behavior, is not considered relevant by the dramaturgical social psychologists.

The second distinction, that of the normative and non-normative, is also a notion that does not have a great deal of currency at the present time. If, as John Dewey[14] said, man "is an active being and that is all there is to be said on that score," then speculation as to the normative conditions antecedent to behavior becomes unnecessary. Dewey's statement, if logically extended, prods social psychologists to examine the consequences of behavior and not to spend time speculating as to the determinants of behavior, whether these determinants be cast in a biological, psychological, or sociological vocabulary. The normative–non-normative distinction, therefore, pales in importance. The image of a conscious, expectation–oriented man is incongruous with the notion that people simply act, and, during and after action, formulate reasons for their activity.

In this light, the matter of people's motivation changes from a context of rationalism to one of rationalizing. Motives are divorced from their traditional aura of causation. The alleged "real" reasons why people are said to act become of little relevance,[15] at least with reference to explaining the behavior of the people to whom they are ascribed. However, the reasons the actors and others ascribe to their activity become of prominent importance in terms of the career of the ongoing interaction. In other words, the avowals and imputations of motives are seen as having definite consequences for the maintenance or non-maintenance of social relationships. In this perspective, motives are seen as words that either justify a certain activity or at least enable the actor or others to feel that they understand the activity.[16] As such, they are

[14] John Dewey, *Human Nature and Conduct* (New York: Our Modern Library, 1930).

[15] We are speaking here, for the most part, of the set of motives that have been assembled by social scientists. Somehow, these motives are thought of as the "true" motives of human behavior. As discussed previously, the species "normative," in all of its varied manifestations, was at one time the primary "true motive" espoused by sociologists. Sociological explanation was couched in a normative vocabulary. The point of the present perspective is that such scientific motives are not necessarily "truer" than the more pedestrian motives and that they are simply the artifact of a relationship between the actor and one particular, and we might add, somewhat isolated, audience (the scientific observer). Because they have definite consequences in the actor-scientist relationship does not make them the only real motives. This seems particularly important in light of the fact that "real" motives are usually, if not always, equated with causes.

Therefore, the fact that sociologists have claimed that individuals orient their actions to the expectations of others should not be taken by itself to mean that such orientation is a universal determinant of human behavior. Rather, such a claim should be seen as arising out of the nature of the sociological pursuit. The characteristic mode of operation in sociology has been a process of remaining objective and distant from the behavior under scrutiny. At such a vantage point, it seems relatively easy to lose one's sense of relativism and to see one's own sociological motives as the true ones and, therefore, as the causes of the behavior. However, given the methodological tack becoming prominent again in social psychology, it seems that a more relativistic approach is possible. Herbert Blumer's dictum that the sociologist "must take the role of the acting unit whose behavior he is studying" (Herbert Blumer, "Society as Symbolic Interaction," in A. Rose [ed.], *Human Behavior and Social Processes* [Boston: Houghton Mifflin Co., 1962]) naturally leads the sociologist to an appreciation of the many "others" to whom the individual actor is related. It leads him, furthermore, to a consideration of the extreme relativity of motivational statements.

This is not to say that scientific motives are of no significance whatsoever. Obviously, they are often incorporated into the behavior of the acting human being and are found quite serviceable. But this is precisely the point—their importance lies in their consequences, not in their supposed reality as antecedents.

[16] This function of motives has long been recognized by a small number of students, particularly

of extreme importance in the handling or mishandling of one's identity. If identity is understood to mean the person as socially situated,[17] the relation of it to motives becomes particularly clear. For instance, Ernest Becker's[18] depiction of clinical depression illustrates that significant deviant identities are often built simply on one's avowal of certain motives, these being, in the case of depression, a "vocabulary of worthlessness." In the more general case, the identity of deviant seems most readily given to persons whose behavior is liable to the ascription of certain kinds of motives.[19] Sometimes, in fact, the inability on the part of the individual to articulate an acceptable motive for his behavior seems to lead to his being placed in the ominous identity of being mentally ill.[20] The point of all this can perhaps best be summarized by paraphrasing a very old truism of sociology: Motives believed to be true, are true only in their consequences.

Thus, it no longer seems helpful to think of one type of behavior as a product of decisions made on the basis of socially current expectations and being, therefore, socially motivated, and another as the consequence of other than normative decisions. This issue is no longer at stake; the distinction, in a manner of speaking, does not exist for the dramaturgical social psychologist.[21]

Therefore, social psychology, as currently practiced by the dramaturgists, discards both distinctions upon which the rubric "collective behavior" is based. In so doing, it seems to render nil the utility of such a term in its vocabulary. However, this is not to declare that the use of the term "collective behavior" and its concomitant conceptualization entails an invalid approach to social behavior. Rather, it is simply to admit that working within the rubric obscures certain considerations that seem particularly relevant to the questions of present-day social psychology. Moreover, this is not to minimize the contributions of those who have labored in the areas of behavior traditionally circumscribed by the rubric "collective behavior." In fact, social psychologists would do well to pay particular attention to the character of the behaviors

by Kenneth Burke, who has spoken brilliantly of the grammar and rhetoric of motives (Kenneth Burke, *A Grammar of Motives* [New York: Prentice-Hall, Inc., 1945]; and Kenneth Burke, *A Rhetoric of Motives* [New York: Prentice-Hall, Inc., 1950]). Quite recently, psychologists have also adopted this very relativistic dimension of motivation. The prodigious amount of work done under the banner of "cognitive dissonance" seems to underscore vividly an image of a rationalizing, rather than a rational, human being.

[17] Gregory P. Stone, "Appearance and the Self," in A. Rose, *op. cit.*, pp. 86–118.

[18] Ernest Becker, *The Revolution in Psychiatry* (Glencoe, Ill.: Free Press, 1964).

[19] Frank Hartung speaks of an "irresistible impulse" in *Crime, Law and Society* (Detroit: Wayne State University Press, 1965); and Donald Cressey of compulsive crime in "Role Theory, Differential Association, and Compulsive Crime," in A. Rose (ed.), *op. cit.*, pp. 443–67.

[20] Thomas Scheff's discussion of "residual rules" is very relevant to this issue (Thomas Scheff, *Being Mentally Ill* [Chicago: Aldine Publishing Co., 1966]).

[21] With the dissolution of this distinction, the issue of "individual responsibility," so important in the contemporary psychiatric-legal debate, takes on a new dimension. The notion of responsibility indeed undergoes a socialization process whereby responsibility is placed in the context of the social relationship, not in the heads or hearts of the actors. Goffman, in advancing the idea of the moral obligation of social relationships (Erving Goffman, *The Presentation of Self in Everyday Life* [Garden City, N.Y.: Doubleday Anchor, 1959]), and Riesman, in talking of the character of other-directed tolerance (David Riesman [with Nathan Glazer and Reuel Denney], *The Lonely Crowd* [Garden City, N.Y.: Doubleday Anchor, 1953]), have isolated very important types of this responsibility. Currently, Ernest Becker (*op. cit.*) and Thomas Szasz (*The Myth of Mental Illness* [New York: Hoeber-Harper, 1961]) seem involved in clarifying this extremely tenuous idea of responsibility. That responsibility, very much like Harry Stack Sullivan's personality, requires an "other" for its reality is illustrated by Thomas Scheff in his discussion of the negotiable character of responsibility (Thomas Scheff, "The Negotiation of Reality: The Process of Assessing Responsibility," unpublished manuscript).

that have been isolated and studied by those interested in the traditional problem of collective behavior, since a number of important dimensions of human activity have been made prominent.[22] Indeed, it might be said that the dramaturgical model of human behavior has incorporated into its perspective many images of human activity unearthed in the study of crowds, masses, audiences, and the like. It is the contributions of the collective behaviorists to which we now turn our attention, first in terms of their specific relation to dramaturgical sociology, and second with reference to their general importance to sociology as a whole.

CONTRIBUTIONS OF THE COLLECTIVE BEHAVIOR TRADITION

It seems, initially, that the dramaturgical model has borrowed from the tradition of collective behavior certain defining elements of its subject matter. Historically, the majority of sociologists have tended to study the static, orderly, enduring forms of human activity and have thereby evolved an explanation of human behavior that is couched in such a context. However, those who have been interested in collective behavior have brought to attention the intermittent, ephemeral, and uncertain character of much that men do. They have pointed to the short-lived panic, the transitory mob, the volatile crowd. In so doing, they have been drawn to those behaviors that fall outside the "structure," the pattern of societal activity. In the process of their concern, they have evolved explanations of behavior that minimize the influence of the social structure. As a consequence, their thinking has been directed to the immediate situation in which "collective behavior" occurs. The concrete physical setting is examined with reference to its implications for human behavior. Furthermore, what occurs in the situation is seen as having consequences for the career of the interaction and is not said to be determined by contingencies removed from the situation.[23] Consequently, the study of behavior careers is given prominence, whether these careers be in the context of an acting crowd, a panic, or the entire process of collective behaving.[24]

This astructural concern with the immediate and the transitory has led the collective behaviorists to isolate forms of interaction that are based on other than cultural[25] kinds of communication, and also to appreciate the importance of emotion in man's behavior. Such concepts as collective excitement, circular reaction, social contagion, incitation, suggestibility, and others, have been advanced to explicitly recognize the acultural and, at the same time, emotional dimension of interaction. In like manner, the interpersonal relation becomes of extreme salience. In the relatively acultural realm of collective behavior, the personal character of much that men do becomes of great importance. The deterministic character of structural relations recedes in influence.

These elements characterizing the study of collective behavior: (1) a concern for the transitory and concrete aspects of social relations, (2) an explicit recognition of "interactional careers," (3) the acultural

[22] This might be due to the "interested observation" of most sociologists in this area. The study of collective behavior, from the time of LeBon, has always had a "Chicago School" flavor, unlike some other area of sociological inquiry.

[23] This often leads to a kind of circular reasoning, the best example being LeBon's logic that characteristics of crowd behavior are at the same time causes of crowd behavior. This problem of tautology only arises, however, when one injects a causal or deterministic bent to the argument.

[24] For an early and valuable statement concerning careers, see the discussion of natural history in Robert Park and Ernest Burgess, *Introduction to the Science of Sociology* (Chicago: University of Chicago Press, 1921).

[25] The word that is traditionally used here is "symbolic." However, the tack taken here is that the distinction between symbols and natural signs is, in this context, spurious. This is because it is believed that the symbolic aspect of an object emerges in communication and that if something takes on meaning (in the form of response) that something is symbolic. It is not, however, in the traditional usage of the term, necessarily cultural.

position, (4) the emphasis on emotion, and (5) the concern with interpersonal relations, seem also to characterize the dramaturgical model of social psychology. For instance, the insistence that social psychologists can study profitably the "situational" and leave well alone the "merely situated"[26] forms of human behavior indicates their concern for the immediate and sometimes transitory situation. That the activity of manipulating props, equipment, and so forth, serves to define situations illustrates the extreme importance of the physical setting. It is, however, in the relatively acultural emphasis on careers in interpersonal relations that the dramaturgical model exhibits its closest affinity to the perspective employed in the study of collective behavior. The dramaturgists define their field of inquiry not in terms of the traditional preoccupation with cultural behavior but in terms of the more appropriate and inclusive notion of meaningful behavior.[27] In so doing, they reject the rhetoric of cultural determinism inherent in culturology. In the dramaturgical sense, to be symbolic is not to be cultural, but to be meaningful. Inasmuch as meaning arises in the responses made to phenomena, their interest is confined to the consequences and not the antecedents of behavior. In their involvement with the consequences, they seem to dwell most on the interpersonal kind of relation.[28] Interpersonal relations become the ascribed locus of most human activity. The development and maintenance of a self is examined, for the most part, in the matrix of a person's interpersonal encounters. Identities established in interpersonal relations are construed, moreover, as having specifiable careers.[29] The relevant episodes and turning points in these careers are described in a context of adult as well as childhood socialization.[30] The element of emotion, affect, becomes again important, as the career of the interpersonal identity seems qualified most readily by mood.[31]

Besides this close affiliation to the dramaturgical model, the study of collective behavior has made numerous contributions to the broader field of sociology. First of all, and most generally, the investigation into collective behavior has led to a reevaluation of the notion of social cohesion. In the characterization of the differing types of human association—the crowd, the public, and the audience—the nature of the social bond is found to differ widely. The manner in which people are held together —how they behave or act in concert— seems, on the basis of this work, decidedly to involve a multiplicity of elements.

On a large scale also, the nature of collective behavior is such that a preoccupa-

[26] Goffman, *Behavior in Public Places.*

[27] Stone, *op. cit.*

[28] Some little evidence that this emphasis on interpersonal relations is a correct one, at least with respect to contemporary society, can be gleaned from the writings of numerous commentators on the modern scene. Particularly David Riesman, in his crude typology of societies and characters in the *Lonely Crowd,* seems to lend support to this view. Riesman's relevance in this context is his insistence that the present other-directed society has a very different character than tradition- and inner-directed types. His point seems to be that modern man is neither pulled strongly by a neat web of cultural values as he was in the tradition-directed era nor is he constrained by the (work) structure to the extent which he was in the society of inner direction. The implication is that the behavior of modern man may be best understood if seen in the context of an other-directed, interpersonalizing round of life. It might be said, in the light of Riesman's categorization, that the dramaturgical model, in this instance, has extended the scope of the perspective of the collective behaviorists. The study of collective behavior emphasized the acultural, astructural at a time when almost all behavior was confined by culture and structure. The dramaturgical model has simply elaborated this perspective to fit the present, where it seems much more behavior is of an acultural, astructural cast.

[29] See, for instance, Erving Goffman, "The Moral Career of the Mental Patient," *Psychiatry,* Vol. XXII (May, 1959); and Howard Becker, *Outsiders: Studies in the Sociology of Deviance* (Glencoe, Ill.: Free Press, 1963).

[30] Anselm Strauss, *Mirrors and Masks* (Glencoe, Ill.: Free Press, 1959).

[31] Stone, *op. cit.*

tion with social change, with emergent forms of behavior, and with the creative has become imminent. This concern with the "new" forms of social life was, until quite recently, almost entirely in the province of collective behavior.

Another area in which the study of collective behavior has conducted inquiry of central importance to general sociology is that of social disorganization. In their involvement with the socially undesirable—the supposedly unconscious in man—and the unconventional, the collective behaviorists have directed a great deal of attention to the prevalence and relevance of values in human behavior. Although a direct analysis of values themselves has not been conducted, the origins and consequences of such have been richly investigated. A perusal of the literature on collective behavior would indeed seem mandatory for anyone about to embark on a thoroughgoing analysis of values.

Closely related to its preoccupation with values, the study of collective behavior seems to have reached the point where a re-examination of what is involved in the notion of goal direction is called for. For instance, many forms of collective behavior are said not to be goal directed. In fact, Turner and Killian,[82] at one point, speak of collective behavior as that which is not directed toward explicit goals. The "expressive crowd," for example, is often held to exist for itself, since no goal outside itself is recognized. However, the objection can be raised that doing something merely for the sake of doing it is most assuredly a goal. If such is admitted, however, the utility of the concept—goal—is appreciably diminished. In any case, this and other such questions raised by the work of the collective behaviorists should be examined, as much of present-day sociology rests on the premise that man is a goal-directed creature.[83]

Still another issue that has become problematic through the study of collective behavior is that of distinguishing between the normal and abnormal in human behavior. Such work as that on *folie à deux*, dancing mania, and other phenomena, pointed to by Ernest Gruenberg[84] in *Explorations in Social Psychiatry*, raises important issues concerning the division between normal and pathological behavior—for one, whether or not sheer prevalence is sufficient to convert an abnormal activity into a normal one.

CONCLUSIONS

In summary then, although the rubric "collective behavior" does not seem to have a useful place in the vocabulary of the dramaturgical social psychologist, it has quite obviously affected the course of both present-day social psychology and sociology in general. Perhaps, however, the principal achievement of those operating under the rubric of "collective behavior" has not been in terms of their direct contributions to either social psychology or sociology. Paradoxically, it lies in what the collective behaviorists, in one sense, did not accomplish. This lack of accomplishment can perhaps best be seen as an inability on their part to appreciate the social nature of individuals, or at least the social nature of the behavior of individuals. Throughout the writings of those interested in collective behavior, the individual is seen as a psychological mechanism. His behavior, at least his collective behavior, is seen as a reflection of his individualistic, socially unadulterated components. Indeed, collective behavior is characterized as a primitive, elementary, autonomously individual, non-

[82] Ralph H. Turner and Lewis M. Killian, *Collective Behavior* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1957).

[83] See H. Schmalenbach, "The Sociological Category of Communion," in Talcott Parsons *et al.*, *Theories of Society* (Glencoe, Ill., Free Press, 1961), pp. 331–47, particularly his discussion of Weber's types of social action.

[84] Ernest Gruenberg, "Socially Shared Psychopathology," in Alexander Leighton, John A. Clausen, and Robert Wilson (eds.), *Explorations in Social Psychiatry* (New York: Basic Books, 1957).

social kind of phenomenon.[35] Throughout the writings, to be social is seemingly equated with being cultural, the acultural realm being conspicuously individualistic. Such a characterization raises the general question, then, of just what or when behavior is social or, perhaps better, sociological. Social psychologists have done much to extend the realm of social to include behaving individuals, but much work in this area remains to be done. Perhaps by directly confronting the argument of collective behavior, social psychologists can better define and elaborate that peculiar circumstance of being social.

[35] At least at certain points in its development. Some kinds of collective behavior, i.e., crowds and masses, seem to retain this character throughout their career; other kinds, such as public and mass movements, seem to lose these characteristics as they develop.

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BOOK REVIEWS

Essays in the Theory of Society. By RALF DAHRENDORF. Stanford, Calif.: Stanford University Press, 1968. Pp. x+300. $8.50.

In his opening essay, "Values and Social Science," Ralf Dahrendorf re-creates the intellectual setting in which Max Weber articulated his position on the separation of science and value judgments. Through this historical perspective, Dahrendorf rescues Weber's perspective from its mummified textbook status, and out of the reinterpretation that follows, he derives the concerns which tie together the ten essays in this volume.

Dahrendorf is in complete agreement with Weber's premise that value judgments disguised as scientific principles can only produce bad sociology. But given its social context and in the context of Weber the man, this premise does not suffice as the principal conclusion of Weber's effort to define the responsibility of the sociologist. Dahrendorf points out that Weber formulated his position in a series of polemics against a leading group of German social scientists who saw their discipline as a political instrument. For these men, social analysis must create social ideals; the analyst's task was to guide society in the "right" direction. Out of his concern with the relationship between his society and his profession at a critical moment in history, Weber sought, in his emphasis on objectivity, to counter the threat to the integrity of sociology in the vision of those he opposed. Weber's own life, in which he united the vocations of science and politics, is ample testimony that he was not espousing the abandonment of moral concerns by the sociologist; at certain points in sociological investigation, such concerns are inappropriate, but for Weber they reappear again in the practice of the sociologist himself.

Given this perspective on Weber, how does one then define the responsibility of the contemporary sociologist? Dahrendorf argues that the sociologist, in whose person the profession of sociology and the realm of the larger society intersect, now must deal with a threat quite different from the one faced by Weber. "It seems to me more important today to warn against the radical separation of science and value judgments than to warn against their commingling" (p. 18). We can now see that "social science and its findings themselves constitute a great moral force" (p. 85). Because of this, "the sociologist as such must always be morally committed if he is to protect himself and others from unintended consequences of his actions. . . . Our responsibility as sociologists does not end when we complete the process of scientific inquiry; indeed, it may begin at that very point. It requires no less than the unceasing examination of the political and moral consequences of our scholarly activity. It commits us, therefore, to professing our value convictions in our writings and in the lecture hall as well" (pp. 17–18). This volume of essays is Dahrendorf's attempt to fulfil this commitment.

The values professed by the author are those of what he calls the perspective of modern liberalism. Dahrendorf articulates and probes the relationships between the sociological and the liberal perspective and the ways in which their moral and political implications intersect and reinforce or contradict each other. He seeks to create a liberalism that is sociologically informed and a sociology whose moral and political implications can be evaluated with respect to the ideas of liberalism.

Basically his liberalism is a familiar one: the ideal of an open, pluralistic society which provides each individual with the opportunity to pursue his own fulfilment. (In his essay on "Liberty and Equality," Dahrendorf usefully reviews some of the problems and ambiguities found in this formulation.) Against this ideal he probes a wide range of sociological subjects, such as structural-functional theory (and conflict or constraint theory as an alternative), social stratification models, and role theory. This latter topic is the subject of the volume's longest essay, "Homo Sociologicus," in which the author writes, "In solving its problems, sociology necessarily takes social roles as its elements of analysis; its subject matter is the structure of social roles. But by reconstructing man as *Homo sociologicus* in this manner, so-

ciology creates for itself once again the moral and philosophical problem of how the artificial man of its theoretical analysis relates to the real man of our everyday experience" (p. 25).

While Dahrendorf makes an important case for the study of these problems, he is less successful in carrying out the task himself, in my opinion. All too often his analysis is abstract and formal; it lacks the very historical depth and social structural focus he accuses others of lacking in their work. I would expect a sociologist thinking about liberalism to address himself to the social structural changes of the last century which have transformed and eroded many classic liberal ideals into bureaucratic pragmatics; Dahrendorf fails to take this historical experience into account when talking about liberalism. Similarly, there is no analysis of our contemporary sociological profession. How can we adequately assess the moral and political consequences of the sociologist's work without a sense of the structural and cultural pressures to which he is responding? Because of these inadequacies, the terms of Dahrendorf's analysis are often thin metaphors with little structural or historical substance behind them.

Inevitably, a comparison of this book with C. Wright Mills's *The Sociological Imagination* (New York, 1959) comes to mind. Both men see conflict rather than equilibrium as the essential state of society; here Dahrendorf is particularly useful in his delineation of the elements and assumptions of such a conflict theory. Most sociologists will also find Dahrendorf's careful, judicious tone preferable to Mills's caustic polemics. Yet I believe Mills's sharp cutting edge is supported by a historical and sociological richness and depth lacking in Dahrendorf's essays, and because of this, Mills's work still poses the more relevant and compelling challenge for the enterprise of sociology.

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Theory and Methods of Social Research. By JOHAN GALTUNG. New York: Columbia University Press, 1967. Pp. 534. $10.00.

Johan Galtung is one of the few creative sociologists of the first rank who is also a gifted applied mathematician and statistician. This book gives his view of where quantitative sociology is and where it ought to be heading. Only one other book in the last decade combines these characteristics of author and purpose—James S. Coleman's *Introduction to Mathematical Sociology* (New York, 1964). Galtung's book has more emphasis on data collection than Coleman's and less on mathematical theories. The reader requires less mathematics to read Galtung—most of his book might well be assigned to all sociology majors; Coleman's should only be read by those who intend to become quantitative sociologists.

Writing a review of a book of such scope and importance is rather like the task of a survey respondent handed a copy of *Das Kapital* and asked to check whether he agrees or disagrees strongly, or simply agrees or disagrees. The arguments one can have in the scope of a review with a major attempt to restructure the field of methodology inevitably take on the character of nit-picking. But let me go straight to the nits.

One argument I have is with the implications of the discussion of levels of measurement on pages 72–76, 209, and 340–41. The notion is widespread in sociology that *in order to use parametric statistical tests,* the operations of addition and multiplication must be scientifically meaningful. But I have seen no place in any derivation where the mathematician says, "Operationally realize the mean and standard deviation." The derivations of parametric tests only make use of the fact that some scientifically produced numbers have a normal distribution (or another known distribution), and say nothing about the scientific meaningfulness of the parameters.

If an ordinal measuring device produces normally distributed responses, parametric tests are applicable. Since all monotonic transformations of an ordinal variable are scientifically legitimate, there often is at least one form of an ordinal scale which creates a family of equal normal contingent distributions. This is what is required for parametric tests. Parametric tests make no assumptions about the level of measurement, only about the distributions of numbers, and these are not as restrictive as we sometimes imagine (cf. p. 519, n. 10). The philosophical or operational paternity of numbers is of no concern to statistical tests. Galtung does not explicitly connect measurement levels with statistical tests, but because the fallacy is common among sociolo-

gists, his failure to disavow it is a bad thing.

A second nit is when statistical tests are applicable. Galtung treats the problem as if the *only* random process in the world were sampling by scientists and that, consequently, statistical tests are only appropriate to reject the hypothesis that findings are due to sampling errors (see his explicit choice, p. 341). But in the theory of errors of measurement, and in industrial quality control, and in many other stochastic situations, we want to reject the alternative ("null") hypothesis that the findings are due to a *constant system of chance causes*. We may well want to know *about a whole universe,* whether its pattern of variation could have been produced by a constant system of chance causes or whether instead there is evidence for some specific systematic, or non-random, pattern. In industrial quality control, for instance, we are often checking the hypothesis that some systematic machine malfunction produced the errors rather than the normal random errors of a process. We do this with a statistical test. Sociological applications of this notion can be found in statistical treatments of panel studies, where the question is posed of whether the changes from one interview to another were random changes in a system in stochastic equilibrium or whether they indicate a trend. Even if the panel were the whole universe, we would want a statistical test to check for trend.

A less important nit in the same general area is Galtung's drift into the assumption that the purpose of statistical tests is to *accept* the alternative (scientific) hypothesis (see pp. 358–88). The only thing statistical tests do is to reject the null hypothesis, as Galtung states on page 342 and then ignores. Whether this rejection supports the theory of the investigator or some theory of "spurious" correlation is not a question for statistical tests as such. It is a question for scientific analysis. But a finding which is very likely due to chance is automatically worthless for science. This is true even though the contrary is also true—that many statistically significant findings are worthless for science. Statistical tests get rid of a lot of worthless garbage, but any issue of the journals will show they do not get rid of all that needs elimination. But we do not criticize garbage disposal just because it does not serve the function of a sewer.

It will be noted that these nits are in areas where there is a long tradition of controversy in sociology, where my position has had a chance to harden. There is a great deal more in Galtung's book—on information concepts as applied to social research, on the relation of theories to data, on the ethics of social research, on the logical delineation of research styles, and many other things. I expect to use his book extensively, as I have used Coleman's, in hardening my position on many topics which have never been so well treated in our literature. Let me recommend especially the gem of a discussion of the ecological fallacy of treating individuals as units, on pages 46–47, and the brilliant discussion of what makes some hypotheses more worth investigating than others, on pages 309–40.

Galtung's English is occasionally quaint, but always clear. Sometimes the proofreading could have been better. The footnotes are in the back, which means that many lazy readers will miss a lot of intelligent comments for the sake of publishing economy.

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The Scientific Community. By WARREN O. HAGSTROM. New York: Basic Books, 1965. Pp. 304. $5.50.

Science and scientists have come under increasing scrutiny on the part of sociologists. In recent years there have appeared a number of studies of scientists, by Robert Merton, Simon Marcson, William Kornhauser, Donald Pelz, Norman Storer, Harriet Zuckerman, Norman Kaplan, and Barney Glazer. These studies have dealt with such issues as the reward and communication systems among scientists; priorities in scientific discovery; the work patterns and productivity of scientists in different organizational settings; and the patterns of professional recognition for scientists. Now, Warren O. Hagstrom in this book undertakes a study of the informal relations among a number of scientists as members of an autonomous community devoted to knowledge production.

The author in this work is concerned with showing that "solidarity of the community and the conformity of its members is secured through intensive socialization and a complementary system of social control." This system of social control, the author maintains, is exer-

cised "primarily through the reward of social recognition for contributions of information." Acceptance by scientists of the goals and techniques of science is accomplished by their interest (a consequence of socialization) in participating in the reciprocal system of recognition. Conformity to the ethos of science is, therefore, obtained by the process of socialization whereby the scientist adheres to the norms inherent in the "information-recognition exchange."

In order to carry out this study of "informal organization of basic science," the author carried out ninety focused interviews with a "sample" of scientists and graduate students in science. The "sample" (technically speaking, this is not a sample but a collection of selected individuals) consists of seventy-six faculty members, eleven graduate students, three "other professionals," and two technicians in eleven different scientific fields and dispersed over five universities. The aggregation was "biased" in order "to include a disproportionate number of eminent men" and was designed for "exploratory purposes and is not random." The eleven graduate students were interviewed in the early stages of the research, but their responses were not included in the tables. (The tables, by the way, have their own difficulties. The errors in Table 8 and the use of percentage differences based on five and six cases are disconcerting, to say the least.)

The author points out that in an exploratory study the type of question to be asked is not always known. It is, therefore, clear that Hagstrom did not always ask all his respondents the same questions and has scattered and atomistic data. This, of course, affects his ability to generalize about and set out any set of systematic findings. It does not dissuade him, however, from asserting that "it seems clear that ability is most easily demonstrated in the formal sciences and the mathematical aspects of the emipirical sciences; perhaps this is why scientists in these areas appear to establish their eminence at an earlier age than scientists in the less theoretical empirical sciences." Notwithstanding this assertion, the footnote to this statement admits: "I do not know of hard data with which to substantiate this assertion." These characteristics of this work are referred to only to make the point that the author is not proposing this book as a serious empirical study with findings generalizable to a universe of scientists. Nevertheless, it is a stimulating and ambitious work which reworks and pulls together many of the findings in the sociology of science.

The study suggests that the threat of anticipation in scientific discovery is of such great importance to scientists that it leads to deviant behavior. Competition for the rewards of recognition leads to plagiarism, theft of ideas, almost paranoid attempts at secrecy, and to early publication of partial results. One of the ways of modifying these deviant tendencies is in teamwork. Teamwork allows the information-recognition exchange and an equal sharing in the recognition given to work. The result of this process is that the scientist protects himself by restricting the access of others to his data; at the same time, the scientist aids himself by aligning himself with others who can help him in his search for recognition. Therefore, a scientist may be restricting access to his data at one time and collaborating with another group at the same time.

Another major conclusion of the author is with respect to anomie in science. "Anomy exists when scientists are highly specialized but when their work is not interdependent; it means that scientists cannot make decisions that will be rewarded with recognition. Anomy is one of the major sources of scientific ritualism and retreatism. It is also a source of conflict within disciplines." The further consequences of conflict may be the structural differentiation of the disciplines.

The author contends that the formal organization of science is subordinate to the informal organization. The decisions of authorities in formal organizations are derived from the informal organization of science. When goal conflict occurs, the decisions of authorities may not be derived from the informal organization, and then such decisions will seem arbitrary and illegitimate.

Finally, the author indicates some of the ways in which science handles disputes. One way is to channel revolutionary theories into the existing organization of the discipline and show how the new theory goes with present theory, and thus forestall open conflict. Revolutionary theories may also suggest new ways of dealing with old problems, or they may give rise to new schools of thought and new bases of social control. The way to resolve conflict, however, is through generational turnover: "Whatever the importance of other

characteristics, age has the overriding importance of deciding the fate of the controversy, for the consensus established among succeeding generations is defined as scientific truth."

The study pays little attention to formal organizations and the consequences of the scientists' membership in them. It emphasizes the informal relationships and the importance of the commitment to norms that give expression to social recognition and competition for priority. The study excludes from consideration such organizational factors as material rewards and competition for position and power. By accepting the statements of scientists at face value, and outside of their organizational context, the author does not inquire into the possibility that the roots of conflict may lie in organizational conflicts that are rationalized as ideological in character. Perhaps this can be the concern and interest of a future study by the author.

The Scientific Community has opened up a rich vein, and it is hoped that the author will continue his energetic mining activities. The footnotes provide a rich bibliographic source replete with useful and perceptive comments.

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Toward a Theory of Minority-Group Relations. By HUBERT M. BLALOCK, JR. New York: John Wiley & Sons, 1967. Pp. x+227. $6.95.

The study of race and ethnic relations, perhaps even more than other substantive areas in sociology, is marked by an overwhelming concern with contemporary problems. Generalizations and formal propositions that are neither bound to the immediate crisis nor part of global theory are less frequent than they might be. Expressing a long-standing dissatisfaction with the theoretical status of the field, the author provides a series of ninety-seven propositions in a manner reminiscent of Robin Williams' earlier effort, *The Reduction of Intergroup Tensions* (New York, 1947).

The framework is generated by integrating empirical studies of race relations with a body of concepts and theories drawn from various parts of sociology and social psychology. The empirical studies deal largely with Negro-white relations, although Jews are included along with occasional references to Brazil and the Union of South Africa. On the conceptual level, attention is paid to the coalition theories of Theodore Caplow and William Gamson; the notion of "power"; Atkinson's approach to motivation theory; and the concept of "status consciousness," which is developed as a subpart of the broader notion of "status concern." The author expands his earlier writings on the influence of population composition. In addition, he makes a strong argument for multiplicative models as opposed to an additive approach. Considerable emphasis is on a "flexibility principle" for dealing with goals and decisions about behavior. As one might expect, there is a keen awareness of the pitfalls inherent in theories based on untestable propositions or in conflicting theories that have no empirical resolution. However, some of the concepts are ambiguous, with their operational delineations not always apparent.

The propositions are worthwhile. They may provide researchers with an opportunity to place their findings into a broader and more generalized set of propositions. In addition, the propositions themselves often suggest future research efforts. It should be kept in mind, however, that many facets of racial and ethnic relations are not readily covered by this set of propositions. In this regard, the book is a fragment of the entire cloth—but a useful fragment nevertheless. It does provide a fine illustration of how research findings and conceptual ideas can be incorporated into a single unit. Whether these propositions themselves will prove of value is, of course, a question which will be answered only after scholars employ them in their research. There are bones one could pick about specific ones, but this is only natural in a monograph which includes ninety-seven propositions.

The book will prove of interest to a wide audience. The propositions, if valid, are relevant to many other contexts besides race relations. The author's restatement of coalition formation theory, his conceptualization of power, and the elaboration of various social psychological mechanisms go far beyond racially based forms of organization or social interaction. In this sense, the book is less of an attempt toward a theory of race and ethnic relations per se and more of a codification of various principles based on their application to

the racial and ethnic scene. As such, the monograph mainly ignores or takes for granted dimensions which appear to be distinctive or unique to race and ethnic relations. But it is still a solid contribution toward placing some facets of race and ethnic relations into a broader context of sociological and social psychological theory.

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Political Mobilization: A Sociological Analysis of Methods and Concepts. By J. P. NETTL. New York: Basic Books, 1967. $8.50.

Social scientists' efforts to come to intellectual grips with the developing areas have, to a striking degree, consisted of attempts at linguistic and conceptual innovation. This tendency is the product of several mutually reinforcing factors. The newness and unfamiliarity of the phenomena have generated a fairly widespread belief that only a new, and perhaps unfamiliar, language will be adequate for analytical purposes. Moreover, the social sciences' general failure to keep up with the natural sciences in achieving dramatic theoretical breakthroughs has created a sense that only a radical improvement in the rigor and precision of social theory will enable sociologists and political scientists to maintain pace with their colleagues across campus. In this view, a major problem with existing words and concepts is their vagueness and imprecision. As a result, common usage must give way to a language which, if somewhat arcane, is validated by its greater analytical cogency.

These views are held by J. P. Nettl in the Introduction to *Political Mobilization: A Sociological Analysis of Methods and Concepts*, and the reader is cautioned that a new vocabulary is about to be introduced, a part of which may be somewhat difficult to understand. And, in the course of the volume, the reader is invited to familiarize himself with a set of abstruse concepts, such as "stalactite" and "stalagmite" mobilization or "cleavage identification" and with such terms as "porous specificities," as well as to accept somewhat unorthodox definitions, for such long-familiar terms as "constitutional" and "elitist." This is justified on the grounds that "much of the discussion deals with unfamiliar things like systems [?] which need a whole new structure of terms and concepts" (p. 24). Now this is a perfectly valid and broadly supported (though hotly debated) position. Nettl, however, feels compelled to become highly critical of those who prefer communicative prose. British academics, in particular, are accused of a belief, bordering on ideology, that "the complexities of society can be adjusted to the procrustean demands of that mythical beast, the intelligent general reader" (p. 23).

This position is not only factually erroneous but misses the most important point around which the debate about the use of language in contemporary social science revolves. The insistence of a large number of both British and American academics upon communicative language is not primarily based upon a populistic belief that social theory should be understood by the general reader. Rather, it is based upon a conviction that no new theoretical insight has yet been stated in sociological jargon which could not have been phrased with equal if not greater clarity and illumination in a more easily understood manner. In its extreme form, the invention of new terms and concepts has become a substitute for real theoretical imagination and originality and has resulted simply in the repetition of old ideas in a different language. For this reason, the most important, and in many respects most difficult, task confronting the reviewer of a book which places a heavy emphasis on terminological inventiveness is to determine exactly what is being said.

Any book suffers in being summarized. One whose primary purpose is nothing less than "to find means of understanding the present state of political societies in the world" (p. 19) must necessarily suffer more than most. With this qualification, it is possible to offer the following two-paragraph précis of *Political Mobilization.*

The most important functions of the political subsystem of any society are interest articulation and authority legitimation. Effective and continuous performance of these functions does not pose any special problem for most developed or Western industrial societies. For, although there are significant discernible variations in the nature of Western political systems (some are "constitutional" while others are "elitist"), the populations of practically all have been mobilized into fairly similar but

quite complex patterns of cleavage. Five distinguishable and cross-cutting bases of social conflict can be identified: religion, state policy, social class, region, and ancestral identity (see p. 128). Because the existence of certain universal attitudes, such as national loyalty and agreement on political procedures, can be taken more or less for granted, these conflicts can be given open expression without seriously jeopardizing the stability and integration of the polity.

The state of the developing areas is altogether different. First, these nations tend to exhibit only a single basis of social cleavage—communal and ethnic differentiation. Primordial groups attachments of this sort are highly explosive if allowed to express themselves politically. Second, because most of the countries in this category are relatively new and because their political institutions were inherited from former colonial powers, rather than being the product of indigenous historical change, universal attitudes of national loyalty and agreement on political procedures barely exist. Representative electoral arrangements which, in the West, serve to buttress and enhance the legitimacy of the state by allowing opposed interests to compete, in the developing areas can lead to political breakdown. This compels political leadership to seek to penetrate society (stalactite mobilization) and to infuse its members with universalist perspectives.

Here, stripped of linguistic adornments, is the essential outline of Nettl's argument. Stated in this form, its most conspicuous quality is a high degree of familiarity. For this analysis is directly in the tradition of Western pluralist thought. Despite his expressed indebtedness to functionalism—and the "Acknowledgements" of *Political Mobilization* are profuse with thanks to Parsons, Easton, Deutsch, and Almond—Nettl is the intellectual heir of a far different intellectual heritage, one which, ironically, has certain of its roots among British academics (Maitland and Laski). Nettl's failure to acknowledge or recognize the great importance of the pluralist tradition in his own thinking provides the key to certain of the principal theoretical shortcomings of his book.

For example, by concealing an essentially pluralistic analysis within a Parsonian framework, Nettl has omitted to include the most important feature of pluralist thought, its distinctive approach to the problem of political stability and societal integration. Beginning with the premise of multiple cross-cutting cleavages, the pluralists employed the notion of segmented group attachments as the point of departure for their explanation of the nonviolent character of political conflict in Western parliamentary systems. Their explanation was at least consistent with the basic terms of the theory. In *Political Mobilization*, Nettl refers, instead, to the presence of certain "universalist perspectives" which tend to "submerge" historical cleavages. Because the origin and exact role of these universalist perspectives are never adequately explained, the argument is not only unclear but tends to take on a highly tautological quality. The analysis comes dangerously close to saying only that, when individuals or groups do not engage in violent disruption of society, this is because they are loyal to it and to its rules. The discussion of the developing areas is simply the opposite side of the coin: representative electoral arrangements which afford a high degree of popular participation in the political process are likely to lead to violence and instability when those who participate lack an overriding sense of loyalty to the system. Thus, in confusing functionalism and pluralism, Nettl fails to make a persuasive case for either.

To what extent the shortcomings of Nettl's book are the product of a preoccupation with terminological and conceptual inventiveness is not altogether clear. Certainly this helps explain the linguistic fusion of pluralist and functionalist thought, two otherwise quite distinct theoretical traditions. It may also account for Nettl's failure to identify the extremely important position of pluralism in his own analysis. In any case, the over-all result is that *Political Mobilization* will do very little to weaken the conviction of those who argue that sociological jargon does not represent a theoretical improvement over lucid prose.

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Party Democracy: Politics in an Italian Socialist Federation. By SAMUEL H. BARNES. New Haven, Conn.: Yale University Press, 1967. Pp. vi+279. $6.50.

The Working Class Tories. By ERIC A. NORDLINGER. Berkeley and Los Angeles: University of California Press, 1967. Pp. 269. $8.50.

Revolutionary Politics and the Cuban Working Class. By MAURICE ZEITLIN. Princeton, N.J.: Princeton University Press, 1967. Pp. vi+306. $8.50.

The simultaneous appearance of three books that attack an approximately equivalent set of problems with similar methods and a roughly comparable array of concepts is still a rare enough event in the social sciences to merit some celebration. The volumes reviewed here all deal primarily with urban, manual workers —in two cases, with national samplings (Cuba and Great Britain); in the third, with members of the Italian Socialist Party in a Tuscan province. All the studies were carried out about the same time (1962–63) and build essentially on survey data, though the three argue skilfully from subjective survey responses to contextual historical and institutional dimensions. All address a long-standing set of issues with respect to working-class or mass politics, problems that one of the authors (Samuel H. Barnes) has elsewhere[1] succinctly formulated as the duality in Western political thought between respect for the individuality of persons at all social levels and fear of the mass as political actors. These preoccupations have bedeviled not only vacillating liberals but also revolutionaries perplexed by the indifference of the oppressed whom they sought to free. Interestingly enough, all three authors, though dealing with such a broad range of political contexts and cultures, and each finding their worker respondents to be essentially acquiescent if not passive politically, nevertheless come to optimistic conclusions about the worker's role in politics, whether revolutionary or reformist.

Zeitlin finds, on the basis of a complex scale including projective, forced choice, and action criteria that a sizable majority of industrial workers in Cuba support the revolution. He seeks to substantiate his belief that the Cuban working class was, at the end of the 1950's, ready for revolution in the sense that it had a long-standing acquaintance with socialist ideologies of revolution, had respect for prerevolutionary Communist union leaders, and was therefore predisposed to appreciate the quality of leadership it has received within the revolution. Income deprivation and unemployment figure importantly here as motivating experiences, but for Zeitlin the most decisive condition—strongly related to the predominance of former sugar workers among supporters of the regime—was the obvious incapacity of the economic system to give more than marginal employment to very large numbers of working people. This helps to explain why even a majority of what might have been regarded as a labor elite in the prerevolutionary framework presently support the regime. More interestingly, Zeitlin's data show that the most active and supportive of the revolution are also those most inclined to be tolerant of counterrevolutionary dissent. Some valuable energies that might have gone toward further elucidation of this important finding are dissipated in an overextended polemic against the thesis of working-class authoritarianism. Given the fact that Zeitlin himself affirms that in Cuba (1) there is freedom to complain but not for organized dissent and (2) that there are no legal rights for dissenters but only the generosity of the revolution as a guaranty of immunity, the degree to which libertarian worker opinions mean genuine liberty in the revolution remains unclear. As with the question of non-support of the revolution, which also gets small attention, the impulse to counter external criticism of the revolution seems to distract attention from issues vital to the business of the revolution itself. What are the particularities of the Cuban context that account for these high levels of tolerance among individuals of modest education in a crisis situation? What is the significance of the apparent concentration of dissidence among the generation following immediately upon that of the revolutionary leadership? Nevertheless, Zeitlin succeeds in conveying, through his own sympathetic interpretations and liberal quotations from the interviews, a compelling sense of the quality of human experience and aspirations bound up in the Cuban effort.

The thesis implied in the Zeitlin book—that authoritarian structures may accommodate and promote democratic attitudes and behavior—is developed in some detail by Barnes for the Italian case. In his view, the

[1] Barnes, "Leadership Style and Political Competence," in Lewis J. Edinger (ed.), *Political Leadership in Industrialized Societies* (New York: John Wiley & Sons, 1967), p. 59.

strong commitment to democratic values and procedures of Italian Socialist leaders stumbles against the political incompetence and passivity of their followers. Democracy becomes a goal and not a process. On these grounds, Barnes finds that in many countries ". . . the Communist system for mobilizing the politically incompetent is well adapted to the human resources with which politicians must work" (p. 248), though perhaps not on a long-term basis. Concerning the Italian case, most of the facts that Barnes regards as compensatory in a democratizing sense for rank-and-file Socialists would seem equally available to Communists. Chief among these he counts the inability of leaders to control the flow of communications to members and the freedom of members "to vote with their feet" (p. 6). On the question of the openness of elites at the local level, the amount of active recruitment from below, and the extent of internal factionalism (all potentially democratizing factors also at work among Socialists), no direct comparisons are drawn with the Communist party. Whatever the facts may be, the author concludes that ostensibly authoritarian structures may indeed promote democracy. Lower-class Italians have more alternatives for party and union affiliation than their U.S. peers. Thus, despite the low competence of the mass, which functions as a structural constraint on democracy within worker-based organizations, Barnes finds in the Italian left more encouraging signs for democracy (defined by the author alternatively in terms of participation in decisions or the availability of alternatives) than in the "politics of material abundance, consensus and technology" (p. 253) that he sees taking shape elsewhere.

The idea cautiously put forward by Zeitlin and Barnes is elevated in Nordlinger's *The Working Class Tories* to a basic postulate of democracy. Nordlinger proposes a theory of stable, effective, authentic democracy based on a single variable—attitudes toward political authority (p. 210). Democracy in this view is maximized by a dualistic orientation (directive and acquiescent) toward leadership or authority. British elitism, firmly supported by a generalized political culture respectful of order and hierarchy, finds its authenticity in its stable ability to engender voluntary submission. The diffuse appetite for strong leaders able to make unpopular choices and whose worthiness is explicitly perceived to flow from elitist family ties and schooling is clearly established in Nordlinger's data both for Tory and Labor voting workers. Since "English democracy must be accorded the highest ranking in terms of its stability, authenticity, and decisional efficiency," (p. 210), the characteristic acquiescence to authority of English working-class political culture must be seen as a possible essential constituent of such democracies. Its absence in the French case is pinpointed as the critical flaw accounting for the instability of democracy there. However, Nordlinger finally concludes rather lamely that his theory of generalized acquiescence with very selective efforts at directiveness by followers may be applicable only to *democratic* systems (p. 250), that is, those having appropriate communications and institutional infrastructures.

Thus, in the end, all three authors wind up relying on a combination of enlightened leadership, the long-term effects of further education and participation, and the force of generalized institutional changes difficult to control entirely from the top as the principal levers of political advancement for workers. It is interesting that it is the authors writing on the revolutionary and leftist worker organizations who take the most pains to document the existence of these paths to further democratic advance (even though the term "democracy" does not even figure in Zeitlin's Index). It is for the British working class, which on balance emerges as the most passive and directionless, that the future is most uncritically taken to be assured.

These quick sketches do not convey the complexity of the argument developed in each volume or the extent to which all build on new data and a common framework for analyzing political subcultures, modes of participation, and paths of socialization and recruitment into political activity for workers. Zeitlin's sample is the most modest, and though he is careful to provide all the figures required to give the careful reader a chance to make up his own mind, he reveals nothing about the guidelines he himself used to decide what part of his findings to take seriously. In general, as with Nordlinger, who adopts the same practice, this presents no problem. But since Zeitlin strains harder to check out more in-

tricate relationships in the data, he is drawn occasionally into overextending himself on the basis of what seem to this writer to be precarious figures. Barnes is the most systematic in this connection.

Zeitlin understandably feels called upon to establish that the conditions for interviewing in his study were not such as to call his results into question. While there will no doubt be many who will reject his view, the constraints operative in his interviewing do not appear to have been greater than is normal when workers are interviewed in their place of work. It is worth noting that two of these important volumes are the result of dissertation research.

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Africa in Social Change. By PETER C. LLOYD. Baltimore: Penguin Books, 1967. Pp. 363. $1.65.

The Best of Both Worlds: A Challenge on Development Policies in Africa. By GUY HUNTER. London: Oxford University Press, 1967. Pp. vii+132. 16*s*.

Peter Lloyd, a first-rate British sociologist with fifteen years of experience in Africa, has written a book on social change which deserves a wide audience among American social scientists and the public at large. There are several basic strengths to this volume. Its orientation is sociological rather than "area studies," and thus the rich empirical material brought to bear is geared toward fundamental theoretical questions. Second, there is an important issue that ties together the diverse discussions of urban life, elites, kinship structures, political parties, social movements, etc., namely, to what extent and in what senses can it be said that traditional themes and social forms are persisting in the face of modernization? Third, some still too widespread fallacies are exploded. For example, Lloyd effectively convinces us that social change is not an "either-or" type of question where participants opt either for "modernization" or "traditionalism"; extended kinship obligations expressed financially do not necessarily abort individual economic ambition and other change-oriented motives; modernization does not necessarily imply an experience of "liberation" for African women from the "constraints" of traditional life; adaptation cannot be discussed within the context of a simplistic dualism between "traditional" and "modern," since societies in west Africa alone vary at least as much as any in Europe between Ireland and the Urals. Finally, there are several excellent discussions on specific political problems; two of the most impressive for me were the roots of the one-party state phenomenon and the ethnic issue.

I was disappointed only in two aspects of the book. First, there is something superficial about the specifically theoretical discussions. In evaluating this, it was hard to determine whether this was due to the author or to the state of theory in social change. What bothered me was the apparent thinness of the functional and the conflict models as they are applied to the material brought to bear by Lloyd. What is going on in Africa now is a type of change which is so fluid that neither the static functional models nor the conflict approach, with its assumption of definable interests, seem valid. Rather, what is apparently needed is a focus upon the social *re*construction of all meanings, and the only approach capable of handling this today would seem to be what is known in this country as ethnomethodology. Some British analysts, notably Clyde Mitchell, are tending in this direction with their microsociological studies of "socal fields," emergent "social categories," and related dynamics of social distance operative in the creation of new groups and redefinitions of existing ones.

My other reservation has to do with Lloyd's lack of theoretical focus on some of the main issues in development theory. In view of his experience and material, one would have liked to know his position concerning the notions of social and economic "dualism" in development analyses, concerning Marxian historicist analyses of imperialism and related distinctions between "traditional" and "underdeveloped" societies, and concerning Communist methods for the creation of developmental infrastructures in rural areas. Finally, the Appendix, with its country-by-country statistical synopses, should not have been presented without some serious warnings regarding the misleading conclusions than can be drawn from

official statistics. Indeed, how not to read such appendixes could usefully have formed a concluding chapter capable of introducing many methodological and theoretical questions in development analysis. But despite these minor caveats, I enthusiastically recommend this volume to colleagues.

Indeed this book can usefully be read in conjunction with Guy Hunter's essays on development. In their Introduction, the directors of the Institute for Race Relations in London, under whose auspices this book was written, sound almost apologetic for releasing these preliminary discussions of what is a larger project. They need not be. This slim volume is one of the most exciting and provocative contributions to development literature I have seen for a long time. There are, aside from an introductory and concluding chapter, four basic issues covered in four separate essays: economic growth and employment, the rural economy, institutions, and education. In these few pages, Hunter lays waste much of conventional wisdom in development thinking and foreign aid policies. His strongest contributions have to do with the following topics: the problem of rural infrastructure versus the self-fulfilling prophecy dynamics of the dual-economy approach; necessary versus unnecessary overhead costs in development; economic abstractions based on unrealistic aggregate data; and specific areas for needed experimentation, such as separating large-scale technology from useful scientific principles embodied in it but needing translation into simpler forms for adaptation to less complex social organization.

The discussion is well organized and has a hard-hitting direct quality about it, as exemplified by Hunter's comments on foreign aid and patterns of world trade. Obviously this is not an attempt at a definitive treatise; for example, the discussion of macro-economic data is not matched by analyses of macro-theory in either economics or sociology. It is, however, a commentary by a practical man (in the best sense) with wide experience—a commentary that raises some rather uncomfortable problems for those who would like to write such treatises.

MANFRED STANLEY

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The Fortunate Few: A Study of Secondary Schools and Students in the Ivory Coast. By REMI CLIGNET and PHILIP FOSTER. Evanston Ill.: Northwestern University Press, 1966. Pp. viii+242. $8.50.

This perceptive study focuses on the social background, recruitment, occupational aspirations, and role expectations of African students in the secondary school system of the Ivory Coast. In addition, attention is given to the career patterns of former students.

The analysis is based on carefully designed interviews of 2,176 students, representing 80 per cent of the 2,712 attending the various categories of secondary schools during 1963. Comments on career patterns of former students are based on questionnaire responses from 419 enrolled in secondary schools during the period 1958–62. The authors note the inadequacy of this latter sample and the incomplete character of data on career patterns.

Aside from analysis of material noted above, the study provides an illuminating account of the background, context, and role of secondary education in the development of a somewhat typical emerging country.

Especially important is the consideration of access among the various groupings in the Ivory Coast to the limited facilities for secondary education—the most important resource in the country for elite formation. The authors contend that "despite limited occupational outlets and the small size of the secondary schools in most African countries, present-day elites are drawn from a very broad base" (p. 202). They note that in the Ivory Coast more than 70 per cent of the "Fortunate Few" come from the less advanced peoples; about the same ratio are children of farmers. The argument, however, is not the rates of access for all subgroups are equal but that candidates are chosen on the basis of their verifiable qualifications—not their social category. Hence the system is open regarding talent recruitment. The authors recognize that this relatively open system may be eroded by future developments.

Finally, the writers' reactions to the issue of "Africanization" of schools is of particular relevance. In their view, "The school in Africa is inevitably an agent of change" (p. 201). Its aim is not to conserve tradition but to expedite transformations. From the perspectives of this

study, therefore, the prescribed role of the "Fortunate Few" is to facilitate the entry of their country into the modern world.

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The Marginal Situation: A Sociological Study of a Coloured Group. By F. H. Dickie-Clark. London: Routledge & Kegan Paul; New York: Humanities Press, 1966. Pp. ix+226. $6.75.

There are about 25,000 Durban coloreds, constituting less than 4 per cent of the population of that South African city. The book faces the common dilemma of how to combine the description of a field situation, the presentation of the results of study, and the exploration of theoretical issues raised. It begins with the latter, proceeding thence to the two former. This involves considerable repetition, since conclusions are anticipated and then restated at the end. The history of the key concepts involved is traced with a thoroughness that borders on the tedious. In the later chapters, where the marginal situation of the coloreds is traced in detail with respect to politics, associational participation, interstratum relations, and internal responses, the same basic points about the position of the coloreds are made again and again.

The field research consisted of a house-to-house survey followed by sample interviews, conducted during 1955 in a housing estate which included the largest single concentration of coloreds in Durban. Participant random observation, attendance at meetings, parties, and other functions, and informal contacts with colored friends continued in subsequent years.

The author argues convincingly for subsuming the older association of marginality with culture conflict into the broader category of hierarchical situations, in which the strata are divided by barriers which are nonetheless to some extent surmountable. Marginality arises from "inconsistency in the ranking of an individual or stratum in any of the matters falling within the scope of the hierarchy" (p. 39). There is no culture conflict in the case of the coloreds. The essence of their situation is that they are completely white in culture and thus culturally equal, but are "excluded from full participation in the social dimension of the White social system and thus their ranking in these matters is inconsistently lower" (p. 96).

During the last few decades, the coloreds have been progressively deprived of nearly all their former privileges, squeezed out of white schools, housing, transport, jobs, sports, and parliamentary and local franchise. They are left in the pathetic situation of despising their African and Indian companions in discrimination and futilely insisting upon a tenuous white ancestry whose concomitants they are denied. They are constantly led into contradictions which are cruel in their absurdity, as when they rejected increased transport services with Africans and Indians as compromising, although in fact many of them were forced by poverty to patronize African and Indian buses because their fares were lower.

The quantitative results of the study, especially those of the author's psychological colleague J. W. Mann, are important, if largely negative. Psychological marginality (as defined) has little influence upon social behavior, and the marginal situation itself is revealed as much more important. Any facile association of the one with the other is exploded. It had been expected that a white-like appearance would be highly relevant to the questions of psychological marginality and pro-white bias, but no direct association was found between these factors. It had also been supposed that extreme psychological marginality would occur when white-like individuals rejected the perceived barrier between themselves and whites and expressed a preference for whites, or when individuals of definitely colored appearance rejected the barrier and found it difficult to choose between whites and coloreds. In fact, two-thirds of those in the sample with very high marginality scored did fall into these two categories. But one wonders whether these categories do not include quite a high proportion of coloreds anyway.

The coloreds are a somewhat unusual group in that they were literally brought into existence by racist action and have their origins as a group solely within South Africa. Those of Durban derive mainly from Mauritius and St. Helena, but South African coloreds in general are an extraordinary mixture of European, Bushman, Hottentot, Malay, and slaves of various origin both African and Indian. Their

situation is also perhaps unusual in the extent to which it is a product of officially imposed segregation. However, the qualitative analysis of their marginal situation adds valuable refinement and precision to the sociological concept of marginality and hence makes a significant contribution to the theory of stratification.

AIDAN SOUTHALL
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The Two Variants in Caribbean Race Relations: A Contribution to the Sociology of Segmented Societies. By H. HOETINK. Translated by EVA M. HOOYKAAS. London: Oxford University Press (for the Institute of Race Relations), 1967. Pp. 197. $7.00.

Professor H. Hoetink has a remarkable ability to get to the basic problems of one of the most complex issues in current social science, that of race relations. While historians, anthropologists, and sociologists have been compiling a vast amount of data on slavery and race relations between Negroes and whites in the Americas, they have failed to synthesize their material or to create a coherent set of models which can be tested cross-culturally. The aim of Hoetink's work is to begin to ask the fundamental questions of causation and to attempt to focus on an agreed set of variables which can be tested.

Accepting the thesis of basic differences in integration between Iberian and non-Iberian societies, he nevertheless finds current causal explanations, primarily religious difference and prior experience with alien creatures, inadequate. To get to a more realistic appraisal of causal factors, he holds that we must carefully divide race relations into two main categories: that of "inter-personal but non-intimate contact between groups" and "intimate personal relations" in which the degree of acceptance of "social equality" is crucial (p. 21). By social equality he means the "willingness to enter into sexual relations, incurring social acceptance, with a member of a different racial group"—in short, marriage and social acceptance as opposed to concubinage.

In reviewing the various European West Indian islands he finds that, while all accept a tripartite color scheme of whites, coloreds (or mulattoes), and Negroes, as distinct from the unique U.S. two-part color system, this is not a crucial difference. Much more important is the fact that there is a sharp "difference in frequency of intimate social relations between the North-West European variant [i.e., the British, French, and Dutch West Indies] and the Iberian one [including Brazil]" (p. 49). His excellent survey of current race relations finds that only the Spanish and Portuguese accept to a crucial minimum degree "biological-cum-social mingling," between mulattoes and whites.

What is the cause for this fundamental difference? For Hoetink, the key determinate is what he calls the "somatic norm image." This he defines as "the complex of physical (somatic) characteristics which are accepted by a group as its norm and ideal" (p. 120). Each group in a multiracial society comes equipped with its own conceptions of physical self among its cultural traits. This conception is developed "only [in] continued social contact between groups with different somatic norm images" (p. 122).

These concepts of self-definition are part of the package of the homogeneous cultures of each of the groups, or segments, which go to make up segmented societies. Here Hoetink takes off from the original theories developed by Furnivall and recently by M. G. Smith, and holds that such segmented societies tend toward homogeneity of the two segments through the adoption of a given set of common cultural ideas, one of which will be an agreed somatic norm image. Once the lower segment adopts as its own the views of the dominant segment as to the ideal physical traits to be accepted by all, and this the author feels always occurs from the dominant to the subordinate and never the other way around, then you have a "race problem." He points to the Haitian example to show that "once the adoption of the dominant somatic norm image is a fact, the society will continue to have a 'race problem' even if the dominant segment is eliminated" (p. 135).

Then what differentiates the northwest European from the Iberian segmented societies in this respect? Both, he agrees, do have the "white" norm as the ideal, and both denigrate Negro physical characteristics. Because of this similarity, "one would expect the same degree of rejection of coloreds [i.e., mulattoes] as marriage partners among members of both white groups. That this is not so should in my

opinion be ascribed to the fact that the white somatic norm image is not wholly identical in the two variants. The somatic norm image of the Latin American whites is 'darker' than that of the North-West European American, so much so that certain somatic categories of the coloured group [i.e., mulattoes] may be regarded by the Latin American (Iberian) whites as white" (p. 167), In short, it is the darker conception of the dominant white image which has allowed for the acceptance of a very significant number of mulattoes into the white category in the Iberian societies. At the same time, since this broader white category obviously includes many mulattoes, and the possibility of even color differences within a given family, there is a much higher toleration of colored ancestry. All this leads the mulattoes to act as "a socio-cultural bridge" in permitting Negroes to pass into the mulatto group and then from the mulatto into the white category, a process which does not take place, at least in the last stage, in the northwest European and U.S. variant cultures. Thus the marginal, or mulatto, grouping is of prime importance in fostering cultural and racial homogeneity in Iberian societies as opposed to the antihomogeneous or "pathological" role they play elsewhere.

This is an oversimplified summary of a complex argument, and leaves out Hoetink's separate critique of segmented society theory and the problem of objectivity in the observation of creole mentality. Though many of his assumptions and materials can be debated, the most serious weakness in his well-developed argument is his conclusion. What are the reasons for the unique Iberian conceptions of "whiteness"? If it is due to their own greater "swarthiness," or their long mingling with North Africans, as Hoetink implies, then how can he explain the somatic norm images of Italians in São Paulo or the United States?

Though Hoetink fails to give us a solution to the origins of the patterns of race relations, neither has anyone else, and his book is of inestimable value in finally providing us with the framework of an agreed set of criteria upon which we can arrive at such a definition. There is little question that all future comparative research must begin with this germinal work.

HERBERT S. KLEIN

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Anthropologists in the Field. Edited by D. G. JONGMANS and P. C. W. GUTKIND. Assen: Royal Van Gorcum, Ltd., 1967. Pp. 277.

This volume, a collection of ten essays by different authors plus an extensive annotated bibliography (fifty-eight pages), is the result of an invitation that its editors, D. G. Jongmans and P. C. W. Gutkind, extended to a number of anthropologists who had participated in the series of lectures on fieldwork that the Institute of Cultural Anthropology of the University of Amsterdam regularly organizes. "Rather than reporting on the results of their research, [the authors were] requested to give an account of how they set about it, of the difficulties they had to face, their dealings with authorities, interpreters and informants, of the pleasure and pains of participant observation" (p. i).

The explicit intent of this book, then, is to make a contribution to the growing, but still relatively spare, literature on the actual, real-life processes of social scientific research, as distinguished from a more theoretical, schematic, impersonal, and idealized model of how such research ought or is supposed to proceed. Four of the articles it contains were previously published. Six of the contributing authors appear to be Dutch, one Belgian, one Canadian, and the other two British, a speculative inference, since the usual professionally relevant biographical information about them is not provided by the editors. The fieldwork on which the authors report was carried out in Southeast Asia (Ceylon, Indonesia, the Malay Peninsula), Oceania (New Guinea), and Africa (Congo-Kinshasa, Guinea, Niger). The range of formal topics to which the essays are advertently addressed includes problems of reliability and validity in social description; the properties of participant observation and survey methods seen in relation to one another, most especially their comparative advantages and disadvantages; oral tradition and history in the field; the restudy as a technique for the examination of social change; research methods in African urban studies; the anthropologist in government service; and ethical problems in modern fieldwork.

All the essays are written in a style that is at once elegantly literate and personally lively. Each essay is interesting and useful in its own way. Several of them make particularly valuable and original methodological contribu-

tions. Most notable among these are P. E. De Josselin De Jong's sociology of knowledge-like analysis from field data of cultural factors conducive to the members of a society showing a "strong sense of structure" versus data that make for a relative "lack of any pronounced structural sense" on their part (pp. 89–101); G. K. Garbett's suggestions as to how the restudy technique in co-ordination with the extended-case method can produce "truly diachronic accounts" of social change (pp. 116–31); and J. Vansina's autobiographical account of discovering and developing field techniques for the use of oral traditions as a historical source (pp. 102–15).

In a way not necessarily planned or fully intended by its editors and authors, *Anthropologists in the Field* is also a source of primary data on the contributors' underlying attitudes toward the essential nature of their discipline, its substantive interests, methods, and concerns, in this era when, under the impact of complex, far-reaching processes of social change, the "'typical' primitive social groups" that anthropologists have been accustomed to studying have been "altered more rapidly than the anthropologist's technique" (A. I. Richards, quoted by P. C. W. Gutkind, p. 138). Three of the essays in this volume, for example, are devoted to what the editors term the "pros and cons" of "the trusty, familiar techniques of anthropological research: participant observation and open-ended interviews" (p. 58), compared with survey methods. In this connection, an invidious point of view is repeatedly expressed. Several of the authors view the survey as a dehumanizing way of "counting" things, "people and houses and fields and acres and answers to questionnaires," using "clever . . . statistical arithmetic" (p. 77), a method that has the "drawback of lacking all verve or colour, of draining the life from everything while the results, when presented, fail to evoke any picture of group life" (p. 29). The survey is not only presented as the chief tool of sociologists, but as virtually synonymous with the field of sociology. In those rare instances where the authors acknowledge that sociologists may reach conclusions that are insightful and evocative as well as fundamentally correct and reliable, it is claimed that they are demonstrating the inherent talents and qualities of "first class anthropologists" (pp. 76 and 85). In vivo fieldwork, relying principally on participant observation and face-to-face interviewing, is depicted as the method par excellence of the anthropologist, through which, in contradistinction to the conventional sociologist, he captures and portrays something that approaches the "wholistic" reality of the community he studies (p. 32), and engages in a "creative," "artistic," as well as scientific and technical activity, all the while "drawing heavily upon" and expressing "that very personal factor . . . , the unique personality of every researcher" (pp. 27 and 102).

It would be a mistake to look upon such statements purely or even primarily as comprising an antisociologic, antiquantification polemic. Rather, what they seem predominantly to be are assertions of certain fundamental, humanistic values that the anthropologists contributing to this volume espouse, in the face of the great changes in the characteristic research situations in which they now find themselves. "The small, isolated, more or less homogeneous tribal group which was the original interest of anthropologists, is becoming rare in the non-western world. The current problems of the new nation are concerned with large social groupings: nations, towns, trade unions, cooperatives, bureaucratic bodies, political parties. . . . Partly with a view to their various development projects, the new nations themselves are particularly interested in information concerning the social and economic situation of the country as a whole" (p. 58). The "resilience of [some] 'old' social systems in the face of change [has been] underestimated very considerably" by various anthropological studies made in the past, while, in other settings, rapid social change has taken place where anthropologists "expected and predicted . . . only slow and gradual change" (p. 124). In the kinds of "post-colonial" field situations in which anthropologists presently conduct their work, new "serious problems" arise concerning "what attitude the ethnographer should adopt towards those in authority" (p. 197). Furthermore, "ethnographic and other sociographic publications find their way back into the field. The Trobriand Islanders now know of Malinowski's books and one of them has reported that Malinowski did not understand their system of clans and chiefs" (p. 205).

In these and other ways, the current and future significance of the classical field methods of anthropology, as well as its established

theoretical and empirical concerns, is being thrown into question. And certain "features of traditional . . . life" explored and described in the simpler, less challenged era of past anthropological work now stand revealed by the developments that have since taken place in previously studied communities as having been "imperfectly analyzed and understood" (p. 135).

Anthropologists in the Field is a somewhat defensive, frequently nostalgic, but, on the whole, honest and constructive acknowledgment by a group of anthropologists that the "happy days of ethnographic osmosis, when the fieldworker could sit down and allow the fluid material from primitive life around him to percolate his intellectual and emotional tissues" (R. Firth, quoted by A. J. F. Kobben, p. 35), as he contemplated "wide uncontaminated open stretches of nature" (B. Malinowski, quoted by J. A. Barnes, p. 212), have come to an end.

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Pakistan's Development: Social Goals and Private Incentives. By GUSTAVE F. PAPANEK. Cambridge, Mass.: Harvard University Press, 1967. Pp. xxii+354. $8.95.

In reviewing a technical book on mathematical approaches to economic growth in a recent issue of this journal (Vol. LXXIII, November, 1967), Stinchcombe commented, "Except for the rare sociologist of modernization who has the old-fashioned notion that economic growth has something to do with it, the substance of the book will be of little interest to the discipline." It will be most unfortunate if this is the fate of Papanek's excellent study. While I am not qualified to assess the details of the economic analysis presented, I can testify that this book is well-done sociology which contains materials relevant not only to an understanding of the developmental process but to a grasp of the fundamental importance of social structure in influencing social process generally. Substantively, the book will be of interest to area specialists; tactically, it should be required reading for foreign aid officials and for senior civil servants in developing nations; sociologically, it will be of interest to students of the interaction between social structure and social behavior; therapeutically, it should serve as a partial antidote to those explanations which locate the sources of social change in personality structure and cultural tradition.

A common stereotype of Pakistan is one which sees the country as a geographic anomaly, barren of substantial natural resources and populated by a tradition-ridden and inert peasantry governed under a theocratic constitution by an authoritarian military regime. There would seem to be, judging from Papanek's description, somewhat more substance to this stereotype than to many others: postpartition Pakistan *was* a geographic anomaly, its civil service was sharply depleted, it had no industrial base and no indigenous class of industrial entrepreneurs, there were sharp disparities in economic potential in the two regions and perhaps even sharper inequities in individual wealth. In spite of these and many other problems, however, Pakistan has showed, particularly in the last decade, a pattern of economic growth and industrial diversification which makes reasonable the hope that economic increments will not be eroded by population increase.

In attempting to find explanations for this largely unanticipated success, Papanek has used a variety of data sources. In addition to systematically examining economic statistics (there are a series of valuable appendixes) and changing government regulations, he surveyed industrialists in firms accounting for 60 per cent of the country's industrial output, as well as a substantial number of government servants in a variety of agencies and on different levels of authority. It might be noted, parenthetically, that he had very substantial success in getting responses from those chosen to be interviewed; he obviously enjoyed his five years of work in Pakistan and equally obviously hopes for continuing success for the country in its attempts to develop. There is no evidence that this bias has affected his interpretation.

Government planning and government controls (in varying mixes of direct and indirect regulation) have provided the structural setting within which Pakistan's industrial economy has flourished. The successful mixture of types of controls in which indirect controls have come to play an increasingly important role in several crucial sectors has not, in every instance, resulted from deliberate design but has, on several occasions, occurred fortuitously. In some instances, indirect controls were

introduced simply because personnel resources were not available for direct regulation. In some of these instances, the savings introduced with an increase in incentives for innovation and a decline in corruption and inefficiency in administration were substantially greater than would have been possible with more direct manipulation.

Government involvement in industrial production has been low, however, and development could not have occurred if it had not been possible to motivate previously inexperienced groups to enter into industrial activity and, at the same time, to provide a structural framework which encouraged savings and reinvestment rather than consumption. It is here, it seems to me, that Papanek makes his major contribution by undermining the myths of cultural and/or personality resistance to social change. He finds that traditional trading communities willingly entered the industrial arena if there were reasonable expectations that profit could be taken. He even demonstrates that agriculturalists respond to economic incentives if risks can be demonstrated to be reasonable. In a commentary on expected problems in generating savings in underdeveloped nations, he writes (p. 185):

> The outlook for private savings is therefore considered discouraging, unless religion or ideology intervenes. Supposedly Protestantism and Communism are the only strong ideological movements to stress austerity, savings, and productive investment. This is unfortunate since no underdeveloped countries are Protestant, and few authors of such arguments advocate Communism. The Japanese experience does not fit neatly into this scheme, so it is usually ignored or ascribed to unusual circumstances. There seems to be no disposition to elevate the Buddhist or Shinto ethic to the esteem enjoyed by the Protestant ethic in encouraging austerity in consumption, high savings and productive investment as a causal factor.

Papanek demonstrates the "social utility of greed" (a chapter title!). But while economic incentives could stimulate entrance into industrial activity, they could not guarantee continued reinvestment and savings. Here, Pakistan was fortunate, first in that the traders and others who entered industry and took large profits had to learn new consumption styles (which they did only gradually) and thus simply turned their profits back into their enterprises; and second, in that by the time those consumption styles began to change, the government had instituted controls which either directed consumption to internally produced commodities or which permitted substantial government revenues to be gained from traffic in imported luxuries.

It is difficult to resist the temptation to review in detail some of Papanek's evidence and arguments. There are few wasted words in his book; it is difficult to convey the full flavor of his argument in a short review. If more economists wrote as sociologically as Papanek, more sociologists would (hopefully) read more economics. This book deserves a wider readership among sociologists than it is likely to get.

ALLEN D. GRIMSHAW

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Some Aspects of Social Structure. Edited by WALTER BIRMINGHAM, I. NEUSTADT, and E. N. OMABOE. ("A Study of Contemporary Ghana," Vol. II.) Evanston, Ill.: Northwestern University Press, 1967. Pp. 271. $9.00.

In retrospect, it seems likely that one of the most solid accomplishments of the regime of Kwame Nkrumah was the census of population taken in 1960. Over the last decade, much basic and applied research in the social sciences in tropical Africa has been impeded by the absence of reliable demographic data, and the Ghanaian census constitutes a major achievement that may well constitute a model for future efforts in other parts of the sub-Saharan area. Certainly, the present volume (the second of a series on contemporary Ghana) could not have been written without these census materials, and no less than three-quarters of its content focuses upon the general characteristics of the Ghanaian population, with special reference to change and migration. As the editor points out, the work is more limited in scope than the title suggests; no attention is given, for example, to political structure or to emergent systems of social stratification.

Unquestionably, the four essays by J. C. Caldwell dominate the volume both quantitatively and qualitatively. His skilful interweaving of census and survey data provides a detailed overview of the contemporary demographic scene and confirms earlier tentative conclusions while generating new and substan-

tial findings. Even when major allowances are made for immigration—one-eighth of the Ghanaian population is of foreign origin—and underenumeration in earlier censuses, the rate of natural increase of population is on the order of 3.5 per cent. This is a staggering figure and would suggest that previous estimates have understated the probable rate of population growth in tropical Africa. In Ghana's case, this increase is largely being absorbed into the densely settled southern portion of the country and particularly into the towns. Indeed, rapid urbanization and remarkably high rates of internal migration from northern areas are indicative of extensive population shifts. As in other African territories, there is every indication that new employment opportunities will not match growing demand. This is a familiar and depressing picture that raises serious questions of public policy to which European experience cannot speak. As Caldwell points out, a ban on immigration would be limited and equivocal in its effects, and though there is some evidence of changing attitudes concerning family size there is no sign that such change is occurring among more than a tiny minority of the population.

The remaining essays suffer by comparison, due to the more limited data available to the authors. P. A. Tetteh's essay on "Marriage, Family and Household" provides a useful postscript to some of Caldwell's observations and indicates that increasing freedom in mate selection and greater emphasis on the conjugal family in urban areas do not erode strong, extended family ties. Moreover, interesting adaptations are emerging in response to pressures toward monogamy generated in a society where polygyny has been traditionally esteemed. G. E. Hurd's chapter on education is a valuable summary of existing materials and highlights the sharp inequalities of educational opportunity that exist in Ghana and most other African states. The article by C. G. Baeta on "Aspects of Religion" is lighterweight stuff; one would wish to have seen more extensive commentary on the newer syncretistic sects. The volume ends with a section on land tenure that is particularly valuable in its attempt to analyze legal problems stemming from large-scale population movements connected with the Tema and Volta river schemes.

This is a worthwhile book that will be welcomed by both general demographers and students of social change in contemporary Africa—a refreshing contrast indeed to some of the more pretentious materials concerning that continent that have appeared in the last few years.

PHILIP FOSTER

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Sebei Law. By WALTER GOLDSCHMIDT. Los Angeles: University of California Press, 1967. Pp. xiii+303. $7.50.

The Sebei are a people of Uganda who were once independent but came under the control of the Baganda in 1906 and the British in 1920. They were once a pastoral people, but even before Baganda times they had developed a mixed economy with a strong dependence on hoe farming. Walter Goldschmidt, in a welcome addition to the ethnography of law, has attempted to reconstruct the law of the Sebei during their last years of independence, so-called old Sebei law.

Sebei Law is a valuable monograph, despite several shortcomings. Goldschmidt makes a valiant attempt to found his account on a large body of actual cases rather than on mere legal vocabulary. He also refuses to define law in timeless cross-cultural terms, preferring to focus on how the "law jobs" are actually performed among the Sebei. Given such methodological starting points, we would hope to be provided with a picture of Sebei law as an ordering process. Instead, it is depicted as a set of rules, and the reader often has relatively little sense of what is legally problematic in these rules, what the difficulties are in interpretation, or how the rules are invoked and implemented in concrete ordering situations. The difficulty derives less from the author's intentions than from the fragmentary and isolated character of many of the reports of Sebei informants and from limited opportunities for actual observation of the legal process. A related weakness is built into Goldschmidt's attempt to write in the ethnographic present about old Sebei law, even though his case materials span an entire century, from the mid-nineteenth century up to the present. Thus, materials relating to a variety of procedural settings are lumped into a single body of cases, in an attempt to describe the relatively timeless features of the normative structure. In consequence, the reader is often con-

fused on questions of time reference and legal development.

Despite these strictures the volume merits high praise for its sensitive and stimulating interpretations. Goldschmidt's account of the "metaphysical infrastructure" of Sebei law and his commentary on the adjustment of pastoral institutions to a mixed economy are particularly rewarding. He presents a fascinating account of the relation between the collective unity of the clan and the individual character of private property. The apparent tension between these poles is resolved by the ability of the Sebei to translate all social relations, either with the clan or with individuals, into property rights and vice versa. The translation is facilitated by the strong identification of personality and property. Thus, the individual is articulated with the society through his participation in a complex network of property rights and their concomitant social obligations and sentiments. This system was originally grounded in a pastoral economy. With the development of horticulture, a new level of sovereignty began to emerge, the fixed territorial community. Goldschmidt presents an intriguing reconstruction of the first development of the symbols, rituals, and procedures of emergent territorial solidarity. Because of Goldschmidt's sophisticated treatment of Sebei social relations, we are given more than an ethnography; we are given a constructive essay in the theory of solidarity.

LEON MAYHEW

University of Michigan

Cultural Pluralism and Nationalist Politics in British Guiana. By LEO DESPRES. Chicago: Rand McNally & Co., 1967. Pp. 240. $4.50.

This book is a creative attempt to develop an empirically applicable model of cultural pluralism and use it to analyze nationalist politics in British Guiana (now Guyana).

Despres begins with M. G. Smith's model of cultural pluralism, but modifies it by introducing the following criteria for distinguishing between homogeneous, heterogeneous, and plural societies: "(1) the extent to which specified groups are culturally differentiated in terms of specific institutional activities and (2) the level at which institutional activities serve to maintain cultural differentiation as the basis for sociocultural integration" (p. 22). If there are no culturally differentiated groups, the society is "homogeneous." If cultural differentiation is maintained only at the local level, the society is "heterogeneous"; and if it is maintained at the national level, the society is a "plural society."

Applying this model of cultural pluralism, Despres systematically shows that Africans and East Indians in Guiana either do not participate in the same local institutions or that the structure of these institutions is modified for one of the groups. Thus, he argues, cultural differentiation is maintained on the local level. On the national level, Despres finds that East Indians and Africans either do not participate in the same institutions or that one group evaluates its participation in such a way that the common institution does not reduce cultural differentiation significantly. Thus, cultural differentiation also is maintained at the national level, and Despres concludes that "British Guiana displays the characteristics of a plural society to a marked degree" (p. 175).

Despres's model of cultural pluralism has one major shortcoming. It fails to distinguish between cultural pluralism and social or structural pluralism (i.e., the segmentation of a society into corporate groups with duplicate or culturally similar sets of institutions). This led Despres in his analysis of Guianese society (1) to use social pluralism in several cases (e.g., political parties) as a measure of cultural pluralism, (2) to ignore the fact that race as a social label can produce a quantum of social pluralism and conflict that is only historically related to cultural pluralism, and (3) to ignore the amount of acculturation that has taken place in Guianese society. Thus, the fact that East Indians and Africans share enough of a common culture to communicate easily and to co-operate with each other in schools, in the leadership of political parties, and in numerous personal friendships is not even taken into account in Despres's assessment of the degree of cultural pluralism in Guiana.

In analyzing the disintegration of the nationalist movement, Despres includes cultural pluralism as one of several pressures for disintegration, but identifies international events, especially American penetration of the Caribbean and Guianese labor movements, as the most important single pressure.

After the disintegration of the nationalist movement, Despres stresses the importance of

the organizational strategies of nationalist leaders as an intervening factor between pluralism and politics. This enables him to avoid the unrealistic proposition that pluralism inevitably leads to violent conflict. Instead, he argues that "cultural sections do not clash by chance or because their structures express incompatible values: They clash because certain individuals and groups have decided that something can be achieved by way of making them clash" (p. 29).

Despres does not see Guianese leaders as being motivated by their cultural differences but only as facing the decision of whether to utilize the pluralism of Guianese society for their political goals. This not only is a tacit admission that cultural pluralism has not affected the values of the leaders but also greatly reduces the explanatory power of Despres's model of cultural pluralism.

Despres tries to recoup this loss of explanatory power by arguing that Guianese nationalist leaders were forced to operate in a neocolonial setting and thus "adopted organizational strategies designed to make existing sociocultural patterns functional with respect to the internal struggle for constitutional power" (p. 283) (i.e., organized political parties along racial lines). This concept of a "neocolonial adjustment," however, is not worked out in enough detail to produce an adequate theoretical framework for predicting or explaining the effects of cultural pluralism.

Take the following statement for example: "Given a neocolonial setting in which cultural sections are politically juxtaposed in a struggle for national power, is violent conflict probable? In general, the answer is yes" (p. 283). The inadequacy of this proposition is shown by the fact that in both Trinidad and Surinam, East Indians and Africans have been juxtaposed in struggles for national power in neocolonial settings for a number of years; however, violent conflict has not developed in either of these countries. Violent conflict may develop in either of these countries in the future, but Despres's theories do not tell us when or why it will occur or not occur, except that it will occur if the leaders want it to occur. Unfortunately, the elaborate model of cultural pluralism ends up looking in the window.

This book merits widespread attention for the same reason that it has stimulated a critical review. It dares a type of theoretical creativity and comprehensive research that is seldom brought to bear on problems of world importance.

JOSEPH B. LANDIS

Yale University

Strangers to the City: Urban Man in Jos, Nigeria. By LEONARD PLOTNICOV. Pittsburgh: University of Pittsburgh Press, 1967. Pp. viii+320. $2.95.

The City in Modern Africa. Edited by HORACE MINER. New York: Frederick A. Praeger, Inc., 1967. Pp. xi+364. $7.50.

Comparative urbanists should benefit during the next few years from the growing literature on urban Africa. The world's fastest agglomerating, least populated, and largely unindustrialized continent is being studied by many urbanists skeptical of inherited theoretical models, anxious to avoid new parochialisms or disciplinary restrictions, and critically concerned with development theory. While American universities and foundations may preoccupy themselves with the urban problems of their own continent in the next few years, these two volumes suggest that they ought to keep track of what is happening elsewhere.

For example, Plotnicov's *Strangers to the City* is based on the assumption that the people we write about are strangers to the analyst as well, an assumption now being brought home in the United States. Two-thirds of his book is devoted to portraits of eight immigrants to Jos, Nigeria, a city about fifty years old with sixty thousand inhabitants. There is also a map and introductory chapters on Jos's political, economic, and social origins and characteristics which are welcome. While Plotnicov does not demonstrate differential individual response to the same specific events, he does carry his point that individual choices are guided by a search for order and are not chaotic responses to chaos, however heterogeneous the total urban universe appears to be. The strict methodologist may not be persuaded by the author's defense of his "sample," which is older and more successful than the population of Jos generally, but he may also find this a less important issue than did the author.

Horace Miner's volume presents papers originally given at a symposium in 1965 and

has the familiar characteristics of such collections. The content is less inclusive than the title, and in fact only one chapter, by Aidan Southall on Kampala, treats a city as an entity. However, Miner, in pulling these materials together, has supplied a useful guide to the authors' contributions and has given helpful characterizations of the city as a social unit and of the process of urbanizing a society. Throughout the book is a very basic concern with just what it is one is studying when one looks at urbanization, and it is to this point that the comparative urbanist will wish to address himself. There is, for example, Peter Marris' reflection on his earlier study of "urban renewal" in Lagos that our motives and methods in dealing with Africa dramatize "our own ideological dilemmas," or Joseph Spengler's excellent application of optimal size theory, developed elsewhere, to urban Africa, or Lionel Tiger's suggestive (if not very empirical) thoughts about the analogies between the role of the modern urban civil service in Ghana and that of the religious hierarchies in medieval Europe. One of the most serious studies in this book, by Remi Clignet, is based on 1,207 interviews in Abidjan, and affirms the proposition derived elsewhere that urbanization is not at all a uniform process but one "associated with increased social differentiation and with accentuated differences in the access of the various social categories to the resources of the total society" (p. 290).

Further concern with the theoretical inheritance is demonstrated by William J. Barber in a disturbing essay concerning the white settler territories of Rhodesia and Zambia. He shows that the "accentuated differences" are so great that it is doubtful whether towns are generative for much of the society in reshaping work attitudes, teaching new skills, channeling capital back to rural areas, or creating new jobs quickly enough to justify current urban capital investment. He urges a reconsideration of the more difficult task of agricultural reform. This is in line with Daniel Lerner's remarks about the urban masses in the underdeveloped world as the displaced persons of a global development process that is not working well. Lerner also suggests that a moratorium might be placed on development for some societies that might be able to achieve it at lower human costs twenty years hence.

Also concerned with extant urban theory is Leo Kuper, who finds it difficult to place towns in the white settler societies of South Africa "within the development sequences of classical sociological theory," a point now probably generally accepted. There are other studies: by William and Judith Hanna dealing with two small "urban-centered African communities" as they "relate to the integrative process" of their respective larger societies (a subject coming into its own among geographers and economists concerned with the problems discussed by Barber), and by Robert LeVine and his associates of father-child relationships and changing life-styles in Ibadan.

American urbanists will find some familiar elements and some new ones in these volumes and a variety of new combinations and conditions. They will find some of their theories verified and others questioned. Those already concerned with the literature of comparative urban studies will want to add these well-made and modestly priced volumes to their libraries.

George Jenkins

University of California, Los Angeles

Identities and Interactions. By George J. McCall and J. L. Simmons. New York: Free Press, 1966. Pp. 278. $7.95.

This book is certainly not for the tidy-minded. But could a book that attempts to "illuminate systematically what is known—along with a great deal that is yet only suspected—about the fine-grained processes of human face-to-face interaction" (p. 12) be anything but untidy?

Rigorous it is not; courageous it is. Most social psychologists consider themselves pretty lucky if they are able to isolate dependable relationships among one or two variables under conditions of their own making. At the same time, I suspect that even these—the analytically oriented—long, at times, to *understand* the complexities of interaction, to say nothing of being able to make a few real-life predictions once in a while. There are a few who even hope for a day when they are not forced into some particular psychological perspective *or* some particular sociological perspective *or* some particular anthropological perspective to examine and explain some fragment of human interaction. They seriously consider the possibility that it might not be

necessary to man a new fortress each decade, whether it be a self theory, a reference group theory, a role theory, a balance theory, or an exchange theory.

So it is with McCall and Simmons. They do not assume that knowledge of the complexities of interaction will come about by adding together the bits and pieces of hard-won empirical generalizations. Rather, they throw a wide net—using what they call, simply, an "interaction" approach (neo-field theory?)—drawing on but not dominated by any of the specifics of currently popular theoretical perspectives. It is this attempt that one applauds.

The attempt begins by outlining some boundaries of interaction, narrowing down possible and non-possible behaviors by specifying social and cultural constraints. This is followed by a review of the basic principles underlying symbolic interaction. Then they get down to business. The central concept is *role identity*—"defined as the character and the role that an individual devises for himself as an occupant of a particular social position" (p. 67)—where the emphasis is on the improvised interactive role rather than the more mechanical concept of role so familiar in sociology. This concept is further developed in a discussion of the organization of role identities and, particularly, hierarchies of role identities.

The key to the theoretical framework emerges in the discussion of the dynamics of interaction (how few since Waller have dared such an undertaking?). Drawing heavily on an exchange perspective, they spell out some of the complexities of negotiation in interaction, with some emphasis on the exchange of support for the interactor's various role identities. There is a rich discussion of the careers of relationships and the "agenda of performance"; among other things, attention is given to the idea of time as the commodity in shortest supply and the resulting emphasis on reward dependability in ongoing relationships.

The ultimate test of the success of this attempt lies, of course, in the empirical grounding of their theoretical framework. The authors refer to their own such ongoing research in the epilogue—concentrating largely on describing operational measures of various concepts. A more immediate "test" is the ability of the theoretical framework to generate researchable hypotheses. Such is promised but not as yet demonstrated. In the case at hand, there is perhaps more than average concern with this "test," insofar as the theoretical antecedents of this work—in general, the symbolic interactionist's position and the more specific role, self, and reference group theories—have not been particularly distinguished in their power to generate such hypotheses.

What about the success of the framework, then, in incorporating and explaining the diverse and sometimes contradictory research findings already available? There is not much we have to go on here, either. The body of the book is, as they say, a "theoretical essay, with research considerations confined to the epilogue, so that the structure of our argument might be more clearly discerned" (p. 260). There is very little research cited at all, on the grounds, I gather, that the authors consider most of the research in interaction not rich enough "to catch something of the flavor of the complex processes intertwined within ongoing social life" (p. 256). Maybe—but a variety of research findings in such diverse areas as social facilitation, ingratiation, emotional contagion, group pressures for conformity, interaction networks, etc., could possibly take on the richness the authors seek if they were skilfully and painstakingly incorporated under their own theoretical umbrella.

We can, however, more happily conclude on two other notes. For one, the contribution of this work to the doings of sociology is considerable. For example, a number of the concepts developed here provide important breakthroughs in our ability to research the *ongoing* process of interaction. For the other, this analysis is, for the most part, interesting, plausible, and insightful. It is sometimes even fun. So much so, in fact, that one is sorely tempted to resist evaluation on the more formal criteria indicated above and to revert to Hemingway's position that "if it feels good it is right."

HELEN P. GOULDNER

Washington University, St. Louis

Encyclopedia of Psychoanalysis. Edited by LUDWIG EIDELBERG and Others. New York: Free Press, 1968. Pp. xxxvii+571. $27.50.

There is a need for responsible and effective intellectual liaison between sociology and such neighboring disciplines and professions as psychoanalysis, and the *Encyclopedia of Psycho-*

analysis might have helped to meet that need. Unfortunately, this compendium is compulsively Freudian, in the most orthodox sense. Terms and theories developed by the minor members of the Freudian inner circle are paraded prolifically, while sparse, uneven, and inaccurately disparaging coverage is given to work derived from psychosocial and other "revisionist" viewpoints. This conservative emphasis is especially unfortunate for social scientists, since it is the progressive wing of the psychoanalytic profession whose substantive concerns are germane to our own and whose concepts and generalizations are sometimes operationalizable.

The book contains fewer than five hundred pages of actual content; the rest are indexes, some redundant. The type is so unusually large for a book of this kind and the spacing is so ample that I wonder what market the publisher had in mind. The price is extravagantly high, and the book appears to be a commercial venture, although the fine print tells us that it was an "investigation" supported by the National Institute of Mental Health.

The only credit which may be given is that the coverage of the orthodox Freudian concepts and theories is clear, accurate, and authoritative. But there still is need for a less dogmatic, less truncated reference work. In the Index, there are five references shown for the work of Erik Erikson, one for Abram Kardiner, one for Karen Horney, and none for Harry Stack Sullivan, Erich Fromm, Rollo May, or Merlieu-Ponty. On the other hand, there are eleven for Wilhelm Fliess, including two on "Freud's friendship with" and two on "Abraham's allegiance to."

KENNETH I. ROTHMAN

University of Chicago

Followers of the New Faith: Culture Change and the Rise of Protestantism in Brazil and Chile. By EMILIO WILLEMS. Nashville, Tenn.: Vanderbilt University Press, 1967. Pp. x+290. $7.50.

Churches and States: The Religious Institutions and Modernization. Edited by KALMAN H. SILVERT. New York: American Universities Field Staff, Inc., 1967. Pp. xiv+224, $7.50.

These two volumes are worthwhile, if not particularly illuminating, contributions to the rapidly growing literature relating modernization and religion. The authors share a grounding in Weber, although they contrast in styles and methodologies; and, in my opinion, the Silvert collection (none of whose contributors are professional sociologists or anthropologists) is more satisfying than the work by a sociologist. *Churches and States* is the fruition of a conference on religion and modernization sponsored by the American Universities Field Staff and held at Indiana University in October, 1966. In a departure from usual concerns in its topic area, it focused on Judaism, Catholicism, and Islam as mediators and/or inhibitors of social change. The contributors explicitly consider the degrees and manners in which these faiths, which have become intertwined with various traditional societies, have then adapted to the ideological and structural changes resulting from rapid modernization. Jacob Neusner examines how confrontation with a gamut of modern polities has led both to alternations in the belief system and institutions of Judaism and to the adoption of such attitudes as socialism and Zionism. Victor Du Bois, George Sanders, and Dennison Rusinow describe Catholic efforts at adaptation to social change and the factors inhibiting this process in (respectively) French sub-Sahara Africa, Brazil, and Austria. A final set of articles by Stuart Schaar, Willard Hanna, Charles Gallagher, and Louis Dupree considers the very different situations and responses of Islam in a variety of sociopolitical contexts in the Middle East and in Malaysia but not, unfortunately, in Africa. If some of these discussions substantiate the view that the "faith" in a given society acts as a reactionary brake on development and fails to adjust to such processes as structural differentiation and functional specificity, with their demand for secularization, then other articles demonstrate that in certain conditions traditional religious institutions, or elements of them, are able to align themselves with the forces of revolution. As Silvert points out in his lucid and regrettably brief Introduction, the critical focus for analyses which seek to clarify these different responses must be on the ideological and power relationships between religious institutions and other institutional systems involved in the general flow of economic change.

Such indeed is the theme of Willems' book as well. Although he subscribes to the thesis that the growth of Protestantism in South America must be seen in the context of industrialization, internal migration, urbanization, and secularization, he largely neglects to relate the development of the Protestant churches to other types of social movements which can be seen as providing alternative responses to the same conditions. While he discusses the political attitudes and actions of the various Protestant groups and examines factors underlying the variation among them, he does not illuminate the problem of the circumstances of choice and the consequences of joining a Pentecostal sect as against a radical union. The book is divided into three parts, the first of which considers the degrees and ways that Protestantism is compatible with traditional Brazilian and Chilean value orientations and organizations. A strong argument is made for the historical presence of various characteristics in both societies that were and are congruent with Protestant social forms and values. Willems points out, for instance, that even during the last century in Brazil there was a class of small rural landowners and a *lumpenproletariat,* neither of whom were fully integrated into the plantation system for the patronage network. In a later section, he attempts to demonstrate that, besides developing in cities, Protestant sects have grown only in those rural areas which were never, for historical or climatic reasons, dominated by haciendas or which are now frontiers with mixtures of extremely heterogeneous elements. He does not consider, though, the mesh or lack of it between Protestant epistemological assumptions and categories and those traditional in Brazil and Chile in as much detail as I would like. Chile is utilized as a means of controlling for factors perhaps unique to Brazil and of generating hypotheses related to Protestantism in Latin America as a whole, but we are told in the conclusion that Chile is also unique and very different from other Spanish-American societies.

In the second section, Willems discusses a wide variety of relationships between Protestant movements and the changes in many sectors of these societies to substantiate his theme that Protestantism, particularly in its evangelical or Pentecostal form, is "adaptive" to the emerging modern social order. Here he makes a number of interesting points the impact of which is somewhat vitiated by the lack of quantitative material and analyses, a pervasive flaw throughout the book that Willems recognizes. It is regrettable that more extensive data on the relation of social class to membership in different Protestant groups through time and in different areas could not have been obtained. There is very little information on how recent political events in both countries have affected their Protestant minorities. In this section as in others, a variety of subjects is handled generally and rather superficially. While there is an extensive treatment of the historical churches' connection with secondary education, the patterns of socialization within Protestant families receive almost no attention. The third section of the book deals with the different histories and characteristics of various Protestant denominations. Willems' analysis of the reasons behind these diverse adaptations to similar social environments is very intriguing, especially for his conclusion that organizational schisms and proliferation of sects seem to be more effective than ideological disputes as a means of expressing rejection of the traditional society and even of some aspects of the emerging one. As a whole, however, the book lacks a theoretical framework which could relate all the divergent empirical bits and conceptual pieces concerning discrete aspects of Latin-American Protestantism to the functions of religious pluralism within a context of structural differentiation and secularization.

JON CHRISTOPHER CROCNER

Duke University

The Sacred Canopy. By PETER L. BERGER. Garden City, N.Y.: Doubleday & Co., 1967. Pp. 171. $4.95.

The Sacred Canopy is termed by its author, Peter L. Berger, an "exercise" in the sociological theory of religion written from the vantage point of an expanded conception of the sociology of knowledge. The latter has been set forth by Berger in *A Social Construction of Reality: A Treatise in the Sociology of Knowledge* (New York, 1966, p. v), which he wrote with Thomas Luckmann.

Berger sees human beings as "world-builders" whose man-made worlds are precarious

in the face of the brevity of human lives and the erosion of time—hence, a need for legitimation in order to insure the internalization of societal norms and so give some durability and "facticity" to these objects of human creation.

Religion is seen as *one* important means of legitimation—an especially effective one, because religion has the unique capacity to take man-made structures out of historical time and to impute to them an aura of the eternal by locating them within a sacred frame of reference. Since all legitimation serves to maintain "reality" and repel the forces of chaos and anomie, a prime function of religion is thus "world-maintenance." Berger sees the fundamental recipe for religious legitimation as the transformation of human products into supra or non-human "facticities." Such legitimations continue to be effective as long as a "community of plausibility" persists. This plausibility is inevitably threatened by the secularization process, which the latter author discusses at some length in terms somewhat reminiscent of Harvey Cox. He sees the "current crisis in theology" as grounded in a "crisis of plausibility" which in its turn is grounded in changing social structures rather than in theorizing. The current problem in religious institutions is thus how to keep going in a milieu that no longer takes for granted their definitions of reality.

Peter Berger has been a sociological colleague of theologians and has sat at the feet of philosophically inclined sociologists. *The Sacred Canopy*, in consequence, shows traces of Berger's theological and philosophical past. On the whole, it may be said that these influences have enabled Berger to write a book which may "speak to the condition" of the theologically minded more directly than do many such books written by sociologists.

To a sociologist, however, the book contains little that is new. This reviewer, maybe a bit obtusely, can see little difference between his expanded concept of the sociology of knowledge and general cultural theory. Furthermore, Berger uses his theoretical lens to illuminate well-trodden paths. Nevertheless, he does succeed in casting an interesting new light upon these paths. He writes, moreover, in a challenging way, skilfully injecting theological materials drawn from both Eastern and Western religions to reinforce his points. He also displays an understanding of the issues that concern practicing religionists, while at the same time he tells them frankly where the going for them is likely to be rough. This book should find a ready market in seminaries and rabbinical schools.

Elizabeth K. Nottingham

Queens College

A Sociology of English Religion. By David Martin. New York: Basic Books, 1967. Pp. 158. $5.00.

There is much information packed into Dr. Martin's short book. True to the British survey tradition associated with Charles Booth (see pp. 29–33), Martin grounds his study in demographic fact-finding surveys concerning the incidence and distribution of religious membership, belief, and practice in the United Kingdom. For Americans who often know more of distant "anthropological" religions than those of people more closely akin to them, this treatment has considerable value.

In England, as in America, the number of those who merely identify themselves with a religious body far outstrips those who maintain institutional practice. Some two-thirds of Englishmen identify with the Church of England, 10 per cent with the Free Churches, and another 10 per cent are Roman Catholics. The approximately 10 per cent still unaccounted for comprise about 5 per cent who claim to have no religious label, 1 per cent who are Jewish, with another 4 per cent belonging to various sects and "foreign" religions (p. 36). These statistics, based on a composite of survey materials, are subject to many of the well-known difficulties of interpretation.

Judged on the basis of institutional religious observance, the English are considerably less "religious" than Americans. Says the publisher's blurb on Dr. Martin's book: "If God is dead anywhere, He would most surely seem to be dead in England, where the majority of the population is notoriously indifferent to churchgoing." Dr. Martin, himself a Christian "believer," sees this indifference as indifference to *institutional* religion rather than to religion per se. According to a mass observation study in the London area, over a quarter of the "doubters, agnostics and atheists pray on

occasion to the God whose existence they doubt" (p. 53), and even go to church on occasion. And over half of these non-believers think that there *should* be religious education in schools! Martin suggests, indeed, that the antiecclesiasticism of the working classes (whose maxim is, "You don't have to go to church to be a Christian") may well have been imbibed from the women elementary school teachers of religion who are often devout Christians with a hearty distrust for churches and clergy. Furthermore, this "irreligiousness" differs very much by region and even by denomination: the north and west are more favorable to the Church of England, Cornwall to Methodism and the Free Churches, while Wales is a stronghold of dissent. As in America, the great area of erosion is the large metropolis, though in England there is moral erosion as well.

Martin's pungent thumbnail sketches of the varieties of religious belief and religious culture are vastly entertaining. His writing is refreshing and devoid of jargon.

Martin is critical of much recent American work in the sociology of religion and finds it "thin." He appends a list of recent British publications in the field, including nearly forty (unpublished) recent Ph.D. theses either completed or in progress. To this reviewer the topics did not differ from those usual in the United States. Martin's own book, however, suggests a broad comparative theoretical frame of reference from which American sociologists could profit.

Elizabeth K. Nottingham

Queens College

Jewish Identity on the Suburban Frontier: A Study of Group Survival in the Open Society. By Marshall Sklare and Joseph Greenblum. ("The Lakeville Studies.") New York: Basic Books, 1967. Pp. xv+362. $11.50.

The Edge of Friendliness: A Study of Jewish-Gentile Relations. By Benjamin B. Ringer. ("The Lakeville Studies.") New York: Basic Books, 1967. Pp. xii+272. $8.25.

How are these books different from all other books about Jews? Certainly, they comprise one of the most detailed and comprehensive studies of Jewish identity and Jewish-Gentile relations ever made. They are also distinctive in including both Jews' and Gentiles' attitudes to each other and in using a large sample of third- fourth-generation Jews. The research was supported by the American Jewish Committee, which also sponsored work leading to Adorno *et al.*'s *The Authoritarian Personality*. It is indicative of recent developments that their concerns have shifted from anti-Semitism to "the question of how to guarantee their [Jews'] survival in a society which is . . . pluralistic but . . . so hospitable as to make group survival difficult" (p. ix).

The data were gathered in the late 1950's through intensive interviews with 432 Jewish and 250 Gentile residents of "Lakeville"—a Midwestern suburb which recently became one-third Jewish. It was selected primarily for having respondents and situations probably representative of the Jews of the near future.

After sketching historical and demographic settings, Volume I focuses on the nature and level of Jewish identity, examining the Lakeville Jews' attitudes and activities particularly in regard to their religious observances, organizations, friendships, and children. Their main conclusion is cautiously pessimistic. (You expected otherwise?) Judaism's survival is highly problematic even though they find that the great majority affiliate with a synagogue, belong to some Jewish voluntary association, have only or primarily Jewish friends, and want to transmit a Jewish identity to their children. Nevertheless, the future looks bleak essentially because the Lakeville Jew lacks a distinctive ideology to ground and maintain his actions. Thus, most feel that they should cultivate the esteem of Christian neighbors, that the "sacraments" between man and God are secondary to a generalized humanitarianism, and that actions which help guarantee cultural survival are not essential to being a good Jew.

Volume II focuses on how Lakeville's Jews and Gentiles feel, think, and act toward each other, especially on factors facilitating or inhibiting relationships among them. Some of the less obvious facilitators are the Jews' civic-mindedness, neighborhoods with Gentile majorities, and instrumental contact situations (on the job or in task-oriented voluntary associations). Still, relations remain superficial apparently due to mutual ignorance and anxiety

about the other group's private feelings and possible public behavior, Jews' readiness to suspect prejudice, and Gentiles' feelings about "the precocious *nouveau riche* behavior of the [Jewish] newcomers' children" (p. 260)—especially threatening to lower-income residents. In sum, the development of a genuine pluralism is doubtful, since Gentiles accept mainly assimilationist Jews, and Jews are very concerned "to gear their behavior to Gentile standards" (p. 264).

In addition to the above, interesting material is presented on factors shaping organizational character and membership; effects of life cycle and parental patterns on religious practices; kinds of rituals likely to survive (Vol. I); and sources, correlates, and types of Gentile attitudes to Jews (Vol. II), etc.

On the whole, these are useful and interesting studies, done, particularly Sklare and Greenblum's volume, with care and imagination. However, both volumes have disappointing aspects. While many tables were presented, not a single statistical test of significance was made. The omission from such a study of the Jews who had intermarried or converted (apparently a sizable number) is also questionable. In addition, I missed a careful synthesis of the findings, a presentation of their interview schedule, and a systematic articulation of the studies with general theories, say, of minority groups or of religion. And surely it is an accomplishment to write two entire books in this area and not once refer to Weber, Durkheim, or Parsons!

Still, the relatively meticulous job of the first volume makes the second's inadequacies even more glaring. In the latter, many frequency distributions on various attitudinal and behavioral patterns are given, but little analysis of "who" these people are is made. When it is made, the effects of factors indicated to be important in the first volume, such as age and class, are often not controlled for. Interpretations are presented as facts with only anecdotes to support them, although they seem readily testable. Finally, there are only a few references to the relevant theoretical and empirical literature.

Most important, I am skeptical of their pessimistic conclusions. After all, the great majority are very concerned to have their children educated religiously and consider "accepting being Jewish and not trying to hide it" as essential to being a good Jew. The decline in home observances seems compensated by the increased synagogue-centered activities and "moralism" over ritualism. Further, important parts of their argument are not carefully justified: that the content of this moralism is merely a vague humanitarianism, that this is not distinctively Jewish, and that this will prove dysfunctional to Judaism's survival. A theoretical perspective, such as evolutionism (à la Bellah or Parsons), might interpret all this as the evolution of Judaism to another stage—where the religion is more differentiated out of family life, where there is greater individual latitude (the "personalism" over "prescriptionism"), and where, for there to be a viable pluralism, a more generalized value system (the diffuse, humanitarian "moralism") must emerge.

ANDREW EFFRAT

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Goals for Urban America. Edited by BRIAN J. L. BERRY and JACK MELTZER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1967. Pp. 152. $4.95.

The Urban R's: Race Relations as the Problem in Urban Education. Edited by ROBERT DENTLER, BERNARD MACKLER, and MARY ELLEN WARSHAUER. New York: Frederick A. Praeger, Inc., 1967. Pp. xii+304. $7.50.

Urban Political Systems: A Functional Analysis of Metro Toronto. By HAROLD KAPLAN. New York: Columbia University Press, 1967. Pp. 320. $8.50.

These three books serve to underscore the variety and complexity of contemporary "urban problems." While of markedly different sociological content and approach, their contributions to the student of urban affairs are complementary.

In *Goals for Urban America,* Brian Berry and Jack Meltzer bring together a series of statements on urban policy. Their basic premise is that past commentators have approached urban policy like horses with blinders. Each has been restricted by the boundaries of his subject specialization—focusing on only a narrow vertical view of the total policy picture. Instead, they urge attention to a horizontal view that cuts across many areas of specialization at a high level of policy.

The papers in Goals for Urban America, with one or two exceptions, are from the 1965–66 series of lectures at the University of Chicago entitled "Key Figures Discuss Urban Affairs." Several faults of the book stem from the origin of its papers, for while the papers deal with policy, few are dedicated to the horizontal approach to policy which the editors emphasize in their Introduction. Indeed, at least two (by Senator Joseph S. Clark and Governor Terry Sanford) simply reflect the importance that their authors attach to the sphere of activity with which they operate (namely, federal and state programs). In addition, elementary statistics on the extent of city growth and concentration are repeated again and again until the reader, who already knew them, despairs of the possibility of finding anything new in these essays.

But he should not. I believe that these papers serve some useful purposes. First, they deal very explicitly with the policy realm, and at least several interesting ideas are expressed. For example, August Heckscher suggests that the products of city planning be viewed in McLuhanesque terms. David Owen challenges the premises of community development schemes. Martin Meyerson calls for a clarification of the functions (and dysfunctions) of ongoing urban programs before the introduction of yet more programs. Jack Meltzer and Joyce Whitley urge fighting the causes, not the symptoms, of slum areas, turning attention away from the individual traits of slum dwellers and toward such phenomena as power and control of local resources. While many sociologists choose to work at a more basic level than policy, acquaintance with how the other half thinks is salutary.

But even what sociologists might deem the weakness of many of the papers can be useful. Leaps into policy usually involve assumptions, and there are many assumptions here which might serve to organize past research and initiate new. When Senator Robert Kennedy, for example, urges elimination of huge urban ghettos, channeling the population into, among other places, a fixed percentage of the dwelling units in New Towns, he implicitly handles a variety of hypotheses on class and ethnic neighborhood relations which have been or could be weighed by empirical research.

Two themes go through these essays. One is a growing need for national programs to attack the sources of urban problems. The second is *that the particular problems of the American Negro accentuate and intensify whatever troubles cities already have and that these problems must be resolved as part of any attempt to achieve a more general urban order.*

The latter theme is the springboard for The Urban R's, whose editors are all associated with the Center for Urban Education in New York City. They have assembled eighteen, mostly original, papers dealing with intergroup relations (and their effects) in and around New York City schools.

There is great potential for experimental designs tying ethnic, race, and class variables to education in large metropolitan areas. Many of the proportions of "majority" to "minority" group members proposed in integration plans elsewhere are to be found in or near New York City right now, either naturally or by fiat, and the Center has been busy exploiting this advantage. As the authors point out, relatively controlled research complements large-scale survey approaches on the same subject (e.g., the Coleman-Campbell report) by leading to findings which also reflect firsthand knowledge of local conditions and influences.

Despite this initial advantage and some good research, *The Urban R's* as a whole is a mixed bag. Admittedly, the line between studies that successfully utilize local condition and those that are trivial or have overgeneralized results may be fine at times, but I believe that parts of *The Urban R's* are clearly on different sides of that line.

On the positive side, the volume contains research by Robert Dentler and Constance Elkins comparing intergroup attitudes and academic performance in naturally segregated and unsegregated schools, a paper by Dentler and Mary Ellen Warshauer on definitions of school segregation, and the description by Robert La Frankie of Englewood, New Jersey, as an ideal type of Northern desegregation crisis. In addition, the editors have made excellent introductory statements binding the various papers. To illustrate the negative side, there are, among other things, a statement based on a single illustration from teaching experience that teacher-student trust is the missing ingredient in slum schools; a report on the effects of a freedom school on attitudes, based on ten experimental subjects and twelve control subjects (with t tests performed, in places on N's of four and six); and a paper which asserts that there are class differences in sensi-

tivity to vocal emotion, even though the middle-class group consists of thirty white children in Hartford, Connecticut, and fifty-three Negro children in Harlem represent the lower-class group.

Representing a macroscopic approach to urban research, Harold Kaplan writes neither an evaluation of metropolitan government in general nor a flat description of the Metro Toronto system in *Urban Political Systems*. Instead, he demonstrates the utility of functional theory in explaining sources of success and failure in urban government using a case which has intrinsic interest as one of the few examples of metropolitan government that has thrived.

Kaplan adopts Parsonian functionalism, although he feels that much of Parsons is not invariably useful for the analysis of political systems (e.g., functional imperatives and normative integration as part of any social system). What he uses most fruitfully is Parsons' general approach to local government as a system, with relations to higher and lower levels of systems, comprised of actors playing roles. Kaplan stresses the integrative and adaptive *behavior* of those in specified roles and shows the importance of balance between the two to a functioning political system.

In chapter i, after lucidly presenting and then modifying Parsonian concepts, Kaplan demonstrates how his fastidiously systematic analysis makes more common elitist-pluralist distinctions appear oversimplified. Then, using data from interviews, written sources, and issue analysis, he directs his functionalist perspective on Metro Toronto.

The picture he paints I find fascinating. It is of a new federation of existing municipalities, founded in 1953, whose popular image of success is related to the unchanging anomic structure of the roles of Metro councilors, an informal cabinet system evolved by the appointed chairman of the government and the brain trust of administrators which his personal magnetism attracted, a Canadian political culture emphasizing a unitary view of the public interest and deference to expertise, the relation of metropolitan government to other levels of government, and a lack of strong interest groups. On the basis of his findings, Kaplan rejects the notion of conflict and integration as polar opposites in political systems and puts the latter in a central position, with conflict at one end and apathy at the other.

On balance, Kaplan's book leaves me far *happier than the other two. Yet its very close and justified attention to the effects of minute details in a system would suggest that no formal reforms in government would be successful without relying on social phenomena which* are utterly unpredictable. For example, much of the success of Metro Toronto can be tied in this analysis to the personality of the first chairman as well as to historical accident, even though Metro Toronto has had the enormous advantage of relative freedom from ethnic and racial tensions. Put in the perspective of the previous two books, it could make one pessimistic.

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The Moynihan Report and the Politics of Controversy. By Lee Rainwater and William L. Yancey. Cambridge, Mass.: M.I.T. Press, 1967. Pp. 493. $3.95.

I believe it was Robert E. Park who referred to some styles of sociology as "superjournalism." By that definition, Rainwater and Yancey have done an excellent job. In a thoughtful and literate manner they have given us what appears to be a thorough reporting of the social and political context within which the now famous Moynihan report appeared and an accounting and critique of the reactions that followed upon it. The book also contains the full text of the report, as well as a reprinting of significant examples of journalistic and intellectual commentary on it.

The book is a case study of a significant confrontation between contending forces during a crucial moment in the continuing drama of race relations in the United States. The Moynihan report appeared during a period when what was then the civil rights movement appeared to be reaching a cresting point. Civil injustice and the other inequities facing the Negro had become generally defined as a major issue confronting society. The executive leadership and hence the particular agencies of national government appeared more responsive to the problems of race relations than at any previous point in our history. The mass media were particularly sympathetic and attentive. More important than these, the civil rights movement, or movements, appeared to

have, and thought of itself as having, a special kind of potency and capacity to influence policy. All that was required was a viable program. Apparently, two factors intervened to transform what appeared to be a moment of impending success into feelings of despair, futility, and frustration. One was the increasing realization of the extensive major societal transformation, at the social and economic levels, that would be required to alter significantly the situation of the country's black population. This was the major and most disheartening political and economic message of the Moynihan report. The second factor was the impact of the escalating political and economic costs of a disastrous Vietnam policy upon Washington's mood and sense of available options. Less than three years later, it becomes clear that Rainwater and Yancey have provided a description of a turning point in the history of our society, a turning point from which still unlearned lessons must be learned.

There are three possible functions this work might have been designed to serve, and, unfortunately, none of the three are fully served—possibly because the aims of each limited the others. The first is as a work in contemporary history or sophisticated journalism. It is on this level that the book appears to work best. The authors interviewed a number of the important participants and appear to have carefully followed the chronology of events. Unfortunately, as with much history and almost all sophisticated journalism, the only check upon the authors is to follow their path, and since these paths quickly grow cold this is an exceedingly difficult task. A second fault—and a particularly irritating one—is the sparseness of description of their informants. Too much of such commentary is prefaced by, "A Department of Labor Official said, . . ." or, "As a long-time participant in the civil rights movement observed. . . ." Ironically, somewhere in the book there is a critical comment upon the unhappy practice of branches of government manipulatively releasing information through sources described only as "informed."

The second level upon which the book works is that of a study of the intersect between social science, formal government, pressure groups, and the public media during a period of policy formation involving a major social issue. On this level, the book provides a series of rich fragments and images of this complex process, but it is not pursued self-consciously. A crucial defect here again involves what was mentioned above: the authors' failure to take the reader into their confidence and tell him how they went about identifying and recruiting respondent-informants.

The third and last level is a consideration of the relevance of and uses of sociology in the formation of social policy. This is clearly the major interest of the authors. The importance of the Moynihan report for Rainwater and Yancey is that it represents one of the few but increasing number of instances where "sociological data" and, more importantly, "quasi-sociological analysis" were salient for what might loosely be called societal policy making on an important level. (Taking the term "policy making" too seriously leads too easily to the dubious assumption that ours is a society where there really are policies on major social issues. The present work leads to the conclusion that nothing could be further from the truth, that too often analyses give the appearance of rationality to governmental decision making when in fact there are only tactics without strategy.) The sections of the book that address themselves to this question are of the greatest general interest to sociologists. Thus, most sociologists—even those with little interest in the civil rights controversy—could find valuable the authors' consideration of problems of interpretation of the social statistics that formed the core of the Moynihan report. Similarly, the concluding section on the role of the social sciences and social scientists in the controversies of our society should properly become part of a continuing dialogue within our profession. They seem to argue for the need for good intentions and good science, and seem to accept wisely the hard truth that neither is a guarantor of the other.

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Prelude to Riot: A View of Urban America from the Bottom. By PAUL JACOBS. New York: Random House, 1967. Pp. x+298. $5.95.

Riots, Revolts and Insurrections. By RAYMOND M. MOMBOISSE. Springfield, Ill.: Charles C Thomas, 1967. Pp. xviii+523. $16.50.

The Urban League rated Los Angeles the best of sixty-eight cities in 1964 in terms of opportunities for Negroes. Certainly Watts was not a typically dilapidated slum area. One-third of its residents owned their own homes, and conditions were superior to those of Negroes in most other large cities. The Los Angeles police force boasted of its friendly relations with the Negro community. One week before the Watts riots in 1965, a police captain on the staff of Chief Parker told me in all sincerity that all was serene because the department's fine professional standard of police work engendered a spirit of trust and respect on the part of minority groups. Obviously, Chief Parker himself suffered from the same delusion. The wide avenues along which could be seen one-family houses with well-tended lawns made Watts a contemporary Potemkin village—the kind that enabled the power structure to preserve its complacency, although behind the pleasant façade social dynamite was ready to blow.

Paul Jacobs' *Prelude to Riot* documents the dismal failure of urban institutions to meet the needs of minority groups in Los Angeles. Thus, it is more than a prelude to riot. It also is a prelude to the report of the National Advisory Commission. The analysis of the Los Angeles Police Department was particularly arresting. There is something almost tragic in an organization that is highly efficient, technologically advanced, and incorruptible, whose men are loyal, courageous, and dedicated to a tradition of service to the public, and yet incapable of dealing with the problems arising from racial and economic tension. As Jacobs puts it, it may well be that the "primary source of the department's inadequacy is precisely the set of qualities of which it is so proud" (p. 22).

Law enforcement is not alone. The pattern of failure is repeated in welfare, housing, education, and employment services. Each bureaucracy reaches the point where its workers seem to say in all seriousness, "How efficiently we could function, if only we could make our clients disappear."

Jacobs weakens the strength of his criticisms when he reveals his own penchant for stereotyped reactions to the police, to whom he was lecturing on the subject of police-community relations: "They [the police] all seemed to have crew cuts, thick necks, and cold eyes. The white officers among them all seemed to be vulgar, crude, and loud, while the few Negroes and Mexican-Americans seemed so concerned with keeping the good will of their peers that they were even more vicious than the white officers in attacking their own" (p. 14). According to Jacobs, these men seemed to be a fairly typical cross-section of the occupation. Policemen, being American, probably share the racist ideology of their communities, but a serious author connected with the Center for the Study of Democratic Institutions ought to be immune from making a reciprocal attack of stereotyped abuse.

The second book, *Riots, Revolts and Insurrections*, is a comprehensive manual for police departments preparing for urban demonstrations and riots. It epitomizes the traditional police approach to crowd control and will probably become an important authority for strategy and tactics to police chiefs desperate for help.

Knowing this, one can only hope that police science has developed a more consistent and sophisticated theory of collective behavior than that proposed by Raymond M. Momboisse. He classifies crowds as casual, conventional, expressive, and hostile. Then he describes the first three types in terms of responding easily to police control, enjoying a ball game, or achieving a non-aggressive release from tension in dancing and singing. Social scientists might accept this popularization with some reservations, but I doubt if the following generalization would be equally palatable: "It is not necessary to dwell on the homicidal tendencies of the crowd. Every crowd is 'against someone.' Hence humanity's unhappy record of persecutions, wars, riots; hence the orgies of wanton killings which have stained the record of every revolution" (p. 31).

College students and teachers will find little comfort in the author's discussion of student demonstrations. For example, such demonstrators are labeled "the rebel group" (p. 320), and an "appreciable percentage of the faculty will be sympathetic to and encourage the rebels" (p. 321), and finally, after warning that communist-oriented attorneys will give continuous support to the students, he recommends that police infiltrate the college: "It is essential that personnel be assigned to maintain a continuous observation of campus activities. The identity and background of student organizers should be known. . . . Rallies

and meetings should be attended and published material carefully studied. If possible, contacts with responsible students should be established . . . (pp. 321–22).

Chapters on "City Battle Tactics" and "Counter-Insurgency Operations" round out the book. Police departments would be wise if they read this book in conjunction with the report of the National Advisory Commission on Civil Disorders that stressed the fact that in the riots during the summer of 1967 no single police tactic appeared to be effective in all situations and that, in particular, various squad formations were useless against mobile bands of rioters.

There is an ominous significance in the knowledge that a handbook of this type is really necessary. It portends a shift in the function and philosophy of the police in our society away from the concept of police work as a generalized service to the community, toward one where police become the equivalent of an army of occupation to prevent popular protest, especially by minority groups, be they Negroes, college students, or faculty.

ARTHUR NIEDERHOFFER

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The Disadvantaged Child. By MARTIN DEUTSCH and Associates. New York: Basic Books, 1967. Pp xii+400. $10.00.

Education Improvement for the Disadvantaged in an Elementary Setting. By GORDON P. LIDDLE, ROBERT E. ROCKWELL, and EVELYN SACADAT. Springfield, Ill.: Charles C Thomas, 1967. Pp. ix+116. $5.75.

The Drifters: Children of Disorganized Lower-Class Families. Edited by ELEANOR PAVENSTEDT. Boston: Little, Brown & Co., 1967. Pp. xvii+345. $10.50.

These three books present an impressive array of hypotheses, findings, and recommendations concerning the role of intervention in the early educational experience of disadvantaged children. The research in each case began in the early 1960's; all the authors express the regret that the long-range impact of their strategies cannot yet be determined, though this is what constitutes the ultimate test of the programs in question.

The primary focus of Deutsch and his associates was upon the effects of the social environment on the cognitive skills and school performance of nursery school and elementary school age children. Their aim was to discover the mediating variables which affect the relationship between background factors and psychological factors and then to formulate intervention programs to overcome the negative effects of the environment. This collection of papers, which was drawn from a series of closely related research projects at the Institute for Developmental Studies at New York University, contains a wealth of data on verbal, perceptual, and attentional deficiencies, on the one hand, and on social class and racially based handicaps on the other. Because the papers were originally prepared as independent contributions for various scholarly journals or conferences, there is some redundancy from one paper to another and a less coherent "plot" than in the other two volumes. Nevertheless, Deutsch and his co-workers evidence consistent concern with the role schools might and should play as agents of social change, and this concern is tempered with the realization that the public school system traditionally has not been organized or operated with such functions in mind. Since the school formed the locus of intervention in these studies, the degree of personal involvement of the researchers with the children was moderate, and a tone of cautious optimism pervaded the evaluative sections.

The aims of Liddle, Rockwell, and Sacadat's longitudinal study of an intervention program for disadvantaged children from prekindergarten through the early years of elementary school were to understand the child more fully, to stimulate the child intellectually by making better use of community resources and school facilities and materials, and to enlist the cooperation of the parents in helping to motivate the child. Two features in particular distinguish this research from that of Deutsch and that of Pavenstedt. First, this project was conducted in Quincy, Illinois, a city of forty-five thousand persons; in such a setting, the involvement of people from the wider community—whether businessmen, college students, or Girl Scouts—is probably a more realistic aspiration than it would be in a huge urban complex like New York City or Boston. The authors point out, however, the need for adequate supervision to give volunteers the emotional support necessary to maintain their

incentive to participate. The second distinctive feature was the use of an unselected sample of teachers. The rationale was that, although the novelty and effectiveness of the program might be reduced by this procedure, the applicability of the program to other school systems would be increased. In line with their expectations, the researchers found that positive changes could occur among culturally handicapped families and their children if the school were willing to reach out intensively to them.

Pavenstedt's research with thirteen severely disorganized families from a skid-row neighborhood in Boston was deliberately designed to include those who were the most disadvantaged of all and to provide intensive, hopefully therapeutic involvement with the families and their nursery school age children. Because of the extreme pathology of the parents, the impact of the intervention program was slight and the long-run prospects for the children seemed rather poor. For example, a major problem in the nursery school was not simply to motivate the children to develop and achieve, but to elicit their engagement in the first place. In spite of the pessimistic forecast attached to the particular subjects of this research, the authors' many insights into the psychological and social handicaps of the disadvantaged serve to increase our understanding of moderately disturbed families as well as those characterized by massive disorganization.

Each report indicates a basic confidence in the validity, efficacy, or necessity of a certain program of intervention in early education. However, it may be that their most significant contribution is not to our appreciation of intervention itself, but the much-needed corrective they provide for prevailing assumptions about disadvantaged children specifically and lower-class families generally.

Because of the emphasis during this decade on the problems of civil rights, racial equality, and ghetto schools, the phrase "disadvantaged child" immediately tends to conjure up the image of a lower-class Negro child trapped in an urban slum. Too readily and too frequently, perhaps, we equate the syndrome of inadequate family income, lack of environmental amenities, and deficiencies in school performance with particular racial characteristics. It was a matter of considerable interest, therefore, to find in both Pavenstedt's North Point Project and Liddle's work in the Midwest that a majority of the disadvantaged children studied were of white parentage, not black, and that Deutsch's research in New York City included about equal proportions of disadvantaged whites and Negroes. Although Deutsch gave some evidence to suggest that the racial factor may compound the problems of Negro children, it is also possible that the very facts of their greater visibility and the potentially explosive discontent of the black community may result in substantial efforts to overcome the deplorable conditions of the ghetto, while the marginal white schools and marginal white families remain forgotten and untouched, the genuinely "invisible poor" (see Michael Harrington's *The Other America* [New York: Macmillan Co., 1962] for an elaboration of this concept).

The other broad contribution, especially in the volumes edited by Pavenstedt and by Deutsch, was the delineation of the differences in life-style and extent of deprivation or family disorganization within the lower class. Even when the demographic aspects of social class, such as income, education, or occupational prestige, were the same, there were often subtle variations among families that served to intervene—for better or worse—between the disadvantaged position of the family and the natural potential of the children. Pavenstedt pointed out that far from being a homogeneous group, a slum population characteristically includes members of the working class, the employed poor, the unemployed poor, and the chronically dependent; no single program of educational enrichment would be suitable for all children from these disadvantaged, yet diverse, backgrounds. Pavenstedt's study also indicated that even the most pathologically disorganized families were not necessarily father-absent households, nor were the families necessarily supported by welfare. Instead, the combination of extreme emotional poverty and maternal deprivation stretching across several generations, pervasive devaluation of the self, and a sense of unpredictable external danger had a profoundly negative influence on the children's development—socially, psychologically, and intellectually.

The "Deprivation Index" devised by Deutsch and his associates offers a somewhat more systematic way of gauging the probable impact of a family's poverty on its children. The index included measures of housing dilapi-

dation, the educational aspiration of the parent for the child, the number of children in the home, the child's participation in conversation at dinner, the cultural experiences provided by the parent for the child, and the child's kindergarten attendance. Whether one is concerned with strategies of intervention in education in a narrow sense or very widely with the creation of a new social order, these refinements in the definition of "the poor" have major implications for the diagnosis of problems, the prescribed treatment, and the long-range prognosis.

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Crime and Punishment in Britain: An Analysis of the Penal System in Theory, Law and Practice. By NIGEL WALKER. Edinburgh: Edinburgh University Press, 1965. Pp. xiii+367. $13.50.

This important book presents a sophisticated, scholarly, and immensely informative description and analysis of the subject announced in the title, but it is in reality more than the title suggests, and might perhaps be better described as a treatise on modern crime and punishment with Britain taken as the example. The author exhibits a rare combination of theoretical interest, analytical skill, a comprehensive grasp of relevant research, and detailed knowledge of the workings of the British system. His book should be of great interest not only to criminologists in his own country but also to those of other countries and especially to Americans.

The book opens with an appraisal of the manner in which persons are picked up or not picked up for processing within the system of criminal justice, but does not include an analysis of the police system. The author comments on the nature of the criminal law and the main varieties of offenses which it defines or creates, devoting particular attention to acts that are defined as criminal in some countries and not in others, and to certain offenses which, although commonly included in the scope of the criminal law, should not be so included, according to some. These latter offenses include incest, euthanasia, bigamy, and bad driving by motorists. The author is skeptical of the possibility of finding any neat intellectual formula for determining what kind of behavior is a proper subject of the criminal law, and contends that it is impossible to reduce the number of functions performed by current prohibitions to less than the ten that he states.

The second chapter, "Trends and Patterns in Crime," presents statistical data concerning current trends, along with numerous reasons for not taking the available figures too seriously. This is followed by a long section of five chapters setting forth the various types of theories of crime causation, both sociological and non-sociological, that are currently "fashionable." This particular portion of the book comes closest to following the conventional line developed in most texts. The author ends this section with a chapter on "Explanation and Prediction," in which he notes that some contemporary criminologists who have become disenchanted with the search for the cause or causes of crime have "begun to abandon causal thinking for what is known as the predictive approach" (p. 110). The author identifies himself with this view.

Professor Walker is at his best in the final two-thirds of the book, in which he discusses the aims, assumptions, and current practices of imposing penalties and the manner in which the system deals with various categories of offenders. The categories considered are adults, young offenders, the mentally abnormal, female offenders, and recidivists. There is a chapter on the sentencing process and another on the efficacy of sentences. Each of the seven chapters in this section, and many of the others as well, provides broad, coherent, general pictures of the topics they consider and contain so much significant information and so many intriguing arguments that it is quite impossible to do justice to them in a brief review. The fact that British criminal statistics are centralized gives the author a tremendous advantage over any American scholar who might attempt to present a similar overview and evaluation of penal practices in the United States.

A point that is repeatedly made by the author, and one which seems of extraordinary interest and potential practical significance, has to do with the relative effectiveness of the various types of corrective measures utilized by British courts. Figures are cited from a

Home Office study of twenty-seven thousand male offenders to indicate that, when other relevant variables are controlled and effectiveness is judged by reconvictions, heavy fines appear to be consistently and markedly the most effective measure. All of the others, such as discharge, probation, borstal training, imprisonment, and so on, show disappointing results. Concerning these latter measures, Walker proposes that they are, for most prisoners, interchangeable. One of the surprising features of studies of this kind is that probation comes out quite badly. Following the logic of these findings, it is proposed that probation should cease to be regarded as the preferred way of handling first offenders. Instead, it is argued that first offenders, with some exceptions, should be fined or discharged; primary recidivists, or those with whom these mild and economical measures do not work, should be placed on probation; imprisonment should be reserved for third and subsequent offenders.

A brief epilogue summarizes the author's views of major current trends and suggests desirable changes in the penal system. The value of the extensive bibliography is enhanced by supplementation of the references to major investigations with brief descriptions of the nature of their samples.

The book is a major contribution to the literature of modern penology and deserves wide reading and careful study.

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Peer Influences on Aspirations: A Reinterpretation[1]

Otis Dudley Duncan, Archibald O. Haller, and Alejandro Portes

ABSTRACT

The hypothesized influence of a best friend's educational and occupational aspirations on the formation of ego's aspirations and the reciprocal influence in the opposite direction can be represented in path diagrams and equation systems involving simultaneous or jointly dependent variables. Standard methods of path analysis may be extended along lines suggested by work in econometrics to secure estimates of coefficients when the models are just identified or overidentified. The most plausible interpretation was reached with a model postulating an underlying factor, "ambition," that is involved in such reciprocal influences and accounts for the correlations among the observed indicators, educational aspirations and occupational aspirations. Homophily with respect to background characteristics does not fully explain homophily with respect to ambition; a path coefficient of about 0.2 runs from friend's to respondent's ambition, and one of equal size runs in the opposite direction.

The hypothesis that interaction with peers influences levels of educational and occupational aspirations of adolescents, enunciated by Haller and Butterworth in 1960,[2] has proved to be an intriguing one, to judge by the attention given it subsequently. Without mentioning the several studies of "school climates," wherein the hypothesis is more or less assumed to be correct in order to interpret ostensible "school effects," we may refer to studies specifically directed to tests of the hypothesis itself or one of several closely related hypotheses which have also been subjected to scrutiny.[3]

To summarize our present knowledge: (1) The supposition that homophily with respect to socioeconomic characteristics is generated by socioeconomic segregation of school populations was considered by Rhodes, Reiss, and Duncan,[4] who were able to show that a minor part of the observed homophily is due to school segregation, the greater part to assortative choices of friends within schools. (2) In their study, Haller and Butterworth were concerned to eliminate socioeconomic homophily as a complete explanation of similarity in aspirations of friends. Hence, they considered peer-pairs whose members were alike in social class background and with respect to level of parental aspiration for their sons, as well as in measured intelligence. They showed that a positive intraclass correlation of close friends' aspirations held within such homogeneous pairs. (3) Invoking balance theory, Alexander and Campbell observed that agreement between friends'

[1] This is a report from Project 5-0074 (EO-191), "Socioeconomic Background and Occupational Achievement: Extensions of a Basic Model," carried out under Contract OE-5-85-072 with the U.S. Office of Education. The assistance in computation of Ruthe C. Sweet is gratefully acknowledged.

[2] A. O. Haller and C. E. Butterworth, "Peer Influences on Levels of Occupational and Educational Aspiration," *Social Forces*, XXXVIII (May, 1960), 289–95.

[3] C. Norman Alexander, Jr., and Ernest Q. Campbell, "Peer Influences on Adolescent Educational Aspirations and Attainments," *American Sociological Review*, XXIX (August, 1964), 568–75; Ernest Q. Campbell and C. Norman Alexander, "Structural Effects and Interpersonal Relationships," *American Journal of Sociology*, LXXI (November, 1965), 284–89; M. Richard Cramer, "The Relationship between Educational and Occupational Plans of High School Students" (presented at the 1967 meeting of the Southern Sociological Society).

[4] Albert Lewis Rhodes, Albert J. Reiss, Jr., and Otis Dudley Duncan, "Occupational Segregation in a Metropolitan School System," *American Journal of Sociology*, LXX (May, 1965), 682–94, and LXXI (July, 1965), 131.

plans and desires to attend college was greater when the friendship choice was reciprocated than when it was not.[5] (4) In a second analysis, the same writers showed that "structural effects" of school socioeconomic composition were mediated by individual status effects, in that the former disappeared when the latter were taken into account: "Given knowledge of an individual's immediate interpersonal influences, the characteristics of the total collectivity provide no additional contribution to the prediction of his [college plans]."[6] (5) While Cramer notes an appreciable frequency of extreme incongruity between educational plans and occupational aspirations, the correlation between the two variables is relatively high, and too high to be explained fully by the operation of background factors as common causes.[7]

That "structural effects" are no large part of the explanation of similarity in friends' aspirations is easily demonstrated by an even more straightforward method than that used by Campbell and Alexander. For illustration, consider the correlation of .4986 between educational plans and friend's plans obtained by William H. Sewell (unpublished) for an all-Wisconsin sample of 4,386 boys who were high school seniors in 1957. Sewell found a correlation of .3364 between respondent's plans and the percentage of students in his class planning to go to college and similarly a correlation of .3327 between friend's plans and percentage going. (This is formally the same as the correlation ratio of individual plans on school.) Hence, if "structural effects" explained the correlation between friends, the latter would have been (.3364)(.3327) = .1119, which falls short of the observed correlation by .3867. Stated otherwise, the average within-school correlation between plans and friend's plans was .4354 (closely comparable to figures cited below for boys in a single school district).

[5] Alexander and Campbell, *op. cit.*

[6] Campbell and Alexander, *op. cit.*, p. 288.

[7] Cramer, *op. cit.*

The evidence is clear, therefore, that neither status homophily nor similarity in aspirations of friends is close to being adequately explained by school "structural effects" or school segregation. Yet the latter factors are important enough that they must be taken into account in estimating the impact of peer interaction on the formation of aspirations. By limiting the inquiry to a single school (or, alternatively, by looking at average within-school relationships), one can eliminate these factors from the analysis.

There remains the question of how to estimate the magnitude of peer influence, a problem not tackled directly in the studies cited, where the authors were content to stop with the detection of significant relationships indicating that a non-spurious correlation between aspirations (or plans) and friend's aspirations actually exists. The estimation (as distinct from detection) of such effects appears to be a particular instance of a generic problem for which explicit explanatory models have not yet been proposed. The purpose of this report is to suggest the possibilities in one kind of model. The purpose is realized with a presentation of some possible reinterpretations of the data originally collected and analyzed by Haller and Butterworth in the paper already cited.

DATA

The original sample consisted of all seventeen-year-old boys in school in Lenawee County (Michigan) during the spring of 1957, interviews and test data being secured for 442 persons. For 329 of these boys, data were included in the same sample for at least one person listed as a best friend. Whether the friendship was reciprocated is not considered in this analysis. Some of the friends are, of course, included among the 329 respondents identified by this procedure, but others are not. It is important to note that the data on social and psychological characteristics as well as aspirations were obtained directly from the

friend and not via the respondent's report on his friend.

The five variables to be considered are briefly enumerated (for lengthier descriptions, see the original publication):[8] *Level of occupational aspiration* is the score on Haller and Miller's Occupational Aspiration Scale. *Level of educational aspiration* is a composite score based on several questions about the number of years of college or university training the respondent planned to complete. *Socioeconomic status* (*SES*) is measured by Sewell's socioeconomic status scale, which includes items of parental educational attainment as well as material possessions in the home. *Intelligence* refers to raw scores on Cattell's Test of G Culture Free. *Parental aspirations* are indexed by a composite score from the answers to questions asked about the degree to which parents "encouraged" the respondent to have high levels of achievement. The distribution of each of these variables was normalized as a preliminary step to all further calculations.

[8] See also Archibald O. Haller and Irwin W. Miller, *The Occupational Aspiration Scale: Theory, Structure and Correlates* (Technical Bulletin 288 [East Lansing: Agricultural Experiment Station, Michigan State University, 1963]).

The intercorrelations of these variables are presented in Table 1. For comparison, the same table includes the correlations derived from the complete data on all 442 boys. It appears that the correlations either for friends or for respondents are fairly representative of those obtaining within the whole population.

TABLE 1

OBSERVED CORRELATIONS FOR 329 RESPONDENTS AND THEIR BEST FRIENDS (ABOVE DIAGONAL), AND "SYNTHETIC" CORRELATIONS, INCLUDING OBSERVED CORRELATIONS FOR 442 RESPONDENTS (BELOW DIAGONAL)

Variable	Symbol	X_a	X_b	X_c	Y_1	Y_2	X_d	X_e	X_f	Y_3	Y_4
Respondent:											
Intelligence	X_a	.....	.1839	.2220	.4105	.4043	.3355	.1021	.1861	.2598	.2903
Parental aspiration	X_b	.16*	......	.0489	.2137	.2742	.0782	.1147	.0186	.0839	.1124
Family SES	X_c	.23*	.09*	......	.3240	.4047	.2302	.0931	.2707	.2786	.3054
Occupational aspiration	Y_1	.45*	.22*	.37*	......	.6247	.2995	.0760	.2930	.4216	.3269
Educational aspiration	Y_2	.41*	.29*	.41*	.64*	......	.2863	.0702	.2407	.3275	.3669
Best friend:											
Intelligence	X_d	.34	.09	.21	.28	.29	......	.2087	.2950	.5007	.5191
Parental aspiration	X_e	.09	.11	.06	.08	.09	.16*	......	−.0438	.1988	.2784
Family SES	X_f	.21	.06	.27	.29	.27	.23*	.09*	....	.3607	.4105
Occupational aspiration	Y_3	.28	.08	.29	.42	.33	.45*	.22*	.37*	......	.6404
Educational aspiration	Y_4	.29	.09	.27	.33	.37	.41*	.29*	.41*	.64*	......

* Observed correlations for 442 respondents; remaining correlations below diagonal are obtained by averaging correlations above diagonal, as explained in the text.

MODELS

It is easy to demonstrate that neither socioeconomic homophily nor friendship assortment by intelligence, nor the combination of the two, suffices to account for the correlation between respondent's and friend's aspirations. For example, the correlation between the two occupational aspirations is given in Table 1 as .42. In a multiple regression of respondent's occupational aspiration on friend's occupational aspiration, with the intelligence, parental aspiration, and SES of both boys included as additional independent variables, the standardized net regression coefficient (β coefficient) is a highly significant .26. If the regression is turned around, with friend's aspiration as the dependent variable and respondent's aspiration as one of the seven independent variables, its coefficient is .24.

While these results demonstrate that

there is a net relationship between respondent's and friend's aspirations with homophily taken into account, the method by which these regression coefficients are estimated is not acceptable if we postulate both a causal influence of friend's on respondent's aspiration and vice versa. If friend's aspiration influences respondent's, it is illogical to use a model in which the latter is an explanatory variable in accounting for the former without reckoning with the reciprocal influence of the former upon the latter. Given that only one observation is made upon each aspiration, presumably at a stage when both have become relatively crystallized, we must think of the two dependent variables as being simultaneously determined, each being influenced by the other as well as by the remaining variables in the model.

Some exposition of simultaneous models, or models incorporating reciprocal influences, appears in the literature on path analysis.[9] Models of this type have been extensively considered in econometrics.[10] It is clear from the discussion in both these contexts that straightforward regression of one dependent variable upon a set of predictor variables does not yield the proper estimate of the effect of a variable which is simultaneously being determined within the model. Our interpretation here is presented in the framework of path analysis, but takes advantage of some of the approaches developed in econometrics. The reader is referred to a previous discussion of path analysis in sociological research,[11] with the warning that some of the simplified algorithms for path diagrams stated there apply only to simple recursive systems and not to simultaneous systems. This paper, then, affords an introduction to and illustration of the treatment of such systems in a path framework. Emphasis is placed on explicit statement of the equations and specifications of the model and the derivations that may be made therefrom to secure equations from which estimates may be calculated.

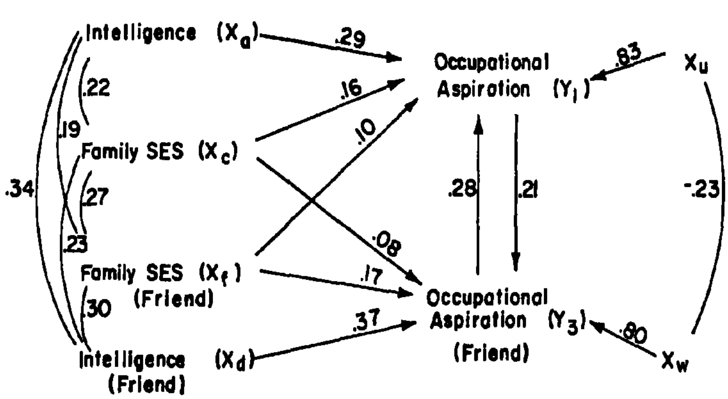


FIG. 1.—Model I

[9] Sewall Wright, "The Treatment of Reciprocal Interaction, with or without Lag, in Path Analysis," *Biometrics*, XVI (September, 1960), 423–45.

[10] J. Johnston, *Econometric Methods* (New York: McGraw-Hill Book Co., 1963), chap. ix; Arthur S. Goldberger, *Econometric Theory* (New York: John Wiley & Sons, 1964), chap. vii; E. Malinvaud, *Statistical Methods of Econometrics* (Chicago: Rand McNally & Co., 1966), Part V; R. L. Basmann, "An Expository Note on Estimation of Simultaneous Structural Equations," *Biometrics*, XVI (September, 1960), 464–80. For a more elementary presentation, Mordecai Ezekiel and Karl A. Fox, *Methods of Correlation and Regression Analysis* (3d ed.; New York: John Wiley & Sons, 1959), chap. xxiv. See also Everett R. Dempster, "The Question of Stability with Positive Feedback," *Biometrics*, XVI (September, 1960), 481–83.

[11] Otis Dudley Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, LXXII (July, 1966), 1–16.

A JUST-IDENTIFIED MODEL

Several models will be presented for didactic purposes before we arrive at one that represents a reasonably firm though tentative interpretation. Model I (Fig. 1) represents occupational aspiration as depending directly on intelligence, family SES, friend's family SES, friend's occupational aspiration, and unspecified residual factors (or "disturbance," in the econometrician's parlance). The crucial assumption of the model is that occupational aspiration does *not* depend *directly* on friend's intelligence. Occupational aspiration does, however, depend directly on friend's family SES, an assumption that seems plausible enough if we consider the possibility that members of the friend's family as well as one's own may afford role models.

Three classes of variables are distinguished. The variables to the left, denoted by X's with letter subscripts taken from the first part of the alphabet, are "predetermined variables" with respect to this model. (The same variables might, of course, occur in a different role in some other model.) The Y's, with numerical subscripts, are the "endogenous variables," determined within the model, sometimes also called "jointly dependent variables." The X's with letter subscripts drawn from the end of the alphabet are "disturbances," standing for the residue of factors influencing aspiration but not explicitly measured.

The essential specification for all models of this type is that the disturbances are uncorrelated with the predetermined variables, though not (of course) with the endogenous variables and not necessarily with each other. Thus $r_{au} = r_{cu} = r_{fu} = r_{du} = r_{aw} = r_{cw} = r_{fw} = r_{dw} = 0$. (We use r_{au} as a more economical notation for $r_{X_aX_u}$, etc.)

The intercorrelations of the predetermined variables are represented by the curved lines connecting them; similarly, there is a correlation between the two disturbances represented by a curved line. (The arrowheads at both ends of such curved lines, which are conventional in path diagrams, are omitted here to suggest that one must not rely on the usual algorithm for reading compound paths from the diagram when the system is non-recursive.)

While the diagram, on the conventions understood for reading it, specifies the model adequately, it is essential that the model also be stated explicitly in equations. Model I comprises two equations:

$$Y_1 = p_{1a}X_a + p_{1c}X_c + p_{1f}X_f + p_{12}Y_2 + p_{1u}X_u$$

and (Model I)

$$Y_2 = p_{2d}X_d + p_{2f}X_f + p_{2c}X_c + p_{21}Y_1 + p_{2w}X_w.$$

To these equations must be attached the earlier stipulation that disturbances are uncorrelated with predetermined variables.

To calculate estimates of the path coefficients, we must derive equations that yield a solution for them. To do this, we take advantage of the condition concerning the zero correlation of the predetermined variables and disturbances. Consider the first equation of the Model I. Multiply both sides of the equation by X_a, sum both sides over sample observations, and divide both sides by N, the number of sample observations. This yields

$$\frac{\Sigma Y_1X_a}{N} = p_{1a}\frac{\Sigma X_aX_a}{N} + p_{1c}\frac{\Sigma X_cX_a}{N} + p_{1f}\frac{\Sigma X_fX_a}{N} + p_{12}\frac{\Sigma Y_2X_a}{N} + p_{1u}\frac{\Sigma X_uX_a}{N}.$$

Since we are dealing with standardized variables (zero mean, unit variance), an expression like $\Sigma Y_1X_a/N$ simplifies immediately to r_{1a} while $\Sigma X_aX_a/N = 1$. Note especially that $\Sigma X_uX_a/N = r_{ua} = 0$, on the specification already stated. We proceed to multiply the first equation through by each of the four predetermined variables and to simplify the results as just indicated. This

work yields the following set of four equations:

$$r_{1a} = p_{1a} + p_{1c}r_{ac} + p_{1f}r_{af} + p_{13}r_{3a},$$
$$r_{1c} = p_{1a}r_{ac} + p_{1c} + p_{1f}r_{cf} + p_{13}r_{3c},$$
$$r_{1f} = p_{1a}r_{af} + p_{1c}r_{cf} + p_{1f} + p_{13}r_{3f},$$
$$r_{1d} = p_{1a}r_{ad} + p_{1c}r_{cd} + p_{1f}r_{df} + p_{13}r_{3d}\,.$$

Or, in matrix form,

$$\begin{pmatrix} 1 & r_{ac} & r_{af} & r_{3a} \\ r_{ac} & 1 & r_{cf} & r_{3c} \\ r_{af} & r_{cf} & 1 & r_{3f} \\ r_{ad} & r_{cd} & r_{df} & r_{3d} \end{pmatrix} \begin{pmatrix} p_{1a} \\ p_{1c} \\ p_{1f} \\ p_{13} \end{pmatrix} = \begin{pmatrix} r_{1a} \\ r_{1c} \\ r_{1f} \\ r_{1d} \end{pmatrix}.$$

It will be noted that the square matrix on the left is not symmetric, unlike the case of the normal equations in conventional regression. Nevertheless, it will almost always be possible to invert this matrix (barring excessive collinearity among the predetermined variables) and thus to solve the four equations for the four unknown path coefficients, all the correlations on both the left- and right-hand sides of the equations being given in the data.

That we have exactly four equations to solve for four unknowns is essentially what is meant by saying that the model, or, more particularly, the relation in which Y_1 figures as the dependent variable, is "just identified." (The same holds, in this model, for the relation in which Y_3 is the dependent variable, but it need not always be the case that the situation in regard to identifiability is the same with respect to all relations in the model.) The econometricians have much more elegant ways of analyzing identifiability, but what it comes down to in cases like the one at hand is this; Y_1 depends on four variables explicitly (leaving aside the disturbance term), and there is just the same number of *predetermined* variables in the model. If there were four explanatory variables for Y_1 but fewer predetermined variables in the model, then the relation for Y_1 would be "underidentified"; that is, we could not derive a sufficient number of equations to yield a unique solution for the estimated paths leading to Y_1 (at least, not without some alteration in the specification of the model). On the other hand, if there are more predetermined variables than there are explanatory variables occurring in the model's equation for Y_1, this relation would be "overidentified" (examples of overidentification are given below).

The same procedure can be followed to secure equations from which we may solve for the paths leading to Y_3; that is, the second equation of the model is multiplied through, in turn, by each of the four predetermined variables, and the results are simplified to yield four equations in known correlations and four unknown path coefficients.

The method just presented yields exactly the same results as does the method of "indirect least squares," which is applicable to just-identified relations in a model. Computationally, however, it is a considerable simplification over indirect least squares, as has been noted in at least one textbook of econometrics.[12]

Some tedious but straightforward work remains if we are to calculate residual paths and the correlation between disturbances. We now multiply through each of the model equations by each of the endogenous variables and by each of the disturbance variables. This yields eight equations, which take the following form upon simplification:

$$r_{11} = 1 = p_{1a}r_{1a} + p_{1c}r_{1c} + p_{1f}r_{1f}$$
$$+ p_{13}r_{13} + p_{1u}r_{1u};$$
$$r_{13} = p_{1a}r_{3a} + p_{1c}r_{3c} + p_{1f}r_{3f} + p_{13}$$
$$+ p_{1u}r_{3u};$$
$$r_{33} = 1 = p_{3d}r_{3d} + p_{3f}r_{3f} + p_{3c}r_{3c}$$
$$+ p_{31}r_{13} + p_{3w}r_{3w};$$
$$r_{13} = p_{3d}r_{1d} + p_{3f}r_{1f} + p_{3c}r_{1c} + p_{31}$$
$$+ p_{3w}r_{1w};$$
$$r_{1u} = p_{13}r_{3u} + p_{1u}; \quad \text{hence} \quad p_{1u}^2 = p_{1u}r_{1u}$$
$$- p_{13}p_{1u}r_{3u};$$

[12] Goldberger, *op. cit.*, p. 348.

$$r_{3w} = p_{31}r_{1w} + p_{3w}; \quad \text{hence} \quad p_{3w}^2 = p_{3w}r_{3w} - p_{31}p_{3w}r_{1w};$$

$$r_{3u} = p_{31}r_{1u} + p_{3w}r_{uw};$$

$$r_{1w} = p_{13}r_{3w} + p_{1u}r_{uw}.$$

A convenient solution routine is to compute $p_{1u}r_{1u}$ from the first equation (all other terms in it now being known), $p_{1u}r_{3u}$ from the second, $p_{3w}r_{3w}$ from the third, and $p_{3w}r_{1w}$ from the fourth. The values thus obtained can be substituted into the fifth and sixth equations to compute p_{1u} and p_{3w}, and it is then possible to obtain r_{1u}, r_{3w}, r_{3u}, and r_{1w} by a back solution. Either of the last two equations can then be used to obtain r_{uw}; if both are used, one has a partial check on the arithmetic.

Although it is advisable to retain a generous number of decimal places in the initial correlations and all intermediate calculations, the results are not likely to be meaningful for more than two places; the rounded estimates are shown in Figure 1.

Model I was also used with educational aspiration in place of occupational aspiration; that is, Y_2 replaced Y_1 and Y_4 replaced Y_3, with the predetermined variables and specifications remaining unchanged. For comparison, the results are as follows:

$$p_{2a} = .27, \quad p_{4c} = .04, \quad p_{2v} = .84,$$
$$p_{2e} = .26, \quad p_{4f} = .22, \quad p_{4z} = .79,$$
$$p_{2f} = .02, \quad p_{4d} = .36, \quad r_{vz} = -.38,$$
$$p_{24} = .25, \quad p_{42} = .29,$$

where X_v and X_z are the disturbances for Y_2 and Y_4, respectively.

The two sets of results are similar in most respects. Intelligence and family SES are appreciable influences on aspiration, while the influence of friend's family SES is barely detectable. The reciprocal paths of influence of friend's aspiration on respondent's aspiration, and vice versa, are around .2 or .3, by no means a negligible value. In both sets of results, consistent estimates are obtained only by acknowledging a rather substantial negative correlation between the disturbances of the two aspiration variables. If the analyst feels uncomfortable with the size of this correlation, as one may well feel in the absence of any evident rationalization of it, he may be inclined to reject the model even though the remaining estimates are reasonable.

Several things could be awry. Perhaps it is mistaken to argue that friend's intelligence can have no direct influence on one's aspiration (even though insertion of an additional path for this variable in Model I will render it underidentified). Perhaps there is a variable (such as "ambition," introduced into Model IV) omitted from the model with respect to which there is pronounced homophily and which is a significant cause of aspiration. In this event, the paths between the aspiration variables are probably overestimated, and the residual correlation is forced to compensate for this. In the present state of theory in social psychology, we are not likely to have firm grounds for asserting the validity of a model on grounds completely independent of the data for a given problem. Hence, it would seem that the best we can do is propose reasonable models, consider their plausibility, and, where indicated, undertake the construction of alternative ones (or await the work of a critic who may do so). Several alternatives to Model I were in fact attempted which yielded even less satisfactory results, but this does not prove that still another alternative could not be plausibly proposed.

AN OVERIDENTIFIED MODEL

One such alternative is instructive, both as an example of estimation procedure when one is confronted with overidentification and as an indication of the sensitivity of the results to what may appear to be minor modifications of the model. In Model II (Fig. 2), we delete the path from friend's family SES to aspiration (and vice versa), suspecting on the basis of results with Model I that this variable is not very consequential. With this deletion, the equations are:

$$Y_1 = p_{1a}X_a + p_{1c}X_c + p_{13}Y_3 + p_{1u}X_u$$

and (Model II)

$$Y_3 = p_{3d}X_d + p_{3f}X_f + p_{31}Y_1 + p_{3w}X_w .$$

As before, the predetermined variables are specified to be uncorrelated with the disturbances, but the latter two variables may be correlated with each other.

The identification problem is now clear. If we use all our predetermined variables as before, we obtain four equations (for r_{1a}, r_{1c}, r_{1f}, and r_{1d}) in three unknowns (p_{1a}, p_{1c}, and p_{13}). The solution obtained from any three of these equations will, in general, not satisfy the fourth. Indirect least squares, or the equivalent procedure described above, is not available as a method of estimation. We resort to a procedure suggested by the econometrician's method of "two-stage least squares." The modifications of that method introduced here are required by the facts that we are working with standardized variables and that the correlation matrix of the observed variables constitutes the initial data of the problem.

We first compute a "first-stage regression" of Y_1 on all the predetermined variables; since we are working from correlations, the regression coefficients come out as β coefficients:

$$Y_1 = \beta_{1a\cdot cfd}X_a + \beta_{1c\cdot afd}X_c + \beta_{1f\cdot acd}X_f + \beta_{1d\cdot acf}X_d + r_{1s}X_s ,$$

where X_s is the residual for this regression. Now, let Y_1^* stand for the sum of the first four terms on the right, *after* that sum has been expressed in units of its own standard deviation, so that

$$Y_1 = r_{Y_1Y_1^*}Y_1^* + r_{1s}X_s .$$

If we multiply this equation through by Y_1 and simplify, we obtain $r_{11} = 1 = r^2_{Y_1Y_1^*} + r^2_{1s}$, which is the usual partitioning of total variance into "explained" and "unexplained," with $r^2_{Y_1Y_1^*}$ being the coefficient of determination, $R^2_{1(acfd)}$.

The two-term expression for Y_1 obtained from this first-stage regression is now substituted into the second equation of Model II in place of Y_1:

$$Y_3 = p_{3d}X_d + p_{3f}X_f + p_{31}r_{Y_1Y_1^*}Y_1^* + p_{31}r_{1s}X_s + p_{3w}X_w .$$

In the second stage, we work with the correlations of Y_3 with X_f, X_d, and Y_1^*, in effect, carrying out an ordinary regression of Y_3 on the latter set of variables. As before, the equation is multiplied through by the relevant variable and simplified. However, we now take advantage not only of the zero correlation of predetermined variables with the disturbances of the model, but also of the following property of least-squares regression: $r_{fs} = r_{ds} = 0$, since X_f and X_d were predictor variables in the regression for which X_s is the residual. Moreover, $r_{X_sY_1^*} = 0$, likewise as a property of

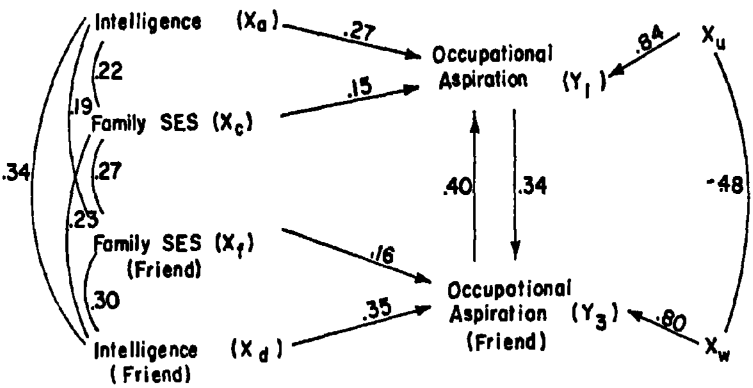


FIG. 2.—Model II

least-squares regression. Finally, since Y_1^* is a linear combination of X_a, X_c, X_f, and X_d, all of which are uncorrelated with the disturbance X_w, we have $r_{X_w Y_1^*} = 0$. From all this it follows that we can ignore the last two terms in the equation for Y_3 when we multiply through by each of the three variables in turn. Hence, we obtain:

$$r_{3d} = p_{3d} + p_{3f}r_{df} + p_{31}r_{Y_1Y_1^*}r_{X_dY_1^*};$$

$$r_{3f} = p_{3d}r_{df} + p_{3f} + p_{31}r_{Y_1Y_1^*}r_{X_fY_1^*};$$

$$r_{Y_3Y_1^*} = p_{3d}r_{X_dY_1^*} + p_{3f}r_{X_fY_1^*} + p_{31}r_{Y_1Y_1^*}\,.$$

A further simplification follows from the fact (easily proved) that $r_{Y_1Y_1^*}r_{X_dY_1^*} = r_{X_dY_1^*}$ (or r_{1d} in the more economical notation) and $r_{Y_1Y_1^*}r_{X_fY_1^*} = r_{1f}$. The third equation is simplified by multiplying both sides by $r_{Y_1Y_1^*}$, so that the equations to be solved can be rewritten

$$r_{3d} = p_{3d} + p_{3f}r_{df} + p_{31}r_{1d};$$

$$r_{3f} = p_{3d}r_{df} + p_{3f} + p_{31}r_{1f};$$

and

$$r_{Y_1Y_1^*}r_{Y_3Y_1^*} = p_{3d}r_{1d} + p_{3f}r_{1f} + p_{31}R^2_{1(acfd)}\ ;$$

it only is necessary to compute $r_{Y_1Y_1^*}r_{Y_3Y_1^*} = \beta_{1a\cdot cfd}r_{3a} + \beta_{1c\cdot afd}r_{3c} + \beta_{1f\cdot acd}r_{3f} + \beta_{1d\cdot acf}r_{3d}$ before solving these three equations for the unknowns, p_{3d}, p_{3f}, and p_{31}. The first two of these equations, of course, could have been obtained in a less roundabout way, but it was desirable to indicate their connection with the two-stage method.

The same procedure, with appropriate interchanges of variables, is next used to estimate p_{1a}, p_{1c}, and p_{13} in the first equation of Model II, after executing a first-stage regression of Y_3 on all the predetermined variables. We now have estimates of all the path coefficients in the model save those for the disturbance variables. At this point we may consider the correlations of the endogenous with the predetermined variables that are implied by these estimates, since the method of estimation did not constrain these correlations to equal their observed values. If we multiply the first equation of the model through by X_a and simplify the result, we obtain

$$r'_{1a} = p_{1a} + p_{1c}r_{ac} + p_{13}r_{3a},$$

where the prime distinguishes the implied from the observed correlation, r_{1a}. Similarly, the remaining seven implied correlations are obtained by multiplying through the model equations by each predetermined variable in turn. We show below the implied correlations computed from such formulas (with their deviations from the observed correlations in parentheses):

$$r'_{1a} = .4235(.0130), \quad r'_{3d} = .5120(.0113),$$

$$r'_{1c} = .3032(-.0208), \quad r'_{3f} = .3385(-.0222),$$

$$r'_{1f} = .2281(-.0649), \quad r'_{3c} = .2271(-.0515),$$

$$r'_{1d} = .3327(.0332), \quad r'_{3a} = .2921(.0323)\,.$$

It will be noted that the largest deviations are for r_{1f} and r_{3c}, while it was p_{1f} and p_{3c} that we deleted from Model I to construct Model II. That this deletion was perhaps inadvisable is suggested, as well, by the implausibly high values for p_{13} and p_{31} (.40 and .34, respectively), compensated by the substantial negative correlation between disturbances, $r_{uw} = -.48$.

The calculation of that correlation, as well as the paths for the disturbances, is effected in the same way that was illustrated for Model I. Here, however, for consistency we must be careful to use the implied correlations of the model in calculating the residual paths.

A PARTIALLY RECURSIVE MODEL

Both models discussed thus far are "simultaneous" models in that the endogenous variables are jointly determined by the model within a single period of observation. In simple recursive models, by contrast, we assume that the endogenous variables are successively determined, and the specification is altered to exclude correlation of the disturbances among themselves. The latter specification (zero intercorrelation of disturbances) is common to the Simon-Blalock procedure of causal analysis and to the examples of stratification models given in a previous publication

on path analysis.[13] There is no apparent reason, however, why features of both types of model cannot be combined in a single construction. (The term "construction" might well be preferred to "model" in contexts like the present one, to emphasize that we are indeed "construing" the data to mean what our interpretation via a diagram or system of equations represents them to mean.)

In Model III (Fig. 3), we reason as though respondents and their friends "first" make up their minds what occupations they would like to pursue, and "then," on the basis of the occupational choice and other considerations, "decide" what educational preparation they may need. That such a construction is at best an oversimplification may be evident from introspection. This model, like the preceding ones, serves primarily a didactic purpose.

In the first sector of the model, occupational aspirations Y_1 and Y_3 are simultaneous endogenous variables and the specification is exactly the same as for Model I. We then assume that with respect to educational aspiration (Y_2 and Y_4) the predetermined variables include not only X_a, X_c, X_f, and X_d but also Y_1 and Y_3. Hence the specification $r_{1v} = r_{3u} = r_{uv} = r_{wz} = 0$; but r_{uz}, r_{vw}, r_{uw}, r_{vz}, r_{3v}, and r_{1z} are not necessarily zero. The equations of the model are

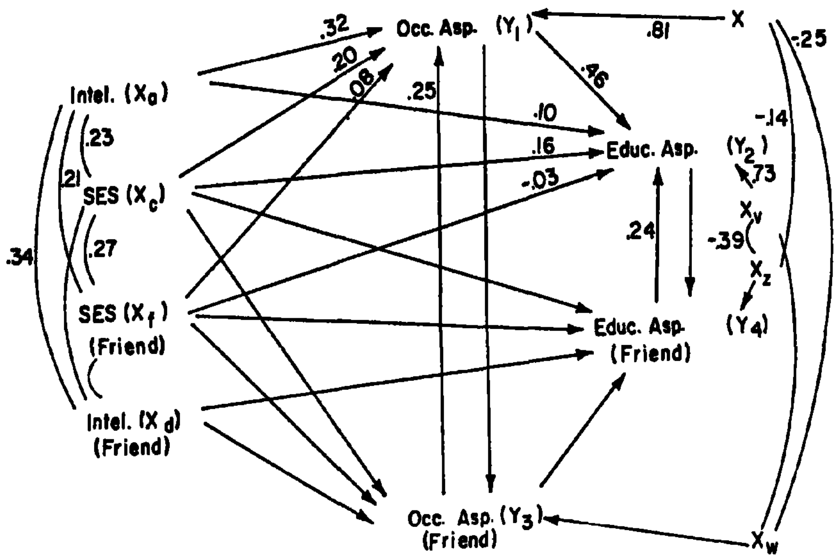


FIG. 3.—Model III

$$Y_1 = p_{1a}X_a + p_{1c}X_c + p_{1f}X_f + p_{13}Y_3 + p_{1u}X_u ,$$

$$Y_3 = p_{3d}X_d + p_{3f}X_f + p_{3c}X_c + p_{31}Y_1 + p_{3w}X_w ,$$

$$Y_2 = p_{2a}X_a + p_{2c}X_c + p_{2f}X_f + p_{21}Y_1 + p_{24}Y_4 + p_{2v}X_v , \quad \text{(Model III)}$$

$$Y_4 = p_{4d}X_d + p_{4f}X_f + p_{4c}X_c + p_{43}Y_3 + p_{42}Y_2 + p_{4z}X_z .$$

In estimating the parameters of this model, we have engaged in a preliminary manipulation of the data which has nothing to do with the properties of the model but which is suggested by the somewhat artificial design of the data matrix. From the original correlation matrix in Table 1, we

[13] Hubert M. Blalock, Jr., *Causal Inferences in Non-experimental Research* (Chapel Hill: University of North Carolina Press, 1964); Herbert A. Simon, *Models of Man* (New York: John Wiley & Sons, 1957), chap. ii; Duncan, *op. cit.*

constructed a "synthetic" correlation matrix which was forced to be symmetrical in variables pertaining to respondent and friend. The intercorrelations of educational aspiration, occupational aspiration, family SES, and intelligence were assumed to be the same for respondents and friends, and the values thereof were assumed to be those observed in the entire original sample of 442 boys. The correlation between friend and respondent on each of these variables was retained from the data on the 329 pairs. The "cross-correlations" between friend and respondents were averaged; thus, for example, in the synthetic correlation matrix $r_{1a} = r_{3d}$ and each is the average of r_{1a} and r_{3d} as initially computed. The synthetic matrix is shown below the diagonal in Table 1. Given the symmetry of the data and the model, it is necessary to carry out computations for estimating only the path coefficients in the first and third equations.

Estimates for the first equation are obtained in the same way as described for Model I, and, indeed, the results differ from those in Model I only slightly. The third equation (for Y_2) is just identified, in virtue of the specification $r_{1v} = 0$. Thus, we can multiply this equation through by Y_1, X_a, X_c, X_f, and X_d in turn and simplify to obtain five equations in the five unknown path coefficients and known correlations. The calculation of residual paths for all equations proceeds along the lines already illustrated for Model I.

From the perspective of the hypothesis that orients this study, the most interesting estimates are those for the reciprocal paths for aspirations: $p_{13} = p_{31} = .25$ and $p_{24} = p_{42} = .24$. The paths from predetermined to endogenous variables seem reasonable, except perhaps for the negative though small value of $p_{2f}(= p_{4c})$, $-.03$. That family SES has an apparently larger influence on educational aspiration than does intelligence may be explained by two considerations: intelligence has a sizable influence on occupational aspiration, which, in this model, intervenes between the latter and educational aspiration; and the family SES scale is heavily weighted by parental educational attainment.

In short, the model gives satisfactory estimates on the whole, if we are prepared to accept the excessively rationalistic assumption that occupational decisions precede educational decisions (both being measured well before the actual decision point), and if we are prepared to overlook the substantial negative correlations between disturbances, $r_{uw} = -.25$ and $r_{vz} = -.39$. Dissatisfaction with our ability to rationalize the latter motivated one more alternative construction on these data.

A COMPOSITE MODEL

The approach taken in Model IV (Fig. 4) was suggested by a remark of Turner and Stevens, who called attention to the possibility of constructions including aspects of both factor analysis and causal modeling.[14] The analogy with factor analysis here consists in the postulation of an unobserved variable, called "ambition" for lack of a better term, that underlies both educational and occupational aspiration. There are perhaps two justifications for such a postulate. First, both aspiration variables are probably rather unreliable relative to the other variables in the model, and it might be advisable to follow a procedure that reduces the attenuation introduced by measurement error before proceeding to a causal interpretation. Second, on a purely introspective basis it seems likely that many boys do not make a neat conceptual separation of educational and occupational aspirations, nor do they make plans for schooling and job choices in any fixed order. The argument here is just the opposite of the one that would have to be used to justify the precedence of occupational aspiration with respect to educational aspiration in Model III. If the reader likes, he may identify "ambition" with general level of aspiration, as the latter was conceptualized in the classic literature on

[14] Malcolm E. Turner and Charles D. Stevens, "The Regression Analysis of Causal Paths," *Biometrics*, XV (June, 1959), 236–58.

the subject.[15] Our procedure would then suggest that level of aspiration can only be manifested in and recognized by orientations toward particular goals, such as educational attainment or occupational achievement. We are, in effect, using verbalized educational and occupational aspirations as *indicators* of the construct, level of aspiration or "ambition."

Although the approach taken to the construction of hypothetical "ambition" variables is suggested by factor analysis, the actual procedure is an ad hoc one that depends on various heuristic considerations rather than on one of the standard factor models. Lest this arouse undue anxiety, we observe that the classic factor models, in their time, were similarly motivated by heuristic concerns. That they have become frozen into a canonical procedure taken over by investigators of problems whose structures are quite different from those of the pioneers is perhaps a commentary on the relative potence of propensities to imitate and to innovate in research. Certainly, after Wright's illuminating contrast between the interpretive and the purely mathematical approaches to factor analysis,[16] we need no longer feel constrained to follow a single routine that someone has ventured to dub "objective" or "optimal."

We begin, as usual, by writing the entire set of equations which are graphically represented in the path diagram of Model IV:

$$
\begin{aligned}
Y_1 &= p_{1G}G + p_{1u}X_u,\\
Y_2 &= p_{2G}G + p_{2v}X_v,\\
Y_3 &= p_{3H}H + p_{3w}X_w,\\
Y_4 &= p_{4H}H + p_{4z}X_z, \qquad \text{(Model IV)}\\
G &= p_{Gb}X_b + p_{Ga}X_a + p_{Gc}X_c\\
&\quad + p_{Gf}X_f + p_{GH}H + p_{Gs}X_s,\\
H &= p_{He}X_e + p_{Hd}X_d + p_{Hf}X_f\\
&\quad + p_{Hc}X_c + p_{HG}G + p_{Ht}X_t.
\end{aligned}
$$

The last two equations contain the substance of the model. G and H are the simultaneous endogenous variables, $X_a, \ldots, X_f$ are the predetermined variables, and X_s and X_t are the disturbances, which need not be uncorrelated with each other although both are uncorrelated with all predetermined variables. Each relation is overidentified,

[15] K. Lewin, Tamara Dembo, L. Festinger, and Pauline S. Sears, "Level of Aspiration," in J. McV. Hunt (ed.), *Personality and the Behavior Disorders* (New York: Ronald Press, 1944), I, 333–78.

[16] Sewall Wright, "The Interpretation of Multivariate Systems," in O. Kempthorne *et al.* (eds.), *Statistics and Mathematics in Biology* (Ames: Iowa State College Press, 1954), chap. ii, especially pp. 27–32.

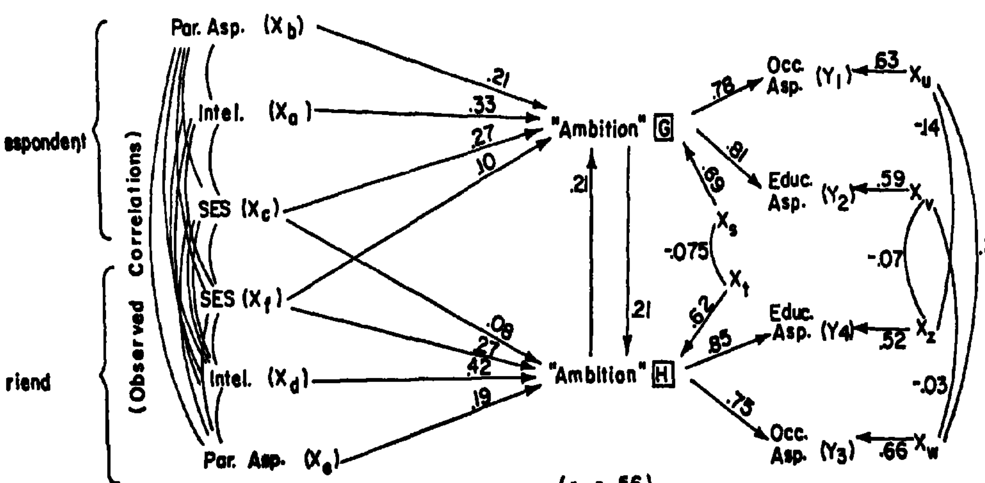


FIG. 4.—Model IV

since each includes only five explanatory variables while there are six predetermined variables in the model. We shall again employ the version of two-stage least squares illustrated for Model II when we come to the estimation of the path coefficients in these two equations.

Before proceeding to this task, we must first construct the two hypothetical endogenous variables by deriving their correlations with the predetermined variables, making use of the intercorrelations of the aspiration scores. Hence, attention will be focused first on the right-hand portion of the diagram.

The factor model employed here says that the correlation between a boy's educational and occupational aspirations is completely accounted for by his "ambition." The correlation between the aspirations of friends is substantially accounted for by the correlation of their respective values on "ambition," but the model allows for some between-friend correlation of specific elements in the two aspiration scores. Hence, the specifications $r_{uv} = r_{wz} = 0$; but r_{uz}, r_{vw}, r_{vz}, and r_{uw} are not necessarily zero. There are six known correlations among the aspiration variables, $Y_1, \ldots, Y_4$, and we have four equations of complete determination, stipulating that each aspiration variable is completely determined by the appropriate "ambition" variable and the residual. These ten conditions, however, do not suffice to estimate the paths in the first four equations of Model IV, inasmuch as we must estimate the four paths from "ambition" to aspiration, the four residual paths, four intercorrelations of residuals, and the unknown correlation between the two "ambition" variables, to wit, r_{GH}, for a total of thirteen unknowns. This portion of the model, by itself, is underidentified. Underidentification is, of course, the typical situation in factor analysis, which the analyst gets around by imposing various mathematical constraints on the solution. Here we invoke, instead, the "external" information provided by the predetermined variables as a constraint on the solution.

We begin by noting that the six correlations among the aspiration variables can be written as follows (obtained by "multiplying through" each of the first four equations in the model by the other aspiration variables):

$$r_{12} = p_{1G}p_{2G} = r_{1G}r_{2G},$$
$$r_{34} = p_{3H}p_{4H} = r_{3H}r_{4H},$$
$$r_{13} = p_{1G}p_{3H}r_{GH} + p_{1u}p_{3w}r_{uw},$$
$$r_{24} = p_{2G}p_{4H}r_{GH} + p_{2v}p_{4z}r_{vz},$$
$$r_{14} = p_{1G}p_{4H}r_{GH} + p_{1u}p_{4z}r_{uz},$$
$$r_{23} = p_{2G}p_{3H}r_{GH} + p_{2v}p_{3w}r_{vw}.$$

Bringing the predetermined variables into the picture, we note that $r_{1a} = p_{1G}r_{aG}$ and $r_{2a} = p_{2G}r_{aG}$. From these it would follow that $p_{1G}/p_{2G} = r_{1a}/r_{2a}$; but we can equally well compute $p_{1G}/p_{2G} = r_{1b}/r_{2b}$, and so on. As a compromise among the six possible estimates of this ratio, we take

$$p_{1G}/p_{2G} = \sum_i r_{1i} \Big/ \sum_i r_{2i},$$

where $i = a, \ldots, f$. Given this ratio p_{1G}/p_{2G} and the previously noted $r_{12} = p_{1G}p_{2G}$, we can solve at once for p_{1G} and p_{2G}. A similar procedure yields p_{3H} and p_{4H}.

Now, if we disregard correlations among the residual factors of the Y's, we may compute the implied correlations,

$$r'_{13} = p_{1G}p_{3H}r_{GH},$$
$$r'_{24} = p_{2G}p_{4H}r_{GH},$$
$$r'_{14} = p_{1G}p_{4H}r_{GH},$$
$$r'_{23} = p_{2G}p_{3H}r_{GH}.$$

Let $\delta_1 = r_{13} - r'_{13}$, $\delta_2 = r_{24} - r'_{24}$, $\delta_3 = r_{14} - r'_{14}$, $\delta_4 = r_{23} - r'_{23}$, and $S = \Sigma\delta^2$. We wish to select r_{GH} so that S is at its minimum. Finding dS/dr_{GH}, setting it equal to zero, and rearranging, we obtain

$$r_{GH} = \frac{r_{13}p_{1G}p_{3H} + r_{24}p_{2G}p_{4H} + r_{14}p_{1G}p_{4H} + r_{23}p_{2G}p_{3H}}{(p_{1G}p_{3H})^2 + (p_{2G}p_{4H})^2 + (p_{1G}p_{4H})^2 + (p_{2G}p_{3H})^2}.$$

The path coefficients in this expression have already been estimated and the correlations are known; hence r_{GH} is easily calculated as .56, which is the hypothetical correlation between "ambition" of respondent and "ambition" of friend. It is a materially higher value than any of the observed correlations between aspiration scores. This is the most important result of the work done to this point. We complete this phase of the calculations by computing residual paths for the aspiration variables, using formulas like $p_{1u}^2 = 1 - p_{1G}^2$, which are obvious from the model equations. Finally, we may return to the identities stated for the intercorrelations among the Y's with enough information in hand to solve the last four equations for r_{uw}, r_{vz}, r_{uz}, and r_{vw}, respectively.

One more step is necessary before we can begin the estimation of the last two model equations. As already noted, $r_{1a} = p_{1G}r_{aG}$ and $r_{2a} = p_{2G}r_{aG}$. Since r_{1a} and r_{2a} are known and we now have estimates of p_{1G} and p_{2G}, we obtain two solutions for r_{aG}. These are not the same, so we strike a simple average of the two and follow an analogous procedure to secure correlations of G and H with all the predetermined variables.

By this sequence of estimates and approximations, we have arrived at a complete correlation matrix for variables G, H, and $X_a, \ldots, X_f$. Coefficients in the last two equations of Model IV are now estimated by the two-stage least-squares procedure already illustrated for Model II. As noted in that illustration, when we come to calculate the paths involving the disturbances p_{Gs} and p_{Ht} and the correlation between the disturbances, r_{st}, we must base our calculations on the correlations implied by the model rather than on the correlations with which we began to estimate the coefficients of the model. To obtain the implied correlations involves more calculations. We have, for example,

$$r'_{Gb} = p_{Gb} + p_{Ga}r_{ab} + p_{Gc}r_{bc} + p_{Gf}r_{bf} + p_{GH}r'_{Hb}$$

and

$$r'_{Hb} = p_{He}r_{be} + p_{Hd}r_{bd} + p_{Hf}r_{bf} + p_{Hc}r_{bc} + p_{HG}r'_{Gb}\,.$$

These are of the form $r'_{Gb} = k_1 + p_{GH}r'_{Hb}$ and $r'_{Hb} = k_2 + p_{HG}r'_{Gb}$. Hence we may solve for the two implied correlations, obtaining

$$r'_{Gb} = \frac{k_1 + p_{GH}k_2}{1 - p_{GH}p_{HG}}$$

and

$$r'_{Hb} = \frac{k_2 + p_{HG}k_1}{1 - p_{GH}p_{HG}}.$$

Since two of the predetermined variables appear in both model equations (while four appear in only one equation each), four of the implied correlations (r'_{Gc}, r'_{Gf}, r'_{Hc}, and r'_{Hf}) are constrained to equal the corresponding initial values. For the remainder, the discrepancies are mostly minor. The tabulation below shows the implied correlations (with their deviations from initial correlations in parentheses):

$r'_{Gb} = .3096\ (.0016)$, $r'_{He} = .3059\ (.0104)$,
$r'_{Ga} = .5145(-.0011)$, $r'_{Hd} = .6333(-.0045)$,
$r'_{Gd} = .3498(-.0210)$, $r'_{Ha} = .3380(-.0052)$,
$r'_{Ge} = .1418\ (.0492)$, $r'_{Hb} = .1296\ (.0079)$.

The modest values of these discrepancies suggest that the model adequately takes account of the configuration of the data. Moreover, the reasonableness of the estimated paths and the small size of the correlation between disturbances ($r_{st} = -.075$) reinforce our satisfaction with the results. We may, finally, consider what correlations among all the observed variables are implied by the model as a whole. Formulas like $r'_{1a} = p_{1G}r'_{Ga}$ are used to compute correlations between aspiration variables and predetermined variables; formulas for the implied intercorrelations of the aspiration variables, neglecting correlations of their residuals, have already been given. A presentation of all implied correlations and their deviations from observed correlations is made in Table 2. These deviations are

analogous to the "residual correlations" obtained after extracting the "meaningful" factors in a factor analysis. They may be attributed to sampling error, if this seems reasonable. If not, they afford material for an investigator who wishes to essay a more convincing interpretation than that afforded by Model IV.

CONCERNING THE REDUCED FORM

In all the discussion thus far we have considered only what the econometricians call the "structural form" of the models. For some purposes, it is instructive also to pay attention to the "reduced form." Returning to the last two equations of Model IV, which constitute the substance of that model, let us substitute the expression for H given in the last equation into the next-to-last equation. We obtain a result that can be put into the form

$$G = a_a X_a + a_b X_b + a_c X_c + a_d X_d + a_e X_e + a_f X_f + a_s X_s + a_t X_t .$$

Similarly, substituting the fifth equation into the sixth, we obtain

$$H = \beta_a X_a + \beta_b X_b + \beta_c X_c + \beta_d X_d + \beta_e X_e + \beta_f X_f + \beta_s X_s + \beta_t X_t .$$

Let $\gamma = 1/(1 - p_{GH} p_{HG})$. Then the constants in the two foregoing reduced-form equations are defined as follows:

$$a_a = p_{Ga}\gamma , \quad a_b = p_{Gb}\gamma ,$$
$$a_c = (p_{Gc} + p_{GH} p_{Hc})\gamma , \quad a_d = p_{GH} p_{Hd}\gamma ,$$
$$a_e = p_{GH} p_{He}\gamma , \quad a_f = (p_{Gf} + p_{GH} p_{Hf})\gamma ,$$
$$a_s = p_{Gs}\gamma , \quad a_t = p_{GH} p_{Ht}\gamma ,$$

$$\beta_a = p_{HG} p_{Ga}\gamma , \quad \beta_b = p_{HG} p_{Gb}\gamma ,$$
$$\beta_c = (p_{Hc} + p_{HG} p_{Gc})\gamma , \quad \beta_d = p_{Hd}\gamma ,$$
$$\beta_e = p_{He}\gamma , \quad \beta_f = (p_{Hf} + p_{HG} p_{Gf})\gamma ,$$
$$\beta_s = p_{HG} p_{Gs}\gamma , \quad \beta_t = p_{Ht}\gamma .$$

Each reduced-form equation represents one endogenous variable as a linear combination of all the predetermined variables and the disturbances. The coefficients in this linear combination are not, however, linear combinations of the coefficients of the structural form.

Since X_s and X_t are specified to be un-

TABLE 2

CORRELATIONS IMPLIED BY MODEL IV BETWEEN ASPIRATION VARIABLES AND OTHER VARIABLES IN THE MODEL

VARIABLE	VARIABLE (SEE STUB)			
	Y_1	Y_2	Y_4	Y_3
Respondent:				
Parental aspiration (X_b)	.2401(.0264)	.2495(−.0247)	.1107(−.0017)	.0972(.0133)
Intelligence (X_a)	.3989(−.0116)	.4146(.0103)	.2887(−.0016)	.2535(−.0063)
Family SES (X_c)	.3567(.0327)	.3707(−.0340)	.3114(.0060)	.2734(−.0052)
Friend:				
Family SES (X_f)	.2623(−.0307)	.2726(.0319)	.4106(.0001)	.3605(−.0002)
Intelligence (X_d)	.2712(−.0283)	.2819(−.0044)	.5409(.0218)	.4749(−.0258)
Parental aspiration (X_e)	.1100(.0340)	.1143(.0441)	.2613(−.0171)	.2294(.0306)
Respondent:				
Occupational aspiration (Y_1)	....	.6247*	.3738(.0469)	.3283(−.0933)
Educational aspiration (Y_2)	....	....	.3885(.0216)	.3412(.0137)
Friend:				
Educational aspiration (Y_4)	....	....	....	.6404*
Occupational aspiration (Y_3)	....	....	....	....

NOTE.—Deviations from observed correlations are in parentheses.

* Model permits no deviation of implied from observed correlation. (Deviations from the other observed intercorrelations among the Y's occur if residual intercorrelations are neglected.)

correlated with the predetermined variables, the coefficients $a_a, \ldots, a_f$ can be estimated by the ordinary regression of G on $X_a, \ldots, X_f$ and the β's from the regression of H on the same six variables. (These regressions have already been computed for the first stage of the two-stage estimation procedure.)

It may be observed that $a_d/\beta_d = p_{GH}$; but $a_e/\beta_e = p_{GH}$, also. If we use our first-stage regression estimates of the a's and β's, the former implies $p_{GH} = .2891$ while the second yields $-.0838$. Similarly, estimating p_{HG} from β_a/a_a and β_b/a_b yields two inconsistent answers, .2338 and .1722, respectively. These inconsistencies present the problem of overidentification in an especially striking fashion. Had the equations in Model IV both been just identified, such inconsistencies would not have arisen. Indeed, the method of "indirect least squares," alluded to earlier as a technique for estimating coefficients in a just-identified system, consists precisely in estimating the reduced-form coefficients first and then deriving therefrom the estimates of coefficients in the structural equations of the model.

Although the overidentification means that the reduced-form coefficients do not yield unique estimates of the structural coefficients, we may still be interested in the reduced form of the model. To begin with, having obtained the structural coefficients by the two-stage procedure, we may compute the implied values of the reduced-form coefficients from them. Table 3 shows the reduced-form coefficients of Model IV, both as estimated from the first-stage regressions and as computed from the structural coefficients. For the most part, the discrepancies appear small, although it is these very discrepancies that preclude the indirect least-squares approach.

TABLE 3

REDUCED-FORM COEFFICIENTS FOR MODEL IV

Independent Variable	Symbol	Dependent Variable: Set (A) *G*	Set (A) *H*	Set (B) *G*	Set (B) *H*
Respondent:					
Intelligence	X_a	.3378	.0790	.3418	.0725
Parental aspiration	X_b	.2199	.0379	.2169	.0460
Family SES	X_c	.3056	.1426	.2919	.1396
Best Friend:					
Intelligence	X_d	.1291	.4464	.0913	.4399
Parental aspiration	X_e	−.0159	.1900	.0418	.2016
Family SES	X_f	.1499	.3034	.1585	.2942

NOTE.—Set (A), as estimated in first-stage regression; set (B), as computed from structural coefficients estimated in two-stage regression.

Apart from computation and estimation, the reduced-form coefficients, as defined in terms of the structural coefficients, have some conceptual or interpretive significance, for their definitions indicate something of the "mechanisms" through which the predetermined variables influence the endogenous variables. Consider

$$a_a = \frac{p_{Ga}}{1 - p_{GH}p_{HG}} = \frac{.3267}{.9560} = .34 .$$

The form of this expression indicates that X_a influences G directly, via p_{Ga}, and that this influence is slightly amplified, to the

extent of $1/(.956) = 1.046$, by the reciprocal action of respondent's and friend's "ambition." A more complex mechanism is suggested by

$$a_c = \frac{p_{Gc} + p_{GH}p_{Hc}}{1 - p_{GH}p_{HG}}$$
$$= \frac{.2749 + (.2074)(.0785)}{.956} = .29 .$$

In this case, there is not only the direct effect, p_{Gc}, but also a compound or indirect effect of X_c on G, via H, as represented by $p_{GH}p_{Hc} = .0163$, while the sum of the two is amplified by the reciprocation. The third type of mechanism is exemplified by

$$a_d = \frac{p_{GH}p_{Hd}}{1 - p_{GH}p_{HG}}$$
$$= \frac{(.2074)(.4205)}{.956} = .09 .$$

Here the influence of X_d on G is entirely indirect, via H, with the same factor of amplification as in the previous examples. Indeed, it is the assumption that some of the predetermined variables influence the endogenous variables only indirectly that permits the model to be identified in the first place. (Econometricians somewhat confusingly call both just-identified and over-identified models or equations "identified.")

The formulas for the reduced-form coefficients, of course, merely reflect the assumptions built into the model. But if the assumptions are accepted, it is of interest to include in the interpretation some evaluation of the relative importance of the various mechanisms that the model implicitly postulates.

EVALUATION AND DISCUSSION

Closing remarks are confined to comments on the rationale and results of Model IV. Some deficiencies of the earlier models have already been mentioned, and others that merit emphasis are shared by them with Model IV.

To recapitulate, the study was concerned with the hypothesis that adolescent boys influence each other in forming their occupational and educational aspirations. The earlier analysis of Haller and Butterworth had demonstrated that some correlation between the aspirations of boys in a peer group remained even if the groups were confined to those homogeneous on background factors presumed to give rise to aspirations. The present analysis accepts a different task: not that of hypothesis testing, but that of *estimation* in the context of an *explicit causal interpretation* of the influences on aspiration. The estimates are meaningful only to the extent that the initial study design is adequate to the purposes of identifying determinants of aspiration and of ascertaining the patterns of homophily operative in a relevant population.

The first question, then, is posed by a limitation on the design noted by the authors of the original study: with these data we cannot rule out the possibility that friendships are formed partially on the basis of common interests in educational and occupational goals. If this be the case, then all the estimates attempted here are beside the point, because we have treated aspirations as outcomes of the background characteristics of the respondent and his friend (treated as "predetermined variables") and of their respective influences on each other. As was also noted in the earlier paper, a longitudinal design would be required to eliminate the possibility of assortment on the basis of aspirations (although it is not entirely clear how the requisite causal inferences would be made, even if the design were longitudinal). The results here are, therefore, in the same provisional status as those of the predecessor study. If assortment on the basis of aspirations proves to be important, our estimates of the mutual influence of friends on each other's aspirations are not merely wrong; they become irrelevant.

Supposing, however, that friendship assortment occurs primarily on the basis of social and personal characteristics other

than aspirations (though possibly, as the models suggest, on the basis of factors affecting aspirations); then we must reckon with the further question of whether the effects of such characteristics are adequately accounted for. Percentages of "explained" variance in the jointly dependent variables of simultaneous models are not readily computed as they are for ordinary multiple regressions. From the size of the path coefficients for the disturbances, however, we might be prone to assume that some relevant background characteristics are omitted. Still, it is not obvious what they might be, since most studies of status achievement and aspirations have focused on variables much like those used here. Strictly speaking, the disturbance in a model represents all variables that operate "accidentally" or randomly with respect to the influence of predetermined variables. Retrospective introspection certainly suggests that many accidental experiences, not necessarily shared with one's best friend, may have an impact on the formation of aspirations. Again, the results must be left in provisional form: if further investigation discloses major background factors including high or low aspirations, and if there is significant homophily with respect to these factors, incorporation of such factors into constructions like Model IV may well result in drastic reduction of the paths representing reciprocal influence of respondent's and friend's aspirations.

Looking more specifically at the results with Model IV, we may note that some of the asymmetry between respondent and friend that seemed implausible in Models I and II has disappeared in the more elaborate model. Except that friend's "ambition" seems to be more heavily influenced by his intelligence than is the case for respondent's "ambition," the results for the two boys are much alike. It is only a coincidence that $p_{GH} = p_{HG}$ when the results are rounded to two decimal places. But either of these coefficients at a value of roughly .2 seems like a reasonable estimate of the influence of a friend's aspirations upon one's own. This estimate does not differ greatly from those for Models I and III, but is on the conservative side by comparison with them. Recalling that the correlation between H and G, the two constructed "ambition" variables, came out as .56, the .21 value for p_{GH} and p_{HG} suggests that a significant part of the explanation for resemblance between aspirations of peers is due to mutual influence, but a goodly part of it is also due to the way in which peers come to associate (assortatively with respect to background characteristics) in the first place—bearing in mind the reservation already stated concerning the cogency of this interpretation.

The result that p_{GH} is very nearly the same as p_{HG} may be regarded as somewhat anomalous. In Model III the two reciprocal paths were forced to be equal, but in Models I and II, where this was not the case, the influence of friend on ego appeared to be somewhat stronger than that of ego on friend. This is perhaps what we should expect, given that the friendship pairs analyzed here are not defined by mutual choices but by the unilateral choice of the respondent. We know (or can presume) that friend is a significant other for ego, but we cannot be sure that the converse is true. Clearly, the whole matter of the extent to which an individual's dispositions are influenced by significant others should be further explored in research designed to include measures of degree of significance of those others, estimated independently of the dependent variable under study.

Of the discrepancies between observed correlations and those implied by the model, only the one of $-.09$ for r_{13}, between the two friends' occupational aspirations, seems interesting (this discrepancy appears in a different guise as $r_{uw} = .22$). The model may seem to fail to represent quite adequately some specific aspect of similarity of friends' occupational choices. It need not be argued, however, that the model underestimates mutual influence of friends' occu-

pational aspirations. If friends encounter the same role models, apart from their families, this could induce some similarity in their cognitive and affective orientations to the world of work.

A final reservation will be stated, although others may well occur to the reader. The parental aspiration variable is based on the respondents' reports of their parents' attitudes. Hence this variable may well be contaminated to some degree by the dependent variables which it supposedly helps to explain. Fortunately, the study design precludes a similar contamination of the data on friend's aspirations.

The reader may well be appalled at all the apparatus brought into play in an attempt to demonstrate the reasonableness of what he already knew—even though he has been privileged to witness only a small part of the trial and error going into the construction of Model IV. The rejoinder to such a possible criticism would surely be that if a hypothesis is worth considering at all, it should be worthwhile to do some hard work to estimate its significance. On the purely conceptual level, it may suffice to recognize peer-group influence on aspiration as an actual process and to reason from that in a qualitative way to some of its consequences. Ultimately, however, we shall want to know, of the factors and processes that operate in the real world, which ones do how much of the work. Constructions like those exhibited in this paper not only offer one approach to the rendering of relevant estimates, but also present interpretations in such a form that their weaknesses—and those of the theories giving rise to them—are fairly evident. If the results of more of our research could be cast into this form, we would begin to understand better how much we do and do not know.

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Professionalization and Bureaucratization in Large Professional Organizations[1]

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ABSTRACT

The conflict and interdependence of the processes of professionalization and bureaucratization are examined in large public accounting firms. The emerging pattern of professional bureaucracy reveals the complementarity of previously offered models and depicts a sequential development. Comparative analysis with professional organizations from other occupations indicates that professionalization is positively related to centralization and size of administrative component. The rate of development of the professional bureaucratic process can be affected by changes in organization size and complexity and in capacity to innovate or create new knowledge.

In recent years, several sociologists have emphasized the professionalization of modern industrial society.[2] Professionalization is seen as the newest major process that is an "effect" of technological change on the occupational structure of the community, the other processes being industrialization, urbanization, and bureaucratization.[3] As the number and types of professionals in the work organization have increased, their conflict with the bureaucratic process has generally deepened.[4] However, it has been shown that a reconciliation can be achieved between the two,[5] that, in fact, the professionalization and bureaucratization proc-

[1] This study was supported by National Science Foundation grant GS-804 and by a New York University fellowship. I wish to thank Harry Gracey, Marvin Koenigsberg, and Erwin O. Smigel for their comments and advice on earlier drafts of this paper.

[2] Nelson N. Foote, "The Professionalization of Labor in Detroit," *American Journal of Sociology*, LVIII (January, 1953), 371–80; Howard M. Vollmer and Donald L. Mills, "Nuclear Technology and the Professionalization of Labor," *American Journal of Sociology*, LXVII (May, 1962), 690–96; Harold L. Wilensky, "The Professionalization of Everyone?" *American Journal of Sociology*, LXX (September, 1964), 137–58; William A. Faunce and Donald A. Clelland, "Professionalization and Stratification Patterns in an Industrial Community," *American Journal of Sociology*, LXXII (January, 1967), 341–50.

[3] Faunce and Clelland, *op. cit.* The authors acknowledge the classification of the latter three processes by Maurice Stein, *The Eclipse of Community* (Princeton, N.J.: Princeton University Press, 1960).

[4] Among the earlier examinations of this development are: Logan Wilson, *The Academic Man* (New York: Oxford University Press, 1942); Peter M. Blau, *The Dynamics of Bureaucracy* (Chicago: University of Chicago Press, 1955); Roy G. Francis and Robert C. Stone, *Service and Procedure in Bureaucracy* (Minneapolis: University of Minnesota Press, 1956); Harold L. Wilensky, *Intellectuals in Labor Unions: Organizational Pressures on Professional Roles* (New York: Free Press, 1956); Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles—Parts I and II," *Administrative Science Quarterly*, II (1957, 1958), 281–306, 444–80.

[5] For example, Mary E. W. Goss, "Influence and Authority among Physicians in an Outpatient Clinic," *American Sociological Review*, XXVI (February, 1961), 39–50; Ronald G. Corwin, "The Professional Employee: A Study of Conflict in Nursing Roles," *American Journal of Sociology*, LXVI (May, 1961), 604–15; Howard M. Vollmer, "Entrepreneurship and Professional Productivity among Research Scientists," in Howard M. Vollmer and Donald L. Mills (eds.), *Professionalization* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966), pp. 276–82; Richard H. Hall, "Some Organizational Considerations in the Professional-Organizational Relationship," *Administrative Science Quarterly*, XII (December, 1967), 461–78.

esses are of necessity interdependent in bureaucratic organizations.[6]

For professional organizations[7] a similar interdependence has been suggested by Litwak, which he calls *professional bureaucracy*, a third model of organization which is, in effect, a synthesis of professional and bureaucratic models.[8] Upon examining large law firms, Smigel uncovered a pattern of bureaucracy which also is given the name of "professional bureaucracy."[9]

In an analysis of the largest professional organizations extant, the "Big Eight" public accounting firms, I inquired into the relationship among the aforementioned processes. A form of professional bureaucracy was found which combined elements of the perspectives of both Litwak and Smigel, thereby indicating their complementarity, and which disclosed changes in the relationship as measured by organizational variables of size, centralization, and size of administrative component.

BACKGROUND AND PROCEDURE

The Big Eight are international public accounting firms[10] founded around the turn of the century as small, local partnerships to manage the bookkeeping and accounting of corporations. With the growth of these clients over the succeeding decades and with the federal requirements of taxation and annual audit set up in the 1930's, the firms expanded their auditing services. Each firm presently has an average of eighty offices located in forty foreign countries and fifty offices located in the major cities of the United States. Offices of the largest American cities contain upward of fifty, and in a few instances more than 1,000, public accountants. The average size of a firm, including all offices, is 5,500.

The focus for this study is the largest offices of the Big Eight, located in New York City.[11] Each office, averaging 1,500 personnel, contains three separate hierarchies: one of public accountants, one of non-accounting "management services" experts, and a non-professional supporting

[6] William Kornhauser, *Scientists in Industry: Conflict and Accommodation* (Berkeley: University of California Press, 1962), p. 197; Blau, *The Dynamics of Bureaucracy* (rev. ed.; Chicago: University of Chicago Press, 1963), p. 9, describes an interdependence maintained in an environment of continual change.

[7] A professional organization is here defined as an organization in which: (1) members of one or more professional groups define and achieve the primary organizational goals (as compared with a professional association—a group organized to initiate and promote general professional objectives of the entire profession or segments thereof); (2) the majority of the people in the organization are professionals; (3) the administrative hierarchy of authority lies within the firm, whereas authority in professional matters is placed in the hands of the professional associations; (4) the profession promotes norms of personal autonomy and altruistic action in all matters relating to use of the body of knowledge.

[8] Eugene Litwak, "Models of Bureaucracy Which Permit Conflict," *American Journal of Sociology*, LVVII (September, 1961), 182. The author does not use the term "professional organization," but the examples he gives—a large hospital, a graduate school, a research organization—fit the definition (see n. 7, above). The third model is a "co-ordination" of the unlike efficiencies of the first two: the Weberian (bureaucratic) model of recurrent events and traditional knowledge and the human relations (professional) model of uncertainty situations—ever developing, non-recurring events involving new knowledge.

[9] Erwin O. Smigel, *The Wall Street Lawyer: Professional Organization Man?* (New York: Free Press, 1964), pp. 275–86. Utilizing Gouldner's rules criterion for distinguishing three patterns of bureaucracy (Alvin W. Gouldner, *Patterns of Industrial Bureaucracy* [New York: Free Press, 1954], pp. 216–17), Smigel depicts a fourth pattern, professional bureaucracy, which is composed of a system of rules external to the organization, is devised by professional associations and the government, and which conditions the behavior of the lawyer through a long socialization process.

[10] The eight firms, alphabetically, are: Arthur Andersen & Co.; Ernst & Ernst; Haskins & Sells; Lybrand, Ross Bros. & Montgomery; Peat, Marwick, Mitchell & Co.; Price Waterhouse & Co.; Touche, Ross, Bailey & Smart; Arthur Young & Co.

[11] The national and international executive quarters for these firms are, with two exceptions, located in New York City. This policy-making group functions independently of the operating or "line" offices in New York City, but is usually housed in the same building.

staff. The largest group, the public accountants, number about 1,000, of whom approximately half are in the beginning position of "junior." Another 300 are seniors, 100 are managers, and the 100 top positions are filled by partners. The average size of the management services hierarchy is 200. Slightly more than half of these are "associates," another quarter are at the next higher level of supervisor, and the remaining 15 per cent are at the highest level of "principal." The clerical staff of 300 constitutes the third major group of the firm, with a system of ordered positions similar to the clerical arrangement found in large bureaucratic departments.

Each firm is also stratified according to four major areas of work. The largest area, auditing, is the primary function of the firm —examining the financial structure and processes of the client. The tax area includes tasks ranging from preparation of corporate and individual tax returns to tax advice on mergers, reorganizations, liquidations, estate planning, and special surveys. The accounting personnel perform the duties of these two areas.[12] In the third area, management services, are located most of the non-accounting specialists (engineers, mathematicians, social scientists), who inform and advise clients on data processing, operations research, general management, personnel, organizational structure, marketing, and other economic considerations. The fourth area, firm administration, is shared by partners and some managers. It consists mostly of recruitment of personnel and their assignment and of periodic meetings on serious client matters.

The work of these firms is extremely important to the financial and investment communities. Collectively, these eight firms audit nearly half the total corporate wealth of the United States. They audit 94 per cent of the 500 largest industrial corporations and the same percentage of the fifty largest merchandising firms, the fifty largest transportation companies, and the fifty largest utility companies.[13] The smaller organizations are not excluded; nor are those which are not required by law to be audited.[14] The average number of clients for a Big Eight firm is 10,000.

Initially, a pilot study was carried out at each firm. On the basis of these broad reviews, a self-administered questionnaire was developed, pretested, and mailed to a systematic random sample of the New York City offices of three Big Eight firms. The sample was stratified first on the basis of the four major work areas and then within each area by position in firm. A highly satisfactory return was received on the first mailing for a total number of 111.[15] From purposive samples constructed based on position in firm, fifty-one standardized interviews were conducted at six of the eight firms.[16] At one of these six firms, observa-

[12] Within each of these work areas there is further specialization by the CPA. Although a partner has a specialty within an area, e.g., corporate reorganizations or tax planning, he assumes responsibility for the entire audit of each of his approximately thirty-five clients. As a result, specialization is limited to that amount of time remaining after his examination and analysis of all financial and related aspects of the client organization.

[13] This amounts to $300 billion + in total sales and net revenues and $400 billion + in total assets, based on the lists in *The Fortune Directory* (1965) (as compared with mention of auditors in the 1965 series of Moody's industrial, public utility, and transportation manuals and *Poor's Register of Corporations, Directors and Executives* [1965]), and personal telephone calls. These percentages are relatively stable over the span of a few years, because clients rarely change their auditors, and rarely do they fall from their top listing during that time.

[14] One Big Eight firm alone audits more than 1,000 banks, 700 savings and loan associations, 700 insurance companies, and 1,200 non-profit institutions, including universities, hospitals, and local, state, and federal governmental bodies; from T. A. Wise, "The Very Private World of Peat, Marwick, Mitchell," *Fortune*, LXXIV (July 1, 1966), 91.

[15] The total return was 92, 85, and 66 per cent for the three firms. Homogeneity and, in some cases, size of the thirteen subsamples limited the disproportionateness of the stratification.

[16] For a total sample number of 162 for the eight firms. Thirty-nine interviews contained a majority of the items appearing on the questionnaire. The remaining twelve focused on areas of

tion was conducted of the entire work process at a client's offices and of the firm's recruitment procedures.

ORGANIZATIONAL SIZE, CENTRALIZATION, AND ADMINISTRATION

From the pilot studies it was obvious that the first task of the research was to detect how professional organizations of such large size manage to remain relatively free from increasing bureaucratization.[17] First, examination of apportionment of work time showed that total time of all professionals in firm administration averaged 12 per cent of total professional work time. This administrative work is spread among 40 per cent of the professional staff, and only 4 per cent spend the majority of their time in it. Clerical staff comprise 20 per cent of the firm. This produces a grand total of 32 per cent spent in non-professional work.

Second, the formal managerial decision-making structure of a Big Eight firm is highly centralized, with a senior partner as "president" of the firm and chairman of the executive or managing committee. The committee is composed of partner-directors for each of the firm's major areas of specialization, with lines of authority within each area and for each region of the country. Even though every partner is given personal responsibility over his audit for the client, the system of checks on his work, along with the formal structure, allows the firms to be classified as highly centralized. One executive partner succinctly stated: "The audit partner calls the signals on the job. He can consult on his problems with any specialists in the firm. One partner reviews all opinions [final report on the client], but this comes after the fact, after the opinion is issued. The partner is responsible to the partnership for his work, but he has complete autonomy within the firm."

How is it, then, that these firms spend so little of their professional man-hours in administration? First, administrative authority is located at the very top. General firm policy is decided by the executive committee. Other administrative tasks such as recruitment and assignment are shared by the partnership as equally as possible. In two of the firms, personnel department positions are filled by a system of rotating partners every few years. As already evidenced, nearly all partners and professional employees remain active in client work. Otherwise, they feel the stigma of being known as a "kept accountant." Second, the public accountant's work is conducted at the client's offices in small groups of three to ten persons, thereby spreading lower-echelon administration throughout the organization. Informality within the work groups is the norm. For each client, the partner selects a work team of managers, seniors, and juniors on the basis of their technical background and their ability to work smoothly with both client and partner. These employees move from one work team to another and from one partner to another several times a year. Centralization therefore is compatible with these professionalized firms.

The comparison of the relation of size to administrative component and to degree of centralization for these firms can be made to only one other study without becoming methodologically inconsistent.[18] This is the

special knowledge of those interviewees in positions of senior partner, management services director, and executive partner. Two firms granted only a single lengthy interview and no questionnaires. Enough material was gathered on size and work apportionment to include it in the analysis. In these two cases, the problems of professional conservatism and secrecy outweighed the interest to participate any further. These problems of gaining access I have examined in my Ph.D. thesis, "Bureaucracy and Change in Large Professional Organizations: A Functional Analysis of Large Public Accounting Firms" (unpublished, New York University, 1967), pp. 36–46.

[17] Bureaucracy is in this instance defined as the process of rationalized efficiency, according to Weber's characteristics; H. H. Gerth and C. Wright Mills (eds.), *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), pp. 196–98.

[18] William A. Rushing, "Organizational Size and Administration: The Problems of Causal Homogeneity and a Heterogeneous Category," *Pacific*

analysis of professional organizations of public personnel agencies.[19] The relationship, according to size, is similar in both studies. Using the measurement bases for the agencies,[20] the largest personnel agencies and the largest accounting firms display a high degree of professionalization, high centralization, and a small administrative component. And the changed relationship for medium-sized agencies (approximately 200 personnel) and medium-sized accounting firms[21] remains alike: a high degree of professionalization and low centralization, whatever the administrative ratio. All twelve accounting firms, the eight large and four medium-sized, follow the pattern of the majority of personnel agencies of like size. More important, the eight largest firms show a much higher measurement on these three variables than do the personnel agencies. Further investigation uncovered factors that indicate the determining variable to be professionalization.[22]

PROFESSIONALIZATION VS. BUREAUCRATIZATION: CONFLICT AND INTERDEPENDENCE

Blau feels that the unusual combination of centralization and professionalization is possible because there are "substitute methods of modern administration," such as detailed statistical records of performance, which check on the work of subordinates and obtain information on operations without frequent direct supervision.[23] In the highly professionalized accounting firms, besides the annual opinions and other performance reports, additional substitute methods of administration are the rules and procedures external to the organizations.

Sociological Review, IX (Fall, 1966), 100–108, emphasizes that the many studies dealing with the relationship between organizational size and relative size of administrative component have reached inconsistent results because total administrative component is a heterogeneous category with respect to types of occupations; therefore, it is not significant as a single total measure. Rather, classes of personnel must be related. In the case of the studies under consideration, only one class is dealt with—large-firm professionals, social agency and public accounting.

[19] Peter M. Blau, Wolf V. Heydebrand, and Robert E. Stauffer, "The Structure of Small Bureaucracies," *American Sociological Review*, XXXI (April, 1966), 179–91; see especially Table 2, p. 183.

[20] *Ibid.*, p. 183:

"The administrative apparatus ratio is *low* when the proportion of clerks among the total staff is less than 60 per cent; *high* when it is 60 per cent or more.

"The managerial hierarchy is *centralized* when the ratio of non-clerical personnel in managerial positions to non-clerical personnel in non-supervisory positions, excluding those listed as neither, is less than one to three; it is *dispersed* when the ratio is one to three or more.

"Professionalization is *low* when the proportion of the operating staff (excluding managers as well as clerks) who are required to have, at least, a college degree with a specified major, is less than 50 per cent; *high* when it is 50 per cent or more."

[21] For purposes of comparison by size, four medium-sized accounting firms (average size 200 personnel) were extensively interviewed. The hierarchy of job titles and occupational specialties is basically the same as that of the large firms, except that the Big Eight afford greater specialization in each work area.

[22] All of the Big Eight have an administrative ratio of less than 30 per cent, are centralized at a ratio of less than one to six if all partners are included as managerial personnel, and are 100 per cent professionalized (using Blau's bases for measurement—see n. 20, above). It is obvious that for highly professionalized organizations more sensitive measures must be developed. For the accounting firms, professionalization is given a wider definition, which includes the attributes of: a body of knowledge with a developed intellectual technique, supported by a formalized educational process with standardized testing and licensing, a code of ethics governing relations with colleagues, clients, and other external organizations, and a professional association to facilitate the maintenance and development of all of the former. Attempts to empirically measure these and other ideal type attributes are found in Richard M. Lynch, "Professional Standards for Management Consulting in the United States" (unpublished Ph.D. thesis, Graduate School of Business Administration, Harvard University, 1959), pp. 30–31, 138–39; Montagna, *op. cit.* (see n. 16, above), chap. iv.

[23] Blau, *The Dynamics of Bureaucracy* (rev. ed.), chap. iii; Blau, Heydebrand, and Stauffer, *op. cit.*, p. 185.

These are the responsibility of the professional association of CPA's, the American Institute of Certified Public Accountants. These external rules include an elaborate and much revised code of ethics, a newly codified volume of principles of accounting, and revised auditing standards and procedures. They are constructed and revised by Institute committees in which the views of the large firms are well represented. These rules serve as a foundation for the firms' more specific internal rules, a few of which are more stringent, others of which merely expand on the external rules. Nearly to a man, the total sample agreed that compared with internal rules, the external rules were the more important rules for their firms and for the profession as a whole. For the firms, the number of problems inherent in constructing and enforcing these professional standards are greatly reduced. And changes are made without an opposing vested interest—there are virtually no full-time administrators whose jobs depend on an elaborate bureaucratic system of one specified routine based on the affected rules.

In the case of these organizations, then, bureaucratization, in the pejorative sense of dysfunction, that is, routine, rigidity, overconformity, is limited by the external rules and by personal autonomy. However, because of the tremendous increase in the number, extent, and specificity of these rules, CPA's fear a severe limitation on the scope of their professional judgment. What was once unwritten rule or mystique is now rationalized; in the process of formalizing its rules, the profession transforms that knowledge from an intellectual to a mechanical technique. As one senior partner put it, "The client asks not what to do but how to do it, as the body of knowledge becomes detailed and easier to interpret." The power of the expert disappears as soon as the area of uncertainty (professional judgment) can be translated into rules and programs.[24] As one partner concluded, "We could audit IBM [the ninth largest corporation in America] in almost one day with very little risk because their internal systems of control are so tight." Partly because of rule making, partly because of computerization, the traditional annual audit, required by federal law, is becoming what one executive partner termed "the annual nuisance." The CPA finds he must submit to detailed professional requirements when constructing reports and opinions for his client. Yet, he should be free of such encumbrances to properly exercise his professional judgment.

The public accountant's response to this threat has been to expand into new areas of uncertainty, especially management services[25] and taxes. Presently, each of these areas accounts for 20 per cent of the professional man-hours in a Big Eight firm. Many CPA's contend that, like taxes, management services were always an integral part of the audit process, and indeed there is very little that cannot be classified within the broad definition of "internal control"

[24] Michel Crozier, *The Bureaucratic Phenomenon* (Chicago: University of Chicago Press, 1964), p. 299: "The elimination of the 'bureaucratic systems of organization' in the dysfunctional sense is the condition for the growth of 'bureaucratization' in the Weberian sense." Also, Michel Crozier, "Crise et renouveau dans l'administration française," *Sociologie du travail*, VIII (July-September, 1966), 327; Peter M. Blau and W. Richard Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962), pp. 240–42; Victor A. Thompson, "Bureaucracy and Innovation," *Administrative Science Quarterly*, X (June, 1965), 4; James D. Thompson, *Organizations in Action* (New York: McGraw-Hill Book Co., 1967). The writings of a leading public-accounting spokesman reflect this thinking: John L. Carey, *The CPA Plans for the Future* (New York: American Institute of Certified Public Accountants, 1965), pp. 191-92, 227. A theoretical perspective of uncertainty is given by Ralf Dahrendorf, *Essays in the Theory of Society* (Stanford, Calif.: Stanford University Press, 1968).

[25] More recently, there has been experimentation with the management attest, an audit conducted for stockholders and other interested third parties to determine management's compliance with certain of its own prearranged standards for information collection, decisioning, and control processes, but not of the results of these processes. Some in the profession are now suggesting that the CPA should begin to think about prearranging the standards for management.

(an audit term). Regardless, new developments in management services, such as computerization, offer CPA's the ability to integrate the planning, measuring, attesting, and communicating of the total information system of an economic organization.[26] Thirty-five per cent of the total sample voluntarily suggested this area (called "operations auditing") to be the only one where judgment and creativity are found to a significant degree. It is the person who is familiar with the computer process who *composes* an "automated audit."

The movement into management services provides the profession with new non-rationalized intellectual techniques. But at the same time, this knowledge is not integrated within the profession and has proved to be dysfunctional to the firms. If the management services work for a client is coordinated with the partner's audit, some administrative authority will tend to be legitimated in terms of incumbency of office. In this situation, obedience is stressed as an end in itself because the CPA as administrator is not able to judge the non-accountant expert on the basis of that expert's knowledge. Rules are initiated by one party (a CPA)—characteristic of "punishment centered" bureaucracy.[27] Added to this is the task of each firm having to organize independently and enforce a new code of ethics and procedures for this technical field until, in time, the professional organization takes over this function.

In sum, the move into areas of uncertainty provides an important basis for continued professionalization—an expanded body of knowledge which supports an intellectual technique and requisite judgment.[28] However, because of this, new bureaucratic problems rapidly emerge. The process can be examined beginning at any stage of its development. External rules form a *pattern* (to use Smigel's term) for professional bureaucracy. They inject uniformity into the social system. But the danger of uniformity and the normal rate of technological development spur the search for new areas of professional control. At any one time in the organizational process, there is a conflicting yet interdependent mixture of uniform and non-uniform events, of rationalization and uncertainty (to use Litwak's description).

CONCLUSIONS

On the basis of the analysis, three generalizations can be drawn concerning large professional organizations: (1) The more highly professionalized the organization, the more highly centralized it is and the smaller its administrative component. (2) As organizations become larger and more complex, technically, strong patterns of punishment-centered bureaucracy may form which are not found in smaller organizations.[29] (3) The collection, analysis, classification, standardization, and enforcement of external rules carried out by the professional association may involve the simultaneous occurrence of rationalization of one body of

[26] These functions of the economic organization are defined by leaders in the profession. For example, Herman W. Bevis, "The Accounting Function in Economic Progress," *Journal of Accountancy*, CVI (August, 1958), 27–34; John L. Carey (ed.), *The Accounting Profession: Where Is It Headed?* (New York: American Institute of Certified Public Accountants, 1962), p. 11.

[27] Alvin W. Gouldner, "Organizational Analysis," in Robert K. Merton, Leonard Broom, and Leonard S. Cottrell, Jr. (eds.), *Sociology Today: Problems and Prospects* (New York: Basic Books, 1959), p. 403. See also a summary discussion by Louis R. Pondy, "Organizational Conflict: Concepts and Models," *Administrative Science Quarterly*, XII (September, 1967), 314–17.

[28] This move takes place because of the rationalization of CPA knowledge in external rules. As Faunce and Clelland (*op. cit.*, p. 342) point out: "While industrialization, urbanization, bureaucratization, and professionalization may occur *simultaneously*, they do not ordinarily develop at the same rate. More typically, they form a *sequence* with a high level of development of one acting as a spur to development of the next. Increased professionalization is, *in part*, an outgrowth of the bureaucratic emphasis upon expertise and rationalism" (emphasis my own, P. D. M.).

[29] This pattern was not a significant one in the twenty largest law firms in the United States (Smigel, *op. cit.*, p. 279), which are, on the average, only one-tenth the size of the large public accounting firms.

knowledge and the development of another.

With regard to (3), both occurrences tend to create dysfunctions. Even though the process of professional bureaucracy allows for change, there is concern that the change may be too rapid or far reaching, as well as too precise or narrow.[30] Also, with rapid innovation, the firms must solve the immediate problems of administration until the slower-moving professional association develops profession-wide norms and laws. The rate of change must somehow be regulated, or the process will be disrupted.

Ultimately of more concern is the problem caused by increased size in (2). If these largest firms are the prototype for the future organization,[31] the serious problems of bureaucracy may not be so easily overcome in a future accelerated process of professional bureaucracy generated by more rapid social and technological change.[32]

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[30] Wilensky, "The Professionalization of Everyone?" (see n. 2, above), pp. 148–49, defines the problem as knowledge being too vague (e.g., social work) or too precise and that there may be an optimal base for professional practice.

[31] Warren G. Bennis, *Changing Organizations* (New York: McGraw-Hill Book Co., 1966), chap. I. Bennis forecasts "on thin empirical ice" that the work organizations of the future will contain adaptive, rapidly changing, temporary systems of diverse professionals operating in complex and creative environments and will displace bureaucracy as the primary form of organizational structure. Also, Harold L. Wilensky, *Organizational Intelligence: Knowledge and Policy in Government and Industry* (New York: Basic Books, 1967), pp. 46–47. Galbraith's "technostructure" agrees with this description; John Kenneth Galbraith, *The New Industrial State* (Boston: Houghton Mifflin Co., 1967), pp. 57–71, 168–75.

[32] See Litwak's explanation for his assumption that "non-uniform events will constitute a major factor in organizational analysis in the foreseeable future" (Litwak, *op. cit.*, p. 181).

Patterns of Management and Adaptations to Organizational Roles
A Study of Prison Inmates[1]

Thomas P. Wilson

ABSTRACT

A common finding in organizational research is that individuals supervised with "participative" management practices tend to have better relations with superiors and closer relations with peers than do individuals under "bureaucratic" supervision. As an explanation, it is proposed that the way subordinates are involved in decisions affecting them influences their attitudes toward themselves in relation to their organizational roles and that these attitudes, or "adaptations," in turn affect subordinates' behavior toward superiors and peers. These hypotheses are investigated in a study of prison inmates' adaptations and relations with staff and fellow inmates. As anticipated, adaptations are systematically related to exposure to bureaucratic or participative management, and, in addition, adaptations account for much of the association between management parctices and inmates' relations with others. In addition, viewing organizations in terms of structures of decision making suggests that the effects of management practices on subordinates' task performance should be influenced strongly by the technology of organizational activities.

Much of the research and speculation on behavior in organizations concludes that individuals who are treated with respect and given autonomy by their superiors have better relations with superiors and peers and also perform better in their assigned tasks than do individuals who are closely and arbitrarily supervised.[2] However, though it appears that supportive management styles do generate improved relations with superiors and close relations among peers, there is some evidence that such management is not consistently associated with high performance.[3] This suggests that the mechanisms by which management practices in organizations affect subordinates are not adequately under-

[1] I wish to thank Allen H. Barton, William J. Chambliss, Daniel N. Gordon, Sheldon L. Messinger, Derek L. Phillips, Elliot Studt, and Paul L. Wuebben for their comments on earlier drafts. The research reported here was conducted under the auspices of the Institute for the Study of Crime and Delinquency and the California Department of Corrections, but responsibility for the conclusions is mine. The work was supported by grants from the Ford Foundation small grants program and NIMH grants 5-R11-MH635 and 3-11MH635-025.

[2] For example, Elliott Jaques, *The Changing Culture of a Factory* (New York: Dryden Press, 1952); Ronald Lippitt, "An Experimental Study of the Effect of Democratic and Authoritarian Group Atmosphere," *University of Iowa Studies in Child Welfare*, XVI (1940), 43–195; Ralph White and Ronald Lippitt, "Leader Behavior and Member Reaction in Three 'Social Climates,'" in Dorwin Cartwright and Alvin Zander (eds.), *Group Dynamics* (Evanston, Ill.: Row, Peterson & Co., 1953), pp. 583–611; Rensis Likert, *New Patterns of Management* (New York: McGraw-Hill Book Co., 1961); Rensis Likert, *The Human Organization* (New York: McGraw-Hill Book Co., 1967); and Norman R. F. Maier and John J. Hayes, *Creative Management* (New York: John Wiley & Sons, 1962). Much of the work in this area is summarized in the books by Likert.

[3] See Peter M. Blau and Richard W. Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962), pp. 186–92; Theodore Caplow, *Principles of Organization* (New York: Harcourt, Brace & World, 1964), pp. 156–58; Robert Dubin, George C. Homans, Floyd C. Mann, and Delbert C. Miller, *Leadership and Productivity* (San Francisco: Chandler Publishing Co., 1965), pp. 1–50; Robert L. Hamblin, "Punitive and Non-Punitive Supervision," *Social Problems*, XI (Spring, 1964),

stood. The present paper attempts to add some clarification through an investigation of prison inmates' adaptations to "bureaucratic" and "participative" patterns of institutional management.

Previous studies of correctional and psychiatric institutions have shown that flexible and supportive management patterns are associated with comparatively good relations between inmates and staff and with close relations among inmates. Early institutional studies dealt mainly with custodial institutions, characterized by authoritarian and arbitrary supervisory practices, and generally inmates were found to be antagonistic to staff.[4] In these institutions, order and control appeared to depend on inmate power structures maintained by covert and often unwitting collusion between staff and an inmate elite, and the "inmate code" was seen as playing an important part in sustaining inmate solidarity in opposition to staff, mitigating the effects of distrust and hostility among inmates, and legitimating the privileged access of the elite to interaction with staff.[5] In contrast, recent research in correctional[6] and mental institutions[7] indicates that this kind of inmate organization is less prominent in treatment-oriented institutions. For example, Street, Vinter, and Perrow compared inmates in several juvenile correctional institutions and found a tendency for inmates in the treatment-oriented institutions

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345–59; and the suggestive findings and comments in Joan Woodward, *Industrial Organization* (London: Oxford University Press, 1965).

[4] The research on prisons is summarized in Richard A. Cloward, Donald R. Cressey, George H. Grosser, Richard McCleery, Lloyd E. Ohlin, Gresham M. Sykes, and Sheldon L. Messinger, *Theoretical Studies in Social Organization of the Prison* (New York: Social Science Research Council [Pamphlet 15], 1960). References to the original research reports are given in Gresham M. Sykes and Sheldon L. Messinger, "The Inmate Social System," footnote on pp. 5–7, in Cloward *et al., op. cit.*, pp. 5–19. For mental hospitals, see Ivan Belknap, *Human Problems in a Mental Hospital* (New York: McGraw-Hill Book Co., 1956); William R. Morrow, "A Persisting Clique of Chronic Mental Patients," *Human Organization,* XX (Spring, 1961), 32–35; and Richard F. Salisbury, *Structures of Custodial Care* (Berkeley: University of California Press, 1962).

[5] See especially Richard A. Cloward, "Social Control in the Prison," and Richard McCleery, "Communication Patterns as Bases of Systems of Authority and Power," in Cloward *et al., op. cit.*, pp. 20–48, 49–77. For additional findings, see Richard McCleery, "Authoritarianism and the Belief System of Incorrigibles," in Donald R. Cressey (ed.), *The Prison: Studies in Institutional Organization and Change* (New York: Holt, Rinehart & Winston, 1961), pp. 260–306; Terence Morris, Pauline Morris, and Barbara Biely, "It's the Prisoners Who Run This Prison," *Prison Service Journal,* II (January, 1961), 3–11; Joseph C. Mouledous, "Organizational Goals and Structure Change: A Study of the Organization of a Prison Social System," *Social Forces,* XLI (March, 1963), 283–90; Gresham M. Sykes, *Society of Captives* (Princeton: Princeton University Press, 1958); and Stanton Wheeler, "Socialization in a Prison Community," *American Sociological Review,* XXVI (October, 1961), 697–712.

[6] Bernard C. Berk, "Organizational Goals and Inmate Organization," *American Journal of Sociology,* LXXI (March, 1966), 522–34; Oscar Grusky, "Treatment Goals and the Behavior of Organizational Leaders," *American Journal of Sociology,* LXV (July, 1959), 59–67; Daniel Glaser, *The Effectiveness of a Prison and Parole System,* Indianapolis: Bobbs-Merrill Co., 1964); McCleery, "Communication Patterns," *op. cit.*; David Street, Robert D. Vinter, and Charles Perrow, *Organization for Treatment* (New York: Free Press, 1966); and Mayer N. Zald, "Organizational Structures in Five Correctional Institutions," *American Journal of Sociology,* LXVIII (November, 1962), 335–45.

[7] William Caudill, Frederick C. Redlich, H. R. Gilmore, and E. B. Brody, "Social Structure and Interaction Process on a Psychiatric Ward," *American Journal of Orthopsychiatry,* XXII (April, 1952), 314–34; William Caudill, *The Psychiatric Hospital as a Small Society* (Cambridge, Mass.: Harvard University Press, 1958); Milton Greenblatt, Daniel J. Levinson, and Richard H. Williams (eds.), *The Patient and the Mental Hospital* (New York: Free Press, 1957); Milton Greenblatt, Richard M. York, and Esther L. Brown, with Robert W. Hyde, *From Custodial to Therapeutic Care in Mental Hospitals* (New York: Russell Sage Foundation, 1955); H. Rowland, "Interaction Processes in a State Mental Hospital," *Psychiatry,* III (April, 1938), 323–37; and Alfred H. Stanton and Morris S. Schwartz, *The Mental Hospital* (New York: Basic Books, 1954).

to get along better with staff and to have closer relations with fellow inmates. They suggest that official goals emphasizing treatment lead to humanitarian and flexible management practices, while custodial goals lead to authoritarian and rigid patterns of control, and they propose that the former generate more positive relations than do the latter.[8]

While this interpretation is consistent with conclusions from research in other kinds of organizations, the problem remains of understanding how participative types of management lead to improved relations between subordinates and superiors and to closer relations among subordinates. Moreover, the seemingly inconsistent effects of management practices on task performance remain to be explained.

An important contribution to these problems is Goffman's observation that the role prescribed for an individual in an organization not only specifies certain patterns of behavior as required of him but also serves as an implicit definition of what sort of person he is and that a member must adapt to this organizational assumption of what he is.[9] Thus, an individual can be viewed as acquiring a set of attitudes toward himself in relation to others in the organization that both enables him to interact viably with others and makes it possible for him to live with himself.[10] Such a complex of attitudes may be termed the individual's *adaptation* to his organizational role.[11] Although the problem of adaptation exists for the members of any organization, it is particularly acute for inmates, since institutional role prescriptions tend to apply to every activity of inmates throughout the day and night and often are inconsistent with an individual's conception of himself as an autonomous person with goals, needs, beliefs, and abilities of value.[12]

For the purpose of this study, adaptations will be classified in terms of the relation between the individual's own personal goals, together with the means he employs to pursue them, and the organizational structure. Thus, three types of adaptation will be identified: *co-operative*, in which the personal goals pursued by the individual coincide with the goals others expect him to pursue as part of his role; *opportunistic*, in which goals are not shared, but the individual relies on organizational activities as means to achieve his own ends; and *alienated*, in which goals are not shared nor is the organization used as an opportunity for the pursuit of goals.[13]

A second important concept is that of an organization as a hierarchical structure of decision making, in which an individual at one level is responsible for decisions affecting those subordinate to him. From the point of view of the subordinate, then, it is possible to distinguish between those decisions for which he himself is directly responsible and those decisions made by his superior that affect him.[14] The critical point is that the latter decisions strongly affect the nature of the situation to which the subordinate must adapt. Consequently,

[8] Street, Vinter, and Perrow, *op. cit.*, pp. 225–27.

[9] Erving Goffman, *Asylums* (Garden City, N.Y.: Doubleday Anchor Books, 1961).

[10] For the general social-psychological model used here, see Daniel J. Levinson, "Role, Personality and Social Structure in the Organizational Setting," *Journal of Abnormal and Social Psychology*, LVIII (March, 1959), 170–80, and Theodore R. Sarbin, "Role Theory," in Gardner Lindzey (ed.), *Handbook of Social Psychology* (Cambridge, Mass.: Addison-Wesley Publishing Co., 1954), pp. 223–58.

[11] It should be noted that a sharp distinction is being made here between role expectations institutionalized and supported by sanctions in the organization, on the one hand, and the attitudes evoked in the individual by these expectations, on the other.

[12] See, for example, Goffman, *op. cit.*, and Sykes and Messinger, *op. cit.*

[13] The fourth logical type, congruence of goals without reliance on organizational activities, should be unstable and hence empirically rare.

[14] See James G. March and Herbert L. Simon, *Organizations* (New York: John Wiley & Sons, 1958), chap. vi. "Decisions" are construed here as involving "search" activities or "absorption of uncertainty."

the way in which he is involved in these decisions should markedly affect his type of adaptation.

Patterns of management, then, can be classified in terms of how superiors involve their subordinates in decisions, and the two such patterns of concern here can be called *bureaucratic* and *participative*. The essential characteristic of bureaucratic management is that decisions affecting subordinates are made by the superior applying rules that both establish the categories in terms of which situations are defined and specify the alternative courses of action. In contrast, participative management consists of the superior involving subordinates in decisions affecting them by having them participate in defining the nature of the situations about which decisions have to be made and in identifying the possible courses of action.[15]

The hypothesis can now be advanced that co-operative adaptations are associated with participative management, since participative practices provide mechanisms for bringing about congruence between superiors' and subordinates' definitions of situations and, moreover, place high value on such consensus, whereas bureaucratic practices do not. Further, the attitudes toward self and others comprising adaptations should influence subordinates' behavior toward superiors and peers. Consequently, as a second general hypothesis, adaptations should operate as an intervening variable between patterns of management and subordinates' relations with superiors and peers. Thus, the common finding that participative styles of management are associated with improved relations between subordinates and superiors and close relations among subordinates should be explained in part by the effects of management practices on subordinates' adaptations.

METHODS

An opportunity to investigate the effects of management patterns and adaptations on subordinates' relations with others was provided by a demonstration project in a correctional institution.[16] Three housing units in the institution were studied, two of which were administered bureaucratically while the third was operated by the demonstration project using participative management practices.

Although the institution was ideologically committed to treatment and offered numerous treatment programs, these, as well as the routines of daily living, were administered by complex systems of rules. To provide an incentive for comformity and to simplify supervision, the housing units of about 180 men each were divided into "honor" and "non-honor" units. New inmates were placed in non-honor units with relatively few privileges and close supervision, and they were permitted to move to honor units, with more privileges and less supervision, only after accumulating seniority in terms of time without disciplinary infractions. Moreover, they remained in an honor unit only as long as they stayed out of disciplinary trouble. A non-honor and an honor unit were selected randomly for study.

The third unit studied, called C Unit, was operated by the demonstration project for the purpose of experimenting with new approaches to treatment within the institu-

[15] In addition, a third type of management, autocratic, can be identified that seems to be common in custodial institutions. Autocratic management is characterized by relatively little emphasis on rational administration of rules and heavy emphasis on what Weber has called "Kadi" justice, in which the superior makes decisions on the basis of *ad hoc* judgments suited to the particular persons involved and the practical ends the superior seeks to attain. In such a case, a subordinate influences decisions primarily through his particular relation with the superior, since there is no rational system of rules on which the subordinate can depend irrespective of the personal views of the superior.

[16] The institution and demonstration project are described in detail in Elliot Studt, Sheldon L. Messinger, and Thomas P. Wilson, *C Unit: Search for Community in Prison* (New York: Russell Sage Foundation, 1968). In that report, co-operative and opportunistic adaptations are referred to as "problem solving" and "wise," respectively.

tion. In doing this, the project established a participative pattern of management within C Unit, in which inmates became involved in decisions about problems of living within the unit and about the program of the project. The C Unit population of 130 men at any one time was drawn randomly from a pool of inmates in the institution, but the eligible pool itself was not completely represesentative of the institution.[17]

The major source of data is an interview survey conducted in the three units after the project had been in operation for about sixteen months.[18] In addition, participant observation over the year and a half preceding the survey provided information about the nature of the project and institution, and institutional files were used to obtain background data on the inmates in the survey samples.

The main independent variable, pattern of management, is indicated by an inmate's housing unit: the non-honor unit was bureaucratic with low privileges; the honor unit was bureaucratic with high privileges; and C Unit was, to a far greater extent than the rest of the institution, participative and had about the same privileges as honor units. The index of type of adaptation was constructed by combining questions reflecting an inmate's attitudes toward each of up to seven staff members to whom he was related.[19] Inmate behavior toward staff was studied using questions

[17] Details of the selection procedure are given in *ibid.* The chief possible source of bias is that certain inmates with extremely bad behavior records acquired either in the reception center or in other institutions before arriving at the institution under study were excluded from the eligible pool. These inmates usually were segregated from the general population on their arrival and often spent much of their stay in segregated housing. Thus, they tended to be somewhat underrepresented in the non-honor units and seldom gained honor status. At worst, this bias might affect comparisons between the participative and non-honor units, and there is some reason to believe that this effect is negligible. See *ibid.*, Appendices A and D.

[18] The survey samples in the two bureaucratic units were drawn randomly with a sampling ratio of one in three, yielding fifty-seven and sixty inmates in the non-honor and honor units, respectively. The C Unit sample of 120 inmates consisted of all inmates who had been in C Unit for at least two weeks at the time of the survey. See *ibid.*, for further details.

[19] Complete details are given in *ibid.* Each inmate was asked two sets of questions about each role relationship he was involved in with staff: counselor, day and evening housing-unit officers, morning and afternoon work supervisors, any one "other" named by the inmate, and participation in supervised groups.

As an example, for the counselor, the first set contained two items. "When you see your counselor, there are a number of things you could talk about. One kind of thing is about procedures, requests, classification, assignments, board reports, and so on. How much of your contact with him is about things like this—would you say over half or less than half? (Probe: Almost all? Over half? About half? Less than half? Very little?)," and, "Do you ever 'talk serious' with your counselor? (Probe: How much do you talk serious with him? A lot? Some? Or not very much?)." If an inmate indicated that not all of his contact with his counselor was about procedural matters and that he "talked serious" a lot or some of the time (i.e., discussed matters the inmate felt to be important in a relatively open and frank way, as opposed to "shucking" the counselor), his attitude toward the counselor was scored as positive on a "shared goals" index. In a similar way, indexes of shared goals were constructed for the attitudes of an inmate toward each of the staff members to whom he was related.

A second set of questions was used to develop a "working relationship" index for the counselor. "How well do you think he understands you and your problems?" "When you really need something done, can you count on him to help you out?" And, "On the whole, how do you get along with him?" If an inmate responded positively to all three questions, his attitude toward the counselor was scored as positive on the working relationship index. Again, parallel questions were asked concerning the inmate's attitudes toward other staff members.

Inmates' attitudes toward a given staff member were then classified as "co-operative" (working relationship and shared goals), "opportunistic" (working relationship but not shared goals), "non-scale" (shared goals but not working relationship), or "alienated" (neither working relationship nor shared goals). Since shared goals should be unlikely to persist in the absence of a working relationship, it was anticipated that the non-scale category would be rare. This proved to be the case,

pertaining to communication with staff about three matters important in the daily lives of inmates: personal worries; rules and procedures; and annoying behavior of other inmates.[20] Inmates indicating they talked to staff about two or three of these topics were classified as "high" on the index of communication with staff, while those indicating one and none were classified as "medium" and "low," respectively. Finally, two indicators were used to describe relations among inmates. First, inmates were divided into those who wanted half or more of their inmate associates as friends after release and those wanting less than half of their associates as friends.[21] Second, inmates were asked if they talked with other inmates about their own personal problems.[22] Those inmates who both wanted half or more of their associates as friends and talked with others about personal problems were classified as having "close" relations with their peers.

FINDINGS

Participative and bureaucratic management practices are strongly associated with co-operative and opportunistic adaptations, respectively, as can be seen in Table 1. Inmates in C Unit were far more likely to have co-operative adaptations than were inmates in the bureaucratic housing units. It will be noted, in addition, that the low-privilege unit had a higher proportion of

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TYPE OF ADAPTATION, BY MANAGEMENT PATTERN (PERCENTAGES)

Type of Adaptation	Management Pattern		
	Bureaucratic		Participative (C Unit)
	Low Privilege	High Privilege	
Co-operative..	5	13	40
Opportunistic.	53	62	52
Alienated.....	42	25	8
Total.......	100	100	100
Base.......	(57)	(60)	(120)

NOTE.—$\gamma = 0.39$; $d_{yx} = 0.39$.

inmates with alienated adaptations than did the high-privilege unit, giving rise to an ordinal relation between housing unit and type of adaptation. This probably reflects the selection process by which inmates gained access to honor units, since alienated adaptations are more likely to result in non-conforming behavior, and thus ex-

since less than 9 per cent of the respondents gave non-scale responses toward any given staff member. Consequently, these were combined with the opportunistic responses.

Finally, to obtain the over-all index of type of adaptation, the indexes describing attitudes toward the several staff members were combined: "co-operative" *adaptation* (co-operative attitudes toward two or more staff members), "opportunistic" *adaptation* (opportunistic attitudes toward two or more staff members, or opportunistic attitudes toward one and co-operative attitudes toward one staff member), and "alienated" *adaptation* (neither a co-operative nor an opportunistic adaptation).

[20] The following questions were used: "Have you talked with any staff in the past several weeks about something that was really on your mind?" "In an institution there are bound to be procedures a man won't like—for instance, unlock procedures that make men wait too long, petty rules, too many shakedowns, TV rules that make for a lot of hassle. When something like this gets really bad in the unit, do you talk with other inmates about it? Do you talk about it with any staff?" And, "There'll be some inmates who do things that bother the other men in the unit—like too much noise, fires, maybe some racial tension. When things of this sort get really bad, do you talk with other inmates about it? How about with staff?"

[21] Inmates were asked to think about the other inmates they associated with most often and to assign them fictitious names. The following question was asked about each such associate: "On the streets would you want him as a close friend, a friend but not really close, just an associate, or wouldn't you want to associate with him?" Positive responses were "close friend" or "friend but not really close."

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clusion from the honor units, than are opportunistic adaptations.

When background characteristics are introduced as control variables, the relation between pattern of management and type of adaptation is unaffected.[23] This, together with the fact that C Unit did not differ markedly from the other units in the distribution of any of the background characteristics, suggests that the findings cannot be explained by possible lack of representativeness in the C Unit selection procedures.

The effects of exposure.—As with other background factors, when the amount of time inmates had spent in their respective units is held constant, the association between pattern of management and type of adaptation is unaffected. Time in unit, however, can also be used as an index of exposure to a particular pattern of management, and Table 2 is organized to facilitate examination of the effects of exposure to each pattern. Looking first at the bureaucratic–low-privilege unit, one can see that the proportion of opportunistic adaptations tends to increase with increased exposure. Since most of these inmates were new to the institution, it appears that opportunistic adaptations developed among new arrivals as the length of their stay increased. This interpretation is supported when time in the institution is added as an additional control variable, although the number of cases is too small to warrant presentation.

TABLE 2

TYPE OF ADAPTATION, BY TIME IN UNIT AND MANAGEMENT PATTERN (PERCENTAGES)

Type of Adaptation	Months in Housing Unit*			
	0–3	4–6	7–12	13 or More
	Bureaucratic–Low Privilege			
Co-operative	8	0	[0]	[0]
Opportunistic	47	62	[3]	[1]
Alienated	45	38	[1]	[1]
Total	100	100	....	....
Base	(38)	(13)	(4)	(2)
	Bureaucratic–High Privilege			
Co-operative	13	7	25	7
Opportunistic	80	67	56	43
Alienated	7	26	19	50
Total	100	100	100	100
Base	(15)	(15)	(16)	(14)
	Participative (C Unit)			
Co-operative	27	39	62	45
Opportunistic	59	58	33	45
Alienated	14	3	5	10
Total	100	100	100	100
Base	(41)	(36)	(21)	(22)

* Item: "How long have you been in —— Unit?"

In the bureaucratic–high-privilege unit, it appears that inmates who had been in the unit for a short time were very likely to have opportunistic adaptations, but as time in the unit increases, the proportion of opportunistic adaptations declines regularly, and the shift tends to be toward alienated adaptations. This suggests that during their initial period in the institution, inmates acquired opportunistic adaptations, and these became most prominent when they moved to honor units. However, as inmates remained in a particular unit, the pattern of living became routinized and, in effect, opportunities for continued pursuit of personal goals declined, with the result that opportunistic adaptations became less rewarding and thus less frequent. This interpretation presupposes that the critical factor is time spent in the same housing unit rather than the total length

[23] The factors controlled were legal status, race, father's occupation, age, intelligence, psychiatric diagnosis, alcoholic history, opiate use, non-opiate drug use, homosexual experience, family-arrest history, prior prison terms, prior jail terms, present offense, having a fixed parole date, time in institution, and time in housing unit. See Studt *et al.*, *op. cit.*, for details.

of stay in the institution, which implies that inmates with a short time in the unit should be very likely to have opportunistic adaptations, irrespective of how long they have been in the institution. This appears to be the case, for among inmates in the bureaucratic–high-privilege unit for six months or less, the percentages with opportunistic adaptations are 75 for inmates in the institution six months or less and 70 for inmates in the institution for seven or more months (percentage bases of 20 and 10, respectively).

Finally, in C Unit, co-operative adaptations increase steadily with exposure to participative management, up to about one year. The decline after this point may be attributable to inmates having exhausted the opportunities of the C Unit program as in the bureaucratic–high-privilege unit, although it is worth noting that the shift is toward opportunistic rather than alienated adaptations. When time in the institution is introduced as an additional factor (see Table 3), it appears that exposure to participative management is more likely to lead to co-operative adaptations if inmates have spent little time in the institution than if they have been in the institution for a long while before coming to C Unit. Inmates who had been in the institution for a long period before entering C Unit had been exposed to the bureaucratic patterns of the institution and should have tended to acquire opportunistic or perhaps alienated adaptations. It appears that developing co-operative adaptations is inhibited by such previous experience.

These findings indicate that co-operative and opportunistic adaptations are associated with exposure to participative and bureaucratic management patterns. Moreover, the inhibiting effect of previous exposure to bureaucratic patterns on acquisition of co-operative adaptations suggests that the attitudes comprising adaptations to organizational roles are not superficial responses to the immediate situation, but instead involve more stable characteristics of the individual. Thus the first hypothesis is supported.

Inmate communication to staff.—As anticipated from previous studies, inmates living under participative management practices in C Unit were more likely to communicate with staff than were inmates in the two bureaucratic units (see Table 4). However, the central question here is the extent to which the effects of management practices on inmate behavior toward staff are exerted through inmates' adaptations.

TABLE 3

PERCENTAGE CO-OPERATIVE AMONG INMATES IN C UNIT FOR SIX MONTHS OR LESS, BY TIME IN INSTITUTION AND TIME IN UNIT

MONTHS IN INSTITUTION	MONTHS IN C UNIT (PARTICIPATIVE MANAGEMENT)		
	0–3	4–6	Percentage Difference
0–6..........	29 (24)	45 (20)	+16
7 or more.....	24 (17)	31 (16)	+ 7

If type of adaptation acts as an intervening variable, then first of all, the tendency toward higher communication with staff in C Unit should be reduced when type of adaptation is held constant. There is some suggestion of this, particularly in the high-communication category, but C Unit inmates still remain less likely than others to be low. In addition, if type of adaptation intervenes between pattern of management and communication to staff, then an association should exist between adaptations and communication when pattern of management is controlled. This also appears to be the case, as can be seen by inspecting the differences among types of adaptation within the columns of Table 4, and as is reflected in the values of Kendall's τ-b for the relation between adaptation and communication within each unit: these are 0.27, 0.40, and 0.15 for C Unit, the high-privilege unit, and the

low-privilege unit, respectively. Finally, the association between pattern of management and type of adaptation found in Table 1 ($\tau = 0.39$) is unaffected when communication is held constant: the values of τ are 0.39, 0.32, and 0.35 for high, medium, and low levels of communication, respectively.[24] There is, then, some evidence that inmates' adaptations play a part in the mechanism by which pattern of management affects inmate communication to staff.

TABLE 4

COMMUNICATION TO STAFF, BY MANAGEMENT PATTERN AND TYPE OF ADAPTATION

(PERCENTAGES)

Communication Index	Management Pattern: Bureaucratic, Low Privilege	Bureaucratic, High Privilege	Participative (C Unit)
	All Types of Adaptation		
High	26	18	38
Medium	18	30	30
Low	56	52	32
Total	100	100	100
Base	(57)	(60)	(120)
	Co-operative		
High	[1]	[5]	52
Medium	[1]	[3]	29
Low	[1]	[0]	19
Total	....	....	100
Base	(3)	(8)	(48)
	Opportunistic		
High	33	14	31
Medium	13	32	29
Low	54	54	40
Total	100	100	100
Base	(30)	(37)	(62)
	Alienated		
High	17	7	10
Medium	21	20	40
Low	62	73	50
Total	100	100	100
Base	(24)	(15)	(10)

Closeness of relations among inmates.—Turning now to relations among inmates, one can see in Table 5 (top line) that C Unit inmates were somewhat more likely than others to have close relations with their peers, which is the direction of finding anticipated from previous studies. However, when type of adaptation is controlled, it is evident that the higher proportion of inmates with close relations in C Unit results from the high proportion with co-operative adaptations in that unit. In addition, closeness appears to be related to type of adaptation, except in the low-privilege unit. Finally, when closeness is held constant, the original relation between

TABLE 5

PERCENTAGE WITH CLOSE RELATIONS, BY MANAGEMENT PATTERN AND TYPE OF ADAPTATION

Type of Adaptation	Management Pattern: Bureaucratic, Low Privilege	Bureaucratic, High Privilege	Participative (C Unit)
All types of adaptation	25	30	38
	(57)	(60)	(120)
Co-operative	[0]	[3]	52
	(3)	(8)	(48)
Opportunistic	27	35	29
	(30)	(37)	(62)
Alienated	25	13	20
	(24)	(15)	(10)

[24] It may be noted that this pattern of findings also argues against the possibility that the findings are the result merely of contamination between the indicators of type of adaptation and communication with staff.

pattern of management and type of adaptation remains: the values of τ are 0.52 and 0.33 for inmates with and without close relations, respectively. These findings, while not as strong as for communication with staff, indicate that type of adaptation plays a part in the processes by which pattern of management affects relations among inmates.

Although the relation between participative management and close relations among inmates, in the top line of Table 5, is in the anticipated direction, it is not particularly strong. The relatively small difference of 8 per cent between the bureaucratic–high-privilege and the participative units can be attributed to an "anomalously high" rate in the bureaucratic–high-privilege unit of 30 per cent instead of around 25 per cent. An explanation for this anomaly is suggested by a detailed examination of the components of the "closeness" index: wanting half or more of one's associates as friends and talking to other inmates about personal problems.

It can be seen in the top line of Table 6 that inmates in C Unit were the most likely to want their associates as friends, and there is some tendency for this pattern to disappear when type of adaptation is held constant. However, the striking finding is that inmates in the bureaucratic–high-privilege unit were persistently the least likely to establish friendships with other inmates. In contrast, an opposite pattern appears in Table 6, Part B: inmates in the bureaucratic–high-privilege unit were the most likely to talk with others about personal problems, and this is particularly so among inmates with opportunistic adaptations. Moreover, this latter finding holds when wanting associates as friends is held constant as well (see Table 7, Parts A and B).

It appears, then, that inmates in the bureaucratic–high-privilege unit were the least likely to form friendships but at the same time frequently talked to other inmates about personal problems, especially if they had opportunistic adaptations. A suggestion as to what might be going on can be found in the character of the honor units in this institution. It will be recalled that inmates gained access to, and remained in, honor units only by conforming very strictly to the complex system of impersonal rules established by the staff. Although the same rules aplied in the non-honor units, the effective pressure to conform was less, since the sanction of loss of currently enjoyed honor-status privileges was absent. In the participative unit, on the other hand, the demand for conformity to staff expectations was high, but these expectations were not so often

TABLE 6

COMPONENTS OF CLOSENESS INDEX, BY PATTERN OF MANAGEMENT AND TYPE OF ADAPTATION

Type of Adaptation	Management Pattern: Bureaucratic, Low Privilege	Bureaucratic, High Privilege	Participative (C Unit)
	A. Percentage wanting half or more of associates as friends		
All types of adaptation..	60 (57)	52 (60)	70 (120)
Co-operative..	[2] (3)	[6] (8)	75 (48)
Opportunistic.	60 (30)	51 (37)	68 (62)
Alienated.....	58 (24)	40 (15)	60 (10)
	B. Percentage talking with other inmates about personal problems		
All types of adaptation..	30 (57)	50 (60)	43 (120)
Co-operative..	[1] (3)	[3] (8)	58 (48)
Opportunistic.	27 (30)	60 (37)	37 (62)
Alienated.....	33 (24)	33 (15)	20 (10)

a unilateral imposition by staff but instead frequently were legitimated by joint deliberation over issues viewed as important by the inmates themselves.

It can be conjectured, then, that inmates in the high-privilege unit, particularly those with opportunistic adaptations, were under considerably greater pressure to conform to externally imposed rules

TABLE 7

PERCENTAGE TALKING TO OTHER INMATES ABOUT PERSONAL PROBLEMS, BY FRIENDSHIP LEVEL AND PATTERN OF MANAGEMENT

Want Half or More of Associates as Friends	Management Pattern: Bureaucratic, Low Privilege	Management Pattern: Bureaucratic, High Privilege	Management Pattern: Participative (C Unit)
	A. All Types of Adaptation		
Yes	41 (34)	58 (31)	54 (84)
No	13 (23)	41 (29)	19 (36)
	B. Opportunistic Adaptations Only[a]		
Yes	44 (18)	68 (19)	43 (42)
No	0 (12)	50 (18)	20 (20)

[a] Cell frequencies for the other types of adaptation are too small to warrant presentation.

than were the inmates in the other two units. Apparently, successful adjustment to this pressure diminished inmates' capacity to form friendships with each other and, at the same time, led to high levels of personal tension[25] requiring some kind of management that avoided violating the rules, such as talking with other inmates about personal worries.

The consequence of this for the relation between closeness and pattern of management in the top line of Table 5 is direct. Suppose that there were no tendency for inmates in the high-privilege unit to talk more often about personal problems. Then in Table 7, Part B, the percentage talking about personal problems for inmates in the high-privilege unit who wanted half or more of their associates as friends would be about 44 instead of 68. If this "corrected" figure is used, then the percentage with close relations in that unit as a whole drops from 30 to 22 and the top line of Table 5 would read 25, 22, 38, and the percentages for inmates with opportunistic adaptations would become 27, 22, 29. Thus, the anomaly in Table 5 can be interpreted as resulting from the effects of high pressure to conform to bureaucratic rules on inmates with opportunistic adaptations.

DISCUSSION

The present study is consistent with previous research in finding tendencies for inmates to have better relations with staff and closer relations with peers under participative management than under bureaucratic supervision. In addition, there is some support for the hypothesis that these effects of management practices are exerted in part through the adaptations of subordinates to their organizational roles. Adaptations, apparently, are affected by the way the individuals are involved in decisions affecting them, which in turn affect their relations with others. However, it also appears to be necessary to take other factors into account; for example, in the present case, the effects of high pressure to conform to bureaucratic rules seem to be important, and, in theory, earlier socializing experiences should be significant as well.[26] In the light of these findings, it may be proposed that a central social-psychological mechanism affecting behavior in

[25] Findings suggestive of this same idea are reported by Arthur Kornhauser, *Mental Health of the Industrial Worker* (New York: John Wiley & Sons, 1965).

[26] As an example, see Melville Dalton, "The Industrial Rate-Buster: A Characterization," *Applied Anthropology*, VII (1948), 5–18.

organizations is the complex of attitudes an individual develops toward himself in relation to others in the organization.

Finally, it should be noted that the data of this study do not speak to one of the major problems raised at the outset, namely, the uncertain relation between management practices and task performance by subordinates. However, the model developed here does suggest a hypothesis. Patterns of management refer to the way subordinates are involved in decisions affecting them that are made by their superiors. It may be suggested, however, that a central factor influencing subordinates' task performance is the nature of the decisions for which they themselves are responsible. At one extreme, a subordinate's role may be highly routinized, requiring virtually no decisions having significant consequences for the activities of the organization. In this case, the subordinate's adaptation is less important in determining the effectiveness of his role performance than such factors as the rewards and punishments his superiors can employ, the norms of his peer group, and his personal expectations about these matters derived from past experience, since external conformity is in large part sufficient for adequate performance of routines. At the other extreme, a subordinate may be called upon to exercise continuously a high degree of discretion in order for the activities of the organization to be successful. In this case, the subordinate's type of adaptation can be very important, for if he is pursuing personal goals incongruent with those of the organization, the effectiveness of his performance in terms of organizational goals will be impaired. Thus, the importance of patterns of management in affecting subordinates' task performance should depend in large part on the kinds of decisions subordinates have to make, and consequently no simple general relation between patterns of management and performance can be anticipated. In particular, it may be suggested that participative management practices will have little effect unless subordinates have, or are given, responsibility for exercising discretion.

These considerations point to the importance of differences among organizations in terms of structures of decision making, which, as Perrow has argued, are strongly affected by the technologies of organizational activities.[27] This suggests, in turn, that in order to place the present discussion within the framework developed by Perrow, the hierarchal model of decision making with which this paper began should be replaced by a model in which decision making is differentiated both laterally and hierarchically, and the notion of pattern of management should be generalized to include the way individuals are involved in decisions affecting them that are made not only by superiors but by colleagues and by individuals in other lines of authority as well. With these modifications, the concept of adaptation provides an explicit link between social-psychological concepts and the structural concept of organizational role.

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[27] Charles Perrow, "A Framework for the Comparative Analysis of Organizations," *American Sociological Review*, XXXII (April, 1967), 194–208. See also Woodward, *op. cit.*, for an extremely important study of the effects of technology.

Immigrants and Urban Governmental Form in American Cities, 1933–60[1]

Daniel N. Gordon

ABSTRACT

The American municipal reform movement's commission and council-manager plans are designed to abolish structural features of mayor-council government historically associated with immigrant-group politics. The percentage of foreign-born persons as a correlate of municipal government in 268 American cities is analyzed for the period 1933–60. The results indicate a positive association between the presence of immigrants and mayor-council government for the entire study period. Some implications of these results for the assimilation hypothesis in urban politics are discussed.

Immigrants to America's shores have historically been an integral part of municipal politics and government in American cities. Of the three forms of municipal government (mayor-council, commission, council-manager), mayor-council is the most politicized of all three, which is reflected by its historical sensitivity and responsiveness to immigrant groups' political demands. Hence, it is assumed that in order to maximize their influence in local government, these groups want to live under the mayor-council plan.[2] This study will analyze changes in the association between immigrants and local governmental form in American cities.

Politicization of mayor-council government came about in this country's "Age of Egalitarianism."[3] The increasing belief in popular democracy in the early nineteenth century led to the political activation of the propertyless urban masses[4] who demanded changes in municipal governmental structures—change to the direct election of mayors, to the ward system for election of aldermen, and to an expansion in the number of elective offices.[5]

[1] Data collection for this study was financed in part by a dissertation fellowship from the Russell Sage Foundation Program in Sociology and Law, University of Wisconsin. I would like to thank Robert R. Alford, James A. Davis, Robert A. Feldmesser, David M. Kovenock, Ronald W. Maris, and Thomas P. Wilson for their reading of an earlier draft.

[2] Robert R. Alford and Harry M. Scoble discuss the politicized nature of mayor-council government on p. 83 of their article entitled, "Political and Socioeconomic Characteristics of American Cities," in David S. Arnold and Orin F. Nolting (eds.), *The Municipal Yearbook 1965* (Chicago: International City Managers' Association, 1965). While the politicized nature of mayor-council government undoubtedly accrues benefits to other groups, historically no group has been as deeply or consistently intertwined with this governmental form as immigrants from foreign shores. Hence, this paper focuses on immigrants.

[3] This phrase was suggested by Lee Benson as an alternative to the more well-known concept of the Age of Jackson (or Jacksonian democracy). Benson argues that in New York State after 1815 the movement for popular democratic reforms was characteristic of several parties, but *not* the Democratic party of Jackson. On the contrary, Jackson's party was politically conservative. See Lee Benson, *The Concept of Jacksonian Democracy* (Princeton, N.J.: Princeton University Press, 1961), esp. chap. xv.

[4] Arthur Schlesinger, Jr., discusses how the Jacksonians enlisted the aid of the propertyless urban masses because they represented a rich reservoir of votes. See Arthur Schlesinger, Jr., *The Age of Jackson* (Boston: Little, Brown & Co., 1945), *passim*. Although Schlesinger is apparently in error when he claims that the Jacksonians mobilized the urban masses, the evidence indicates the correctness of his claim that they *were* mobilized. See, e.g., Edward Pessen, "The Workingmen's Movement of the Jacksonian Era," *Mississippi Valley Historical Review*, XLIII (December, 1956), 428–43.

[5] Illustrative of this impact of the egalitarian ideology on local government was the Working-

The direct election of the mayor puts him directly under electorate control. The election of aldermen from small election districts (wards) facilitates constituents' contacts with and effective representation by their elected representatives. Finally, large numbers of elective offices increase the scope of electorate control over local government.

The ward system of elections and the direct election of the mayor, then, were often established as a direct response to this country's newly emergent egalitarian political ideology. These structural changes in municipal government produced a political system within which immigrants could later obtain political power.

Up until the New Deal, politicians often sought support from immigrant groups through the auspices of political machines which dispensed patronage, charitable aid, and legal advice.[6] (Currently, political machines have generally disappeared from the American political scene.[7]) The ward system of elections usually was the basis of political machines.[8] Also, according to Hays, wards have been historically important for the representation of locality-based interests. He wrote: "[Ward-elected city councilmen] spoke for their local areas, the economic interests of their inhabitants, their residential concerns, their educational, and religious interests—i.e., for those aspects of community life which mattered most to those they represented. . . . They defended the community's *cultural practices* [and] its *distinctive language* or *national customs*."[9]

The ease with which immigrants can secure representation in local government, then, appears to be inversely proportional to the size of local election districts. The elected-at-large Boston City Council demonstrates that generally "councilmen elected at large are less responsive to the voters' wishes than ones elected on a district basis." In addition, Boston's old ward system "gave minority interests better representation than does the present at-large one."[10] And Williams and Adrian's study of four manager cities shows that ward elections not only facilitate immigrant-group access to local government but also make it easier for members of these groups to get elected to the city council.[11] Ward election campaigns are cheaper than at-large campaigns,[12] and the electorate to which the candidate appeals for votes is generally smaller and more homogeneous.

Equally as important as wards to immigrant-group politics (perhaps now more important with the demise of the ward-based political machines) is the directly elected mayor. Mayoralty candidates often seek immigrant-group support. The ethnically "balanced" ticket in New York—that is, where the mayoral slate has an Italian, an Irishman, and a Jew on it—is an example

men's Movement in New York City, whose energies were largely directed to changing the shape of municipal government during the period 1829–37. See Walter Hugins, *Jacksonian Democracy and the Working Class* (Stanford, Calif.: Stanford University Press, 1960), esp. chaps. i, ii, and vii.

[6] The historical importance of the political machine for immigrant groups is chronicled in Oscar Handlin, *The Uprooted* (Boston: Little, Brown & Co., 1951), pp. 209–17. Also see Robert K. Merton's discussion of the functions of political machines in *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1957), pp. 71–82, and Edward C. Banfield and James Q. Wilson, *City Politics* (Cambridge, Mass.: Harvard University Press, 1965), pp. 116–21, esp. pp. 116 and 118.

[7] The decline of the political machine is described in Handlin, *op. cit.*, pp. 221–26, and Banfield and Wilson, *op. cit.*, pp. 121–25.

[8] Banfield and Wilson, *op. cit.*, pp. 92–93; Handlin, *op. cit.*, pp. 209–17.

[9] Samuel P. Hays, "The Politics of Reform in Municipal Government in the Progressive Era," *Pacific Northwest Quarterly* LX (October, 1964), 161; italics added.

[10] Banfield and Wilson, *op. cit.*, p. 95.

[11] Oliver P. Williams and Charles R. Adrian, *Four Cities* (Philadelphia: University of Pennsylvania Press, 1963), *passim*.

[12] James Weinstein places in historical perspective the added burden placed upon minority groups by at-large elections. See his article entitled, "Organized Business and the City Commission and Manager Movements," *Journal of Southern History*, XXVIII (May, 1962), 177.

of the politicians' quest for nationality-group support. In fact, according to Glazer and Moynihan, these groups are paramount in New York City mayoral races.[13] Also, mayors have often been guided by ethnic considerations in making their patronage appointments.[14] Finally, the office of mayor itself may be the focus of nationality-group loyalties. Wolfinger presents election data from New Haven, Connecticut, which demonstrates a continuing Italian loyalty to the Republican party in municipal elections. Italians shifted their loyalty to the Republicans when an Italian ran for mayor on the Republican ticket. Italian support for Republicans persists today regardless of Republican candidates' ethnicity.[15]

These structural changes in municipal government—the ward system and the direct election of mayors and many other elective offices—which strengthened the political voice of immigrants became the very features of local government which the American municipal reform movement set out to abolish. Objecting "to the structure of government which enabled local and particularistic interests to dominate,"[16] upper-class businessmen and professional persons in many American cities attempted to establish business-class control of city government. These upper-class groups wanted to dominate local politics "so that they might advance their own conception of public policy,"[17] which was based upon a business model. The reform movement quickly became national in scope—at first through the advocacy of the commission plan and then through the advocacy of the currently popular council-manager plan.

After Galveston, Texas, became the first city to adopt the commission plan in 1905, various policy-oriented political scientists of the day extolled the virtues of this "efficient" and "business-like" governmental form.[18] They contended that the proper handling of city affairs necessitated the concentration of power and authority in the hands of a *few elected-at-large* commissioners. In reality, however, commission government did not always work out according to the reformers' plan.

Combining policy-making and administration in the hands of a few commissioners made the commission plan very flexible in its actual (versus intended) mode of operation. If professionally trained commissioners were elected, the commission plan would operate as intended, that is, according to the business model. However, because the city commissioner's office was a poorly paid part-time job, men who ran for office generally lacked the professional qualifications to conduct the affairs of city government. At least one commission city, Jersey City, New Jersey, was even characterized by traditional machine politics—large-scale patronage and immigrant-group conflict over government.[19]

Although their job was publicized to be the impartial conduct of government, commissioners often responded to different political climates—for example, commissioners curried the favor of immigrant groups. The commission plan's nebulous status, then, makes it somewhat more variable in actual operation than that plan's successor, council-manager government.

[13] Nathan Glazer and Daniel Patrick Moynihan, *Beyond the Melting Pot* (Cambridge, Mass.: M.I.T. Press, 1963). See the chapter entitled "Beyond the Melting Pot," esp. p. 302.

[14] See, e.g., Theodore J. Lowi, *At the Pleasure of the Mayor* (New York: Free Press, 1964), *passim.*

[15] Raymond Wolfinger, "The Development and Persistence of Ethnic Group Voting," *American Political Science Review,* LIX (December, 1965), 896–908.

[16] Hays, *op. cit.*, p. 161.

[17] *Ibid.*, p. 162. Both Weinstein, *op. cit.*, and Hays contend that the business community was not primarily opposed to corruption found in many ward cities. Businessmen did object to the fact that they had little say in local affairs.

[18] See, e.g., Ford H. MacGregor, *City Government by Commission* (Madison: University of Wisconsin, 1911), and E. S. Bradford, *Commission Government in American Cities* (New York: Macmillan Co., 1911).

[19] Dayton David McKean, *The Boss: The Hague Machine in Action* (Boston: Houghton Mifflin & Co., 1940).

Seeing the commission plan's failure to attain what he considered a desirable level of efficiency and impartiality, Richard Childs proposed council-manager government. Childs patterned his plan after a business-corporation model of organization. His model assumed that only two factors, the people and the structure of government, affect the political process. He attributed differences in government performance solely to structural differences, since he further assumed *consensus* in municipal affairs. (To Childs, municipal affairs were *not* political.) Therefore, to obtain his conception of efficiency and impartiality, Childs coupled the short-ballot principles of commission government—small city councils elected at large—with the idea of an appointed manager.[20] Childs and other reformers were successful in their efforts to institute the short-ballot principles in conjunction with the commission and council-manager plans.

Table 1 shows that one-tenth (10 per cent) of the manager cities elect all their councilmen from wards as compared with one-third (38 per cent) of the mayor-council cities. Also, three-fourths (76 per cent) of the manager cities and all of the commission cities have no wards as compared with one-fourth of the mayor-council cities. Moreover, as Table 2 demonstrates, the percentage of small, medium, and large mayor-council cities with ten or more councilmen is greater than the percentages for comparably sized commission or manager cities. In fact, the percentage of *small* mayor-council cities with ten or more councilmen is greater than the percentages for *large* commission or manager cities (42 per cent as compared with 0 per cent and 2 per cent, respectively).

TABLE 1

METHOD OF ELECTING CITY COUNCILMEN BY FORM OF GOVERNMENT IN 1960 ($N = 264$)*

Method of Electing City Council, 1960	Form of Government, 1960 (Per Cent)		
	Mayor-Council	Commission	Council-Manager
Councilmen elected by wards......	40	0	10
Councilmen elected by combination of wards and at large...........	38	0	14
Councilmen elected at large.......	22	100	76
Total........................	100 ($N=115$)	100 ($N=38$)	100 ($N=111$)

* These 264 cities are all the cities (but four) with a 1930 population of 30,000 or more which can legally change their form of government. The basis for selecting this group of cities and their characteristics is discussed in the methodology section. Data sources for governmental forms are listed in n. † of Table 3. Two mayor-council cities and two manager cities did not report their form of councilmanic elections.

Historically, then, the American municipal reform movement grew from upper-class businessmen's desire to gain control of municipal government,[21] but the movement quickly spread across this country. (Most reformers advocate their governmental plans

[20] John Porter East, *Council-Manager Government* (Chapel Hill: University of North Carolina Press, 1965), chaps. ii, iii, iv, and v.

[21] Hays, *op. cit.*, and Weinstein, *op. cit.*, document the upper-class origins of the American municipal reform movement and the reformers' desire to wrest control from the hands of the lower classes. The reformers' desire to exclude minorities is also mentioned in Banfield and Wilson, *op. cit.*, pp. 170–71. Finally, Handlin, *op. cit.*, pp. 219–20, details the contradictions between the political ideology of business-class reformers and immigrants. However, regardless of the origins of municipal reform, reformers are now convinced that reform government is desirable in all cities.

TABLE 2

NUMBER OF CITY COUNCILMEN IN 1960 BY FORM OF GOVERNMENT, 1960, AND POPULATION SIZE, 1960 ($N = 267$)*

No. of Councilmen, 1960	Form of Government, 1960 (Per Cent): Mayor-Council	Commission	Council-Manager	Total
	Population Size, 1960: Small (25,000–52,000)†			
3– 4	3	8	0	3
5– 7	23	92	73	52
8– 9	32	0	18	22
10–19	32	0	9	18
20 or more	10	0	0	5
Total	100 ($N=31$)	100 ($N=12$)	100 ($N=22$)	100 ($N=65$)
	Population Size, 1960: Medium (53,000–94,000)			
3– 4	0	17	0	2
5– 7	18	83	73	51
8– 9	16	0	16	14
10–19	50	0	11	26
20 or more	16	0	0	6
Total	100 ($N=38$)	100 ($N=12$)	100 ($N=37$)	99‡ ($N=87$)
	Population Size, 1960: Large (95,000 or More)			
3– 4	2	21	0	3
5– 7	19	79	57	43
8– 9	21	0	42	28
10–19	35	0	2	16
20 or more	23	0	0	10
Total	100 ($N=48$)	100 ($N=14$)	101‡ ($N=53$)	100 ($N=115$)

* See methodology section for a discussion of the cities analyzed here. Data sources for governmental form are given in n. † of Table 3. Data sources for population size are given in n. ‖ of Table 4. One manager city did not report its number of councilmen.

† Trichotomous categories are used for all the independent variables in this study (the number of councilmen is not an independent variable) to allow for maximum variation while conserving sufficient cases for controlled analysis. Category boundaries remain consistent throughout the study period. The categories were derived as follows: First, the lowest and highest values on the independent variables (1930–60) were designated the lowest and highest boundaries, respectively. Second, the boundaries between the low and medium categories and medium and high categories for each census year (1930, 1940, 1950, and 1960) were averaged. These averaged boundaries were then designated as the boundaries between low and medium and medium and high, respectively.

‡ Rounding error.

for *all* American cities.) The commission and council-manager plans—with their at-large city councils and their absence of directly elected mayors—spread throughout the United States. However, because these governmental forms were not attuned to immigrant-group politics, it can be assumed that these groups would resist adoption of reform government. Therefore, it is predicted that the presence of large immigrant populations in cities is associated with the presence of mayor-council government.

The association between immigrants and mayor-council government may be affected by city size, economic base, regional factors, or population increases.

Kessel demonstrated a relationship between city size and form of government and also between economic base and form of government. The manager plan is most common in cities of 25,000–250,000 population, and the mayor-council plan is most common in manufacturing cities. It is usually assumed that larger populations are more heterogeneous and that they need a more politicized form of government—mayor-council. With respect to economic base, Kessel sees three possible reasons why mayor-council government is more common in manufacturing cities. First, businessmen in manufacturing cities are likely to be concerned with national rather than local markets. Therefore, local governmental organization is not important to them. Second, manufacturing cities are likely to have absentee-owned industry with managers who do not care about local government. Third, "industrial laborers . . . are more likely to suffer the same kinds of socio-economic handicaps [as are found] with [respect to] ethnic groups. They need political channels for expression, channels which the mayor-council system is more likely to provide."[22]

Wolfinger and Field attributed variations in governmental form to regional differences.[23] However, their comparison of differences in means could hide much of the variation in their independent variables. Alford and Scoble, using conventional cross-tabulation techniques, did not find a similar regional effect.[24] Because the evidence with respect to region is contradictory, this study will afford a further test of regional effects.

Finally, a high rate of population growth might lessen the association between nativity and form of government. Commenting on the impact of newcomers on city government, Kessel wrote that rapidly growing cities face many administrative problems in providing newly required services. Since these cities have many new people, "political patterns tend to be rather amorphous." In other words, the general disruption of stable patterns makes organization against the adoption of commission or manager government difficult. In contrast, cities with stable populations are apt to have well-defined and persistent political pressures."[25]

Because they may affect the association between immigrants and governmental form, then, population size, economic base, region, and population change will be used as control variables.

METHODOLOGY

The cities used throughout the time period of this analysis are all the American cities which had a population of 30,000 or more in 1930 ($N = 306$). These are the cities covered in the first report of governmental data published by the International City Managers' Association in 1934. The desire to obtain governmental data for the earliest possible year dictated the selection of this population of cities.

This group of cities represents the universe of cities with 30,000 or more population in 1930. Strictly speaking, the results presented here are applicable only to this

[22] John H. Kessel, "Governmental Structures and Political Environments: A Statistical Note," *American Political Science Review*, LVI (September, 1962), 615–62. The quotation is from p. 619.

[23] Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," *American Political Science Review*, LX (June, 1966), 306–26.

[24] Alford and Scoble, *op. cit.*, pp. 90 and 94.

[25] Kessel, *op. cit.*, p. 617.

large and important group of cities. However, comparison of these cities with American cities of 25,000 or more population (1960) will lend perspective as to any broader generality of the results presented here.[26]

The cities analyzed here include 94 per cent of the cities with 1960 populations of 100,000 or more and 75 per cent of the cities with 50,000 or more. With respect to the distributions of foreign born in the two sets of cities (our set and the set of 25,000+), the distributions are approximately similar. Fifty-six per cent, 39 per cent, and 5 per cent of the 25,000+ cities have low (0–5), medium (6–15), and high (16 or more) percentages, respectively. The corresponding percentages in the population of cities used here are 44 per cent low, 48 per cent medium, and 8 per cent high. The distributions on the dependent variable (governmental form) are also similar. Thirty-six per cent of the 25,000+ cities have mayor-council government, while 12 and 52 per cent have commission and manager governments, respectively. These percentages compare with 43 per cent mayor-council, 14 per cent commission, and 42 per cent manager in the population of cities used here.

The principal bias of the cities used here is their underrepresentation of rapidly growing cities. Sixty-three per cent of this study's cities increased in population between 1950 and 1960 as compared to 81 per cent of the 25,000+ cities. (Correspondingly, 37 per cent of the cities analyzed here and 19 per cent of the 25,000+ cities declined during this period.) Of the 25,000+ cities, 16, 25, and 40 per cent showed low (0–10 per cent), medium (11–29 per cent), and high (30 per cent or more) growth, respectively. The percentages for the cities used here are 20 per cent low, 25 per cent medium, and 18 per cent high.

Since population change will be employed as a control variable, a correction for the population-growth bias is built into this study. Results presented here are probably applicable to all cities of 25,000–30,000 or more population. No applicability is intended for small (less than 25,000 population) cities.

Using governmental form as an indicator of immigrant-group political activity presupposes that cities are free to choose their form of government. Of course, this assumption is untrue for some cities because their states prohibit one or more governmental forms. To obtain information about state proscriptions on local government, letters were sent to state municipal leagues requesting such information. Statutory data for non-responding states were obtained by search of state constitutions and statutes. Lack of any law regulating municipal government was interpreted to mean an absence of state restrictions. All cities whose governmental form is prescribed by state law were eliminated from the sample. The working sample consists of 268 cities.[27]

Regardless of legal considerations, it may be argued that governmental form could not be affected by immigrant-group politics because cities are not realistically free to change their form of government. Politicians, government personnel, and others have organizational and career interests at stake and will vigorously oppose any attempt at change. Advocates of governmen-

[26] I would like to thank Professor Robert R. Alford of the University of Wisconsin for providing me a portion of his data from which the comparisons made here were drawn. These comparisons involve only cities which are *legally free* to change their form of government. (A discussion of legal requirements with respect to governmental forms follows these comparisons.) Since the data supplied were grouped by Professor Alford, the categories from which comparisons are made are not identical with those used in this study.

[27] Indiana cities were eliminated because they are required to employ mayor-council government. Pennsylvania cities were eliminated because Philadelphia, Pittsburgh, and Scranton must use the mayor-council form while the remaining cities must employ the commission form. Alabama cities were eliminated because the council-manager plan is prohibited. Baltimore, Maryland, and Louisville, Kentucky, must have mayor-council government, so they, too, were eliminated. Finally, because it has no home rule, Washington, D.C., was not included.

tal change, then, must overcome serious roadblocks to achieve change. However, the roadblocks are not insurmountable. Since 1900 approximately 60 per cent of our sample cities changed their form of government.[28] Of this 60 per cent, 40 per cent ($N = 67$) changed their form of government after 1930. Apparently change is quite possible and not at all unusual.[29]

The percentage of foreign-born persons was chosen as the independent variable. Immigrant groups in American cities—be they the Irish of Boston, the Italians, Irish, and eastern European Jews of New York, the Polish and Germans in Milwaukee, etc.—have traditionally been involved in municipal politics. Politics was often an avenue of social mobility for new citizens. And electoral success by their members still provides a source of pride to nationality groups. In other words, municipal politics cuts across national-origin lines and has involved most immigrant groups.[30]

Data comparisons between years and between control groups are facilitated by an index of relationship. Because the dependent variable is nominal, Cramer's V was chosen as the measure of association.[31] The value of V varies between zero and one regardless of the number of rows and columns and is independent of sample size.[32]

[28] It is reasonable to assume that *all* the cities analyzed here at one time employed mayor-council government, since that was the only governmental form (with the exception of the town-meeting form) extant before 1905.

[29] Approximately 14 per cent ($N = 38$) of the cities changed their form of government *more* than once after 1900.

[30] Of course, probably not all immigrant groups have involved themselves in politics. But the involvement of immigrants has been sufficiently widespread to consider these groups a single entity.

[31] Measures of association like Kendall's tau and Goodman and Kruskall's gamma assume ordinal data. See Robert H. Somers, "A New Asymmetric Measure of Association for Ordinal Variables," *American Sociological Review*, XXVII (December, 1962), 802.

[32] Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill Book Co., 1960), p. 230.

RESULTS

Comparison of the distributions of governmental forms in 1933 and 1960 (see the totals column in Table 3) reveals that the total percentage of mayor-council cities in the sample has remained fairly constant—49 per cent in 1933 and 43 per cent in 1960—while the total proportion of manager cities has increased by over one-half (26 per cent and 42 per cent, respectively). Also, the proportion of mayor-council cities with wards and at-large elections has remained fairly constant. Commission cities, in contrast, have declined from one-quarter to one-sixth of the sample between 1933 and 1960. Of all the cities which changed their form of government during the period 1933–60 ($N = 67$), 73 per cent changed to the manager plan, 22 per cent changed to the mayor-council plan, and only 5 per cent changed to the commission plan. We can conclude that there is a trend toward manager government.

Is the proportion of immigrants still a correlate of mayor-council government, in spite of this trend? The answer appears to be yes (see Table 3).

Table 3 reveals that of the cities with a low percentage of foreign-born persons in 1930, 22 per cent employed the mayor-council plan with wards (in 1933) as compared with 58 per cent of the cities with a high percentage of foreign-born persons. In contrast, 34 per cent and 10 per cent of the cities with a low or a high percentage of foreign born (1930), respectively, had manager government with at-large elections in 1933. In addition, of the cities with a low or a high proportion of foreign born, 29 per cent and 22 per cent, respectively, used commission government.

Data presented in Table 3 further show that the percentage of foreign born is still a correlate of mayor-council government. We see that of the cities with a high percentage of foreign-born persons, 65 per cent had mayor-council government in 1933, 73 per cent in 1940, 61 per cent in 1950, and 65 per cent in 1960. Correspondingly, the

TABLE 3

FORM OF GOVERNMENT IN 1933, 1940, 1950, AND 1960 BY THE PERCENTAGE OF FOREIGN-BORN PERSONS IN 1930, 1940, 1950, AND 1960, RESPECTIVELY
($N = 268$)

FORM OF GOVERNMENT	PERCENTAGE OF FOREIGN BORN			
	Low* (0%–5%)	Medium (6%–14%)	High (15%–37%)	Total
1933 ($V = .259$):				
Mayor-council (wards)†	22	33	58	42
Mayor-council (at large)	8	5	7	7
Commission‡	29	27	22	25
Council-manager (wards)	7	6	2	4
Council-manager (at large)	34	29	10	22
Total	100 ($N=76$)	100 ($N=66$)	99§ ($N=125$)	100 ($N=267$)‖
1940 ($V = .271$):				
Mayor-council (wards)	25	37	61	41
Mayor-council (at large)	7	4	12	8
Commission	27	26	18	24
Council-manager (wards)	11	5	1	6
Council-manager (at large)	30	28	8	22
Total	100 ($N=83$)	100 ($N=93$)	100 ($N=92$)	101§ ($N=268$)
1950 ($V = .188$):				
Mayor-council (wards)	26	43	48	38
Mayor-council (at large)	9	4	13	8
Commission	20	21	20	20
Council-manager (wards)	12	6	4	8
Council-manager (at large)	34	26	15	26
Total	101§ ($N=101$)	100 ($N=94$)	100 ($N=71$)	100 ($N=266$)#
1960 ($V = .288$):				
Mayor-council (wards)	17	49	41	34
Mayor-council (at large)	9	6	24	9
Commission	16	11	21	14
Council-manager (wards)	16	7	0	10
Council-manager (at large)	41	27	14	32
Total	99§ ($N=117$)	100 ($N=118$)	100 ($N=29$)	99§ ($N=264$)**

* Categories for the percentage of foreign born were derived in the same way as the categories for population size. See n. * in Table 2.

† Wards means that some or all of the city councilmen are elected from wards. Data sources for the forms of government and council elections were obtained from the *Municipal Yearbook* for the years 1934 (Table I), 1941 (Table II), 1951 (Table III), and 1961 (Table IV). All these volumes were published in Chicago by the International City Managers' Association in 1934, 1941, 1951, and 1961, respectively. Since the data reported in these books are for January in the year they were published, the governmental data really reflect the governmental forms existing in the previous year.

‡ Commission government, by definition, has no wards.

§ Rounding error.

‖ One city did not report its form of council elections.

Two cities did not report their form of council elections.

** Four cities did not report their form of council elections.

percentage of cities with large immigrant populations employing manager government was 12, 9, 18, and 14 in 1933, 1940, 1950, and 1960, respectively. In general, the percentages of low-, medium-, and high-immigrant cities which employ mayor-council government have remained fairly steady.

However, there has been a shift upward in the percentages of cities with high foreign-born populations using at-large elections with the mayor-council plan. Of the cities with a high percentage of foreign born, 7 per cent in 1933, 12 per cent in 1940, 13 per cent in 1950, and 24 per cent in 1960 had mayor-council government with at-large elections.) The possible significance of this result will be discussed shortly.

In general, controls for region, economic base, population size, and population change do not attenuate the association between nativity and governmental form (see Table 4). However, in cities with high population growth (1930–60), the association is reversed in 1960 (and only 1960). (Since Cramer's V does not indicate the direction of association, this reversal is not obvious.) Of the high-growth cities, 15 per cent with a low percentage of foreign born had the mayor-council plan with wards as compared to none of the cities with a medium percentage of foreign born. Ten per cent of these high-growth cities with a low percentage of foreign born had mayor-council government without wards, as did 10 per cent of these cities with a medium percentage of foreign born. Also, 5 per cent of these cities with low foreign-born populations had the commission plan, whereas none of the cities with medium foreign-born populations had this plan. Finally, of the high-growth cities, 70 per cent of them with low foreign-born populations had manager government with at-large elections, as did 80 per cent of the cities with medium foreign-born populations. (Ten per cent of these cities with a medium percentage of foreign-born persons had the manager plan with wards.) Only two cities—one with an at-large mayor-council government and one with an at-large manager government—had a high percentage of foreign born.

This attenuation in high-growth cities in 1960 suggests a possible disruption effect. Specifically, it is possible that immigrants maintain their traditional political behavior where they are present in large numbers and where the social milieu is fairly stable. However, population increases, at least in local politics where immigrant groups interact with the larger population, tend now to break up stable cultural political patterns. Immigrant groups are no longer sufficiently solidary to maintain their habitual political patterns in the face of many newcomers to the community. Of course, the initial disruption in immigrants' political patterns does not necessarily entail an immediate switch in the groups' political sentiments, although these sentiments can also be expected to change eventually.

Comparison of changes in governmental form as related to nativity can provide another perspective to the previous results. Because the mayor-council plan is assumed to be more consonant with immigrant politics than either the commission or manager plans, the magnitude of foreign-born populations should be positively related to adoption of mayor-council government and negatively related to abandonment of that plan.

Table 5 presents a three-decade (1933–40, 1940–50, and 1950–60) analysis of the immigrant correlates of changes in governmental form (see Table 5).

Of the 1933 mayor-council cities, 4 per cent and 5 per cent with a low or a high percentage of foreign born, respectively, abandoned that governmental form. Also, of the 1933 commission and manager cities, 4 per cent, 7 per cent, and 7 per cent with low, medium, and high percentages of foreign born, respectively, switched to mayor-council government. This lack of association between nativity and governmental change (1933–40) is further reflected by the small magnitude of Cramer's V. (Cramer's $V =$ .050 for the association between nativity and abandonment of mayor-council government. And $V =$ .063 for the relationship

TABLE 4

CRAMER'S *V* FOR THE RELATIONSHIP BETWEEN THE PERCENTAGE OF FOREIGN-BORN PERSONS AND GOVERNMENTAL FORM WITH AND WITHOUT CONTROL VARIABLES, FOR THE YEARS 1933, 1940, 1950, AND 1960

CONTROL VARIABLE	YEAR			
	1933	1940	1950	1960
None	.259	.271	.188	.288
Region:				
East*	.271	.177	.146	.245
Midwest†	.165	.252	.289	.438
West‡	.340	.485	.376	.339
South§	.253	.157	.221	.199
Percentage employed in manufacturing:				
Low (16–38)‖	.215	.272	.218	.238
Medium (39–48)	.306	.202	.209	.278
High (49–80)	.270	.403	.212	.491
Population size:				
Small (25,000–52,000)#	.304	.275	.320	.340
Medium (53,000–94,000)	.219	.241	.220	.423
Large (95,000 or more)	.306	.337	.226	.278
Proportion population change:**				
Decrease–small increase (0.032–1.495)	.266	.214	.162	.306
Medium increase (1.496–2.156)	.305	.453	.367	.484
Large increase (2.157–71.00)	.315	.547	.375	.336††

* The eastern states are Maine, New Hampshire, Massachusetts, Connecticut, Rhode Island, New York, New Jersey, Maryland, and Delaware. No Vermont cities were in the sample because of their small size. Pennsylvania cities were eliminated from the sample (see n. 27 above).

† The midwestern states are Ohio, Illinois, Wisconsin, Minnesota, Iowa, South Dakota, and Nebraska. No North Dakota cities were in the sample because of their small size. Indiana cities were eliminated from the sample (see n. 27 above).

‡ The western states are Montana, Colorado, Utah, Arizona, California, Oregon, and Washington. No Idaho, Wyoming, New Mexico, Nevada, Alaska, or Hawaii cities were included in the sample because their cities were too small in 1933.

§ The southern states are Texas, Oklahoma, Kansas, Missouri, Arkansas, Louisiana, Mississippi, Florida, Georgia, North Carolina, South Carolina, Virginia, West Virginia, Kentucky, and Tennessee. Alabama cities were eliminated from the sample (see n. 27 above).

All of the regional divisions correspond to those used by Alford and Scoble, *op. cit.* (cited in n. 2 above).

‖ The percentage employed in manufacturing was obtained by dividing the total employed labor force into the number of workers in manufacturing and mechanical industries (1930) and by dividing the total employed labor force into the total number of craftsmen, foremen and kindred workers, and operatives and kindred workers (1940, 1950, and 1960). Although the census categories of occupations changed between 1930 and 1940, 1950, and 1960, the three latter years appear to be close enough to the 1930 definition to make a meaningful comparison. The data sources for occupation were as follows: 1930: U.S. Bureau of the Census, *Population*, Vol. IV: *Occupations by States* (Washington: U.S. Government Printing Office, 1932), Tables 4 and 5; 1940: U.S. Bureau of the Census, *Population*, Vol. II: *Characteristics of the Population* (Washington: U.S. Government Printing Office, 1943), Table 33 (cities less than 100,000 population), and Vol. III: *The Labor Force*, Table 11 (cities with populations of 100,000 or more); 1950: U.S. Bureau of the Census, *Census of Population. 1950*, Vol. II: *Characteristics of the Population* (Washington: U.S. Government Printing Office, 1952), Table 35; 1960: U.S. Bureau of the Census, *Census of Population: 1960*, Vol. I: *Characteristics of the Population* (Washington: U.S. Government Printing Office, 1963), Table 74.

The low-, medium-, and high-percentage categories of the labor force employed in manufacturing were derived in the same manner as the population-size categories. See n. * of Table 2.

\# The derivation of the population-size categories is explained in n. * of Table 2. The data sources for population size were as follows: 1930: U.S. Bureau of the Census, *Population*, Vol. III: *Reports by States* (Washington: U.S. Government Printing Office, 1932), Table 15; 1940: U.S. Bureau of the Census, *Population*, Vol. II (cited in n. ‖ above), Tables 31 and 34; 1950: U.S. Bureau of the Census, *Census of Population: 1950*, Vol. II (cited in n. ‖ above), Table 35; 1960: U.S. Bureau of the Census, *Census of Population: 1960*, Vol. I (cited in n. ‖ above), Table 34. These sources were also used to obtain the percentages of foreign-born persons.

** The proportion of population change from 1900–1930 was used as the control variable for 1933. For 1940, 1950, and 1960 the proportion population change for 1930–60 was used. The category boundaries were obtained in the same manner as those for population size. The data source for 1900 population (from which change 1900–1930 was computed) was the U.S. Census Office, *Population*, Part I (Washington: U.S. Census Office, 1901), Table 8. A few cities were not incorporated in 1900. Population size for them was first obtained for 1910 from the U.S. Bureau of the Census, *Population, 1910* (Washington: U.S. Government Printing Office, 1913), Tables II and III.

†† Since Cramer's *V* does not give the direction of association, the reader should be informed that this association was the reverse of all the others.

between nativity and adoption of that plan.)

Between 1940 and 1950, the association between nativity and adoption of mayor-council government, while not very large ($V = .097$), is the reverse of that for the previous decade. The association between nativity and abandonment of mayor-council government is somewhat larger ($V = .144$), also the reverse of expectation. The absence of a relationship between the percentage of foreign born and change (1933–40) and the slight reversal (1940–50) indicate a possible decline in immigrant politics because of declining interest and/or numbers.

However, between 1950 and 1960 there is an apparent resurgence of the immigrant influence on governmental form. The association between nativity and abandonment of the mayor-council plan is in the expected direction, and Cramer's $V = .210$. The percentage of foreign born is also positively related to adoption of the mayor-council plan ($V = .221$).

No reason is immediately apparent for the earlier absence and subsequent resurgence of an immigrant impact on governmental change. However, consideration of nationality groups' status in our population of cities presents some interesting possibilities for consideration.

TABLE 5

CHANGES IN FORM OF GOVERNMENT 1933–40, 1940–50, AND 1950–60 BY THE PERCENTAGE OF FOREIGN-BORN PERSONS IN 1930, 1940, AND 1950, RESPECTIVELY

Form of Government	Percentage Switching to	V	Percentage of Foreign Born							
			Low (0%–5%)		Medium (6%–14%)		High (15%–37%)		Total	
			%	N*	%	N	%	N	%	N
1933:	1933–40:									
Mayor-council..	Other	.050	4	(23)	8	(25)	5	(78)	6	(126)
Other..........	Mayor-council	.063	4	(53)	7	(42)	7	(43)	6	(138)
1940:	1940–50:									
Mayor-council..	Other	.144	7	(27)	3	(38)	13	(67)	9	(132)
Other..........	Mayor-council	.097	5	(56)	5	(55)	0	(25)	4	(136)
1950:	1950–60:									
Mayor-council..	Other	.210	22	(36)	14	(44)	2	(43)	12	(123)
Other..........	Mayor-council	.221	2	(66)	6	(51)	18	(28)	6	(145)

* The numbers in parentheses are the bases upon which the percentages were computed.

DISCUSSION

Large-scale immigration ended in the 1920's with the passage of restrictive immigration laws, and this fact is reflected in our population of cities. The percentage of foreign-born persons declined between 1930 and 1960 in *all* of the 268 cities analyzed here. Also, cities' ranks with respect to the size of their foreign-born populations has remained relatively constant. The percentage of foreign-born persons in 1930 correlates (Pearsonian r) .95, .94, and .89 with the percentages of foreign-born persons in 1940, 1950, and 1960, respectively. Finally, Hadden and Borgatta report that in 1960 the percentage of foreign-born persons correlated .87 with the percentage of native-born persons with foreign or mixed parentage.[33] Therefore, the following conclusions seem probable.

First, the persons comprising the immigrant populations within cities in 1930 and

[33] Hadden and Borgatta analyzed all cities with 25,000 or more population in 1960. See Jeffrey K. Hadden and Edgar F. Borgatta, *American Cities: Their Social Characteristics* (Chicago: Rand McNally & Co., 1965), p. 128.

1960 are basically the same group, although thirty years older in 1960. Of course, some immigrants died from old age, some left the cities, and some new ones came in. However, there is no reason to assume that old immigrants dying or leaving the cities were essentially replaced by new ones, because all cities declined with respect to their percentages of immigrants. Further, if immigrants dying or leaving the cities were replaced, replacement would have to be proportional, since cities generally maintained their rank on the size of their immigrant populations. Finally, the high correlation in 1960 between the percentages of foreign-born persons and the children of foreign-born persons indicates that they tend to stay in their parents' home city. In sum, urban immigrant populations decline, but their children apparently live on in these cities. With the passage of time, then, nationality groups have lost their immediate ties to their countries of origin and have increasingly become Americans with foreign ancestry. (This would be true even if there had been a complete reshuffling of immigrants and their children between cities.) Given these developments, the results presented here have implications for the assimilation hypothesis in municipal politics.

Broadly stated, the assimilation hypothesis concludes that with increasing exposure to American political institutions, nationality groups behave less like their Old World counterparts and more like Americans. It is commonly assumed that local politics are relevant for immigrants only during their earliest years as citizens. It is taken for granted that local politics, especially machine politics often associated with mayor-council government, have traditionally served as an avenue of social mobility and as a means of charitable assistance. However, it is reasoned that increasing prosperity and increasing exposure to American behavior patterns decrease the salience of local politics to immigrants. In addition, New Deal and subsequent welfare legislation has also eliminated immigrant groups' dependence upon local politicians for minor patronage positions and small favors. Since, with some exceptions, the severance of traditional immigrant-political ties is assumed to be nearly complete, there should be little or no current association between nativity and form of government.

While the data do indicate a decline between 1933 and 1950 in the relationship between nativity and governmental form, they also indicate a resurgence in the association between 1950 and 1960. Why was this apparently assimilative process reversed? One possible explanation was offered by Raymond Wolfinger—his "mobilization theory of ethnic voting." This theory, in Wolfinger's own words, states:

> The strength of ethnic voting depends on both the intensity of ethnic identification and the level of ethnic relevance in the election. The most powerful and visible sign of ethnic political relevance is a fellow ethnic's name at the head of the ticket, evident to everyone who enters the voting booth. Middle-class status is a virtual prerequisite for candidacy to a major office; an ethnic group's development of sufficient political skill and influence to secure such a nomination also requires the development of a middle class. Therefore, ethnic voting will be greatest when the ethnic group has produced a middle class, i.e., in the second and third generations, not in the first.[34]

Of course, no data presented here are directly related to voting. However, some speculative discussion can be fruitful, since these data are not inconsistent with Wolfinger's view.

As Wolfinger's New Haven data show, the office of the directly elected mayor—which is generally synonymous only with mayor-council government—provides a focus for ethnic loyalties. The appearance of a "fellow-ethnic's name at the head of the ticket" is a source of ethnic pride. The reform plans of government have no head of the ticket. Rather, commissioners and councilmen are generally elected as a body of co-

[34] Wolfinger, *op. cit.*, p. 905; this passage is italicized in the original.

equals within an inherently nonpartisan atmosphere. In other words, reform governments reduce the stakes of elective office sufficiently to reduce their possible ethnic relevance.

Of course, there have been some changes in immigrant-group politics. The decline between 1933 and 1960 of ward elections in mayor-council cities with large foreign-born populations (refer to Table 3) attests to this. But wards were most crucial during the earliest days of immigrant politics, especially during the era of immigrant-supported political machines. (Perhaps cities with large immigrant populations were the most machine ridden. Hence, these were the cities in which reformers made special efforts to at least eradicate wards.) Neither Boston nor New York (where the ethnically "balanced" mayoral ticket is still much in evidence) now has small ethnically homogeneous wards. Yet ethnicity remains very relevant in both cities.

"Over the generations . . . the triumph of acculturation in America has been, if not complete, at least *numerically* and *functionally overwhelming*."[35] This view of Milton Gordon's has received some currency in the sociological literature.[36] However, the data presented here indicate that Gordon's conclusion overestimates the demise of the immigrant tradition, at least in the politics of urban America. The immigrant tradition will apparently survive where nationality groups remain in sizable numbers.

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[35] Milton M. Gordon, *Assimilation in American Life* (New York: Oxford University Press, 1964), p. 110; italics added.

[36] See, e.g., Will Herberg, *Protestant, Catholic, Jew* (New York: Doubleday & Co., 1960). Herberg feels that the immigrant experience in America generally conforms to the Anglo-American ideal.

Conventionalization and Control: An Examination of Adolescent Crowds[1]

Thomas S. Smith

ABSTRACT

Detailed analysis of repeated teen-age resort riots is presented to illustrate selected processes involved in crowd conventionalization. A model of conventionalization is constructed which emphasizes the interdependence of police control, public opinion, and crowd action. Special consideration is given to neglected properties of crowd situations which can be accounted for in terms of patterns of police control and the interdependence of status systems and modes of crowd participation.

I

Several types of collective phenomena are regular, expected parts of the social landscape without being "routine" in any consistent theoretical sense. Parties, festivals, ceremonials, commemorative gatherings, and similar events are appropriately studied as part of the field of collective behavior. Rather than being routine, they are gatherings which have been characterized as "conventional." That is, they are regular and periodic; they have some attributes of control, and they are recurrent. While some social scientists have been led to remark upon the functions of such conventional events, there has been no systematic effort to describe and explain the processes through which they emerge and endure.

"Conventionalization"—a term derived by Ralph Turner and Lewis Killian[2] from Herbert Blumer's distinction among "casual," "expressive," "active," and "conventional" crowds[3]—is an appropriate name for these processes. It denotes a transformation in the attributes of a crowd toward those defining Blumer's "conventional" type, namely, crowds which are "regularized" or have patterned and anticipated attributes and which also display characteristics of control facilitating and disposing their recurrence.

This paper examines a form of crowd conventionalization illustrated by an intensive case study of repeated adolescent beach riots. The pattern of these events is quite familiar, since accounts of similar events have appeared in the news media for a number of years. Their patterning and recurrence, and the changes they have undergone, suggest some of the neglected properties of crowd situations. It is in terms of some of these properties that a preliminary model of conventionalization may be constructed. The analysis will attempt to show: (*a*) that the repetition of these crowd actions can be explained partially by examining their cultural significance, their meaning as symbolic dramas, in the experience of the participants, police control agencies, and wider

[1] The author wishes to acknowledge the perceptive criticism of earlier drafts of this paper by Jan Hajda of the Department of Sociology, Portland State College; Morris Janowitz, David Street, and Donald Levine of the Department of Sociology, University of Chicago; and Max Heirich of the Department of Sociology, University of Michigan.

[2] Ralph Turner and Lewis Killian, *Collective Behavior* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1957), pp. 143–61, 527–29; cf. Ralph Turner, "Collective Behavior," in Robert E. L. Faris (ed.), *Handbook of Modern Sociology* (Chicago: Rand McNally, Inc., 1964), pp. 382–425.

[3] Herbert Blumer, "Collective Behavior," in Alfred McClung Lee (ed.), *Principles of Sociology* (rev. ed.; New York: Barnes & Noble, 1951), pp. 178 ff.

public audiences; (*b*) that the salience of the events can be understood through tracing their social impact upon relevant social structures; and (*c*) that the crowd action became regularized as a symbolic activity within adolescent culture by influencing, through its "status threat" potential, the stability of adolescent peer group leadership. What we believe we have observed in these crowd actions is a development here called "ceremonialization," which we treat as a type of conventionalization. Preliminary explanation of these developments is proposed later in this paper through an analysis of changes and emergent tendencies in the repeated encounters between the adolescent crowds and an adjacent authority system.

II

Case materials.—The events recounted here are the product of personal observations, interviews with participants and the police, a review of newspaper accounts, and access to tactical reports of state police. Detailed field notes, made after the first outburst, led to the collection of the rest of the data, including further observational materials from similar disturbances occurring over the following five years. Altogether, the materials cover the period between the Labor Day weekend in 1959, when the first outburst occurred, and the Labor Day weekend in 1964, when the last observations were made. The events occurred in Ocean City, Maryland.

On Labor Day eve in 1959 there were some 75,000 vacationers in Ocean City, most of them from Baltimore, Washington, and Philadelphia. It was not a quiet night; the bars and nightclubs were filled, and the Boardwalk was congested with people in all states of celebration. The police were busy, as was normal for the last big weekend of the summer season.

An hour before midnight one of the local policemen arrested a youth for disorderliness and intoxication, while a group of the boy's friends looked on. The arrest was clumsy, the policeman using his nightstick to stun the boy and prod him from the gathering of spectators. Before the officer was out of sight, the episode had become a focus for bystanders, and a mood of discontent had begun to spread.

State police records indicate that the first alert was received at their Salisbury barracks just before midnight. A teletype message requesting assistance had been sent from the chief of police in Ocean City. In the very center of the resort, on the Boardwalk at Ninth Street, in a location where they had traditionally congregated for the preceding half-decade, a crowd of about one thousand teen-agers had begun to riot and was out of control. Of the score of police in Ocean City, all but the chief and one of his assistants were temporary summer employees without training or police experience. Sharing a local enthusiasm for maintaining an image of their town as a relaxed family resort, they had never been very successful in managing the town's growing teen-age clientele. On successive weekends, for several seasons, they had watched their town being transformed by the influx of adolescents, who were attracted to the resort by the prospects of "cutting up," partying, and by the ease of buying liquor from among the large number of marginal businesses opened only for the summer. The 1,700 natives of the town dubbed these Friday-through-Sunday immigrants "weekend warriors," an appellation the teen-agers enthusiastically adopted into their own argot and which, at last, the riot had chilled with the tincture of reality.

By the time the state police could establish a command post in Ocean City, the rioting crowd of youths had swollen in size and intensity. The police estimated the crowd at about two thousand. The noise and the prospect of excitement attracted most latecomers, who joined earlier arrivals in milling about the scene of the arrest. They protested, intensifying the unrest which their earlier experiences with the resort authorities had already made explicit. There were a number of rallying cries, but one—

"On to City Hall!" the spot to which the arrested youth had been transported—was finally adopted by a succession of spontaneous leaders, who used it to generate movement in the crowd. The city hall became its target.

Massed together, the members of the crowd forced a path through the streets, over police cars and fire engines and through manicured gardens and miniature golf courses, toward the inlet between the ocean and the bay at the southern edge of the narrow barrier reef where the city had originated. Fire engines, ambulances, and police cars hastily retreated to a point in advance of the moving crowd, where they were deployed across streets that led to the target, a few blocks behind them. In the street along which the crowd advanced, fifty state policemen formed a phalanx to arrest its progress and otherwise intimidate its participants with the regalia of riot control: gas masks, tear-gas rifles, clubs, assorted guns, flares, megaphones, and walkie-talkies. Behind the barrier and on the tops of adjacent vehicles, local officials and state police considered alternative strategies for control should the one behind which they stood prove ineffective. The mayor declared a curfew; the field force commander of the state police read the ordinance into a megaphone and otherwise attempted to reason with the crowd periphery; and the fire chief threatened to play a stream of water into the path of the advance.

When the motion of the oncoming crowd stopped, 25 yards separated the two sets of faces, by then confronting one another fully aware of the probable outcome of their encounter. For a full quarter of an hour, the two sides stood poised in a tense equilibrium. On the part of the teen-agers, there seemed to be an unsurrendering but defeated defiance, exploring, in assorted individual acts and shouts (which in retrospect would seem irresponsible to their authors) the extent to which the authorities, appropriately equipped to suppress their challenge, could be innocuously provoked while preoccupied with an inactive but larger threat.

The police were well aware of their capacity to control the situation. They sought only to restore public order by dispersing the crowd. But to do so implied the tactic of decomposing it, fragmenting it into smaller collectivities less resistant to control and, ultimately, into the personalities that made it up. Then it would be not simply legally rational and clearly legitimate but also an incumbent duty upon them to treat the youths as individuals whose rebellious actions, however understandable in the mass, were explicitly defined within the law in terms of repressive legal consequences. To do less, they felt, would be to discredit their own competence; yet to do anything at all before fragmenting the crowd, before depressing the emotional transport that held its members, created dilemmas in a different order of significance than that urging the control of the situation upon them.

As the crowd pressed toward the barrier, it moved unknowingly into a pocket of control. Only to the north, in the direction from which it had come, was additional movement still possible. To the police, concerned with preserving property and preventing injuries, the best possible outcome in the situation was for the crowd to disperse spontaneously, just as it seemed to have arisen. It seemed rational to make arrests only for their symbolic value and, indeed, somewhat futile and unjust to make them in order to punish individuals. They sought, not to apply the law, but to restore the order within which the law functioned.[4]

[4] Indeed, because riots like this one have structures unamenable to describing a locus of legally defined responsibility, the acts they promote, both within themselves and directed against them, occur in a distinctly impersonal matrix. Police control in such situations assumes a quasi-military ethic: the allocation of repression proceeds according to a set of tactical principles which randomizes the distribution of targets for sanctions, with respect to personalities, and systematizes its allocation,

When they observed the crowd of teen-agers confining itself before them, and watched its forward motion stop, the police began to assert their control. The crowd, whose actions had been somewhat unpredictable, was now "in hand," its possible courses of action anticipated and inhibited. Though decomposing, the crowd necessitated a few symbolic arrests, the larger strategy involved simply "cooling off" the participants. Knowing the crowd to be in hand, the police thus stood firmly about it, absorbing minor acts of aggression, waiting for the mood to dissolve and for its participants to retire for the night.

Ten blocks from the point of its inception, the movement of the crowd of teen-agers had fitfully ended. The noise that had enveloped it all along reached a higher level. The chanting continued and then abated, supplanted now by a focus half inside itself and half still externalized toward the barrier ahead of it. A few voices toward the forward edge of a pod that extended into a side street unsuccessfully tried to redefine the path of movement, and yet others, to whom the majority refused to listen, insisted that the barrier should be confronted. Immobility threatened the mood maintained until then, brought on still further attempts to redefine the situation, and resulted in the ambivalent equilibrium foreshadowing the end of the encounter. Activeness gave way to an overwhelming passivity, to an excited form of collective introspection that sought expressive symbols of resistance to the authorities. For a few minutes then—and again the next day, when another crowd formed at Ninth Street—the subdued rioters enacted minor aggression against the varieties of authority then confronting them (local politicians, local police, state police officers and patrolmen, and a few unidentified elders in the entourage of the town's mayor). Some rioters shouted curses and standard insults and screams; others audibly degraded the competence of the mayor and struck at the toadying of those about him; and a few destroyed minor bits of property, where unnoticed, and defied ordinances against the wearing (and non-wearing) of certain kinds of apparel. A few at the front of the crowd bowed to and "mooned"[8] the fire chief, mayor, and chief of local police. But almost as if consciously attempting to maintain a protective control over their members, the majority of the participants, suddenly having become more aware of their vulnerability or at least that the police were beginning to show signs of meaning business, began again to mill about, to shuffle uneasily amidst their peers. Toward those whose excitement seemed to have transported them to near frenzy were directed calming remarks, muffled suggestions that they, among all the rest, because of their extraordinary loudness or whatever, seemed to have attracted special attention from the observing police cadre. As the authorities had gambled, the crowd seemed to be cooling itself off, reasserting its own rationality, spontaneously decomposing.

Shortly thereafter, small groups began to break away, retreating slowly back into the city to join the others lost along the way in the march toward the city hall. As the crowd dwindled, the police moved forward to coax the others along, making arrests here and there among the most resistant stragglers. Where just moments earlier an enthusiastic curse would have been submerged in the anonymous barrage, it now exposed its source as the last "warrior" in the streets. A few small groups lingered about even as the last arrest was made,

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with respect to unconquered (uncontrolled) territory.

[8] The verb "moon," as it is used by teen-agers and some college students, refers to the act (generally confined to males) of "dropping trow" (dropping one's trousers) so as to expose the nude hindquarters contemptuously to unexpecting onlookers, usually adults or prissy peers. Its origins seem to be in archaic ceremonies which involved ritual defecation upon figures of authority. In the context of the riot, it preserved this gestive significance.

disappearing with the rest only as it became clear that they, too, however innocently curious they wanted to appear, were no less immune from the jurisdiction of the curfew than those already marked for a night in jail. In a few moments the streets were again quiet; the extra police patrols during the night proved to be a superfluous precaution.

Once having happened, having been discussed and partially evaluated, the riot seemed to gather significance. Almost as soon as it was over, it had a name: the "Labor Day Riot." And sometimes, though most people seemed to leave it implicit, the qualification "teen-age" was inserted, as if to color the images provoked by the name with associations connected to popular stereotypes of adolescence—itself an implicit way of excusing the riot, rationalizing it, making it seem less than a threat to public order and more one of its expected concomitants. Even during the day after the first riot, when another teen-age crowd came up from the adjacent beach and assembled in the afternoon at Ninth Street (this time to voice displeasure at the arrest of one of their members who defied a city ordinance requiring the wearing of a shirt by a person walking on the Boardwalk), a transformation in the nature of their crowd activity became apparent. The police, who had foreseen the possibility of another outburst during the weekend, deployed a strengthened contingent of men in the area where the teenagers generally congregated. Without threatening the crowd, they cordoned off the area as inconspicuously as possible, readying themselves should another march begin, but refusing to stimulate or provoke the crowd by providing it with too great an appearance of their concern with the situation. Surrounded by the array of police, aware of them, and yet disinterested in anything but their control, the teen-agers stood their own ground. Again, but this time more cautiously, they used the apparent anonymity provided by the crowd situation to shout insults and to express contempt for the local authorities. The crowd situation, far from again being a medium of active aggression and mobile rebellion, nevertheless generated a high degree of excitement and involvement. Rather than involving thoughtless movement and activity, however, it seemed to have become a predominantly expressive crowd, its members caught up in a mood that seemed to embody their discontent, collectively symbolizing what they now acknowledged as rebellion against the authority system. And again, as on the night before, the crowd slowly began to cool off. Battered by little more than minor verbal abuse, the police again arrested a few of the more apparent leaders and then removed all but a token patrol from the area. As a number of the participants phrased it, this last day's events "capped off" the season. By degrees, they slowly gathered up their friends and belongings and headed out of the resort, back to their homes and to another year of school.

The reaction to the teen-agers' riot was curiously mixed. In the metropolitan newspapers there was the expected factual reportage, but in addition there appeared a number of quick editorial reactions and feature stories. Somehow, what had happened was felt to involve a kind of sympathetic "human interest" angle; the riot was not simply another riot, another disturbance in some closed population or asylum. It involved youth, and that made a difference. There was no condoning what had happened; public order had been disrupted, and that was deplorable. But the reasons for the riots troubled people. The crowd was not a drunken mob, its participants not juvenile delinquents.

Instead of blaming the teen-agers, the papers in fact displaced the responsibility to parents and to local officials in the resort, reserving praise for the state police for having handled the situation so efficiently. The two principal groups of participants, the teen-agers and the police, got off with sympathetic slaps, on the wrist and on the

back, respectively, in the end making both feel rather good about the whole thing, and with the public, in its own turn, left to ponder the situation vicariously.

However ill-defined and ambivalently responded to, the affair was not quickly forgotten, either by the teen-agers and state police, or the public, the media, or the local officials in Ocean City. Because of its location, its participants, its timing, its general temper, the riot retained an element of fascination for all. The "Labor Day Riot" stood out symbolically in the consciences of the various groups responding to it, as if awareness of it satisfied expressive needs originating in sources common to all. The lasting response was not loud and clamorously persistent among any of the groups; yet, in a muted way, its strength perhaps growing out of the simple ambiguity of information about the facts, the response became infused with a sort of populism. Among the participants and the public, at least, the riot seemed to appear as an exemplary prototype of small-scale rebelliousness against the recalcitrance of local authorities.

The riot in fact had sufficiently powerful consequences and seemed to have arisen from such sufficiently intractable sources that another one was expected to occur on the following Labor Day. The police expected it to recur; the local officials in Ocean City expected it; the press expected it (in fact several metropolitan newspapers sent reporters and photographers); and, most significantly, the teen-agers, beyond expecting it, hoped that it would recur. In fact, another riot, or near-riot, did occur at the end of the next summer; and another one happened the year after that. For the following five summers, in fact, crowds of teen-agers congregated in Ocean City on Labor Day eve. The crowd situation had become periodic and regularized. The state police routinely deployed riot control squads in the city during each Labor Day weekend. And each year the press sent reporters, photographers, and feature writers to cover the events. They were treated annually to much the same spectacle: A crowd would form at Ninth Street; the police would instantly surround and capture it; a few arrests would be made, and the police, in the process of cooling off the crowd, would get "dumped-on" to the delight of onlookers and retreating participants. The newspapers would feature the story the next day and grind subsequent features out of it for weeks after that. The police would get a slap on the back, and for the following year the teen-agers would talk excitedly about what had happened.

III

Analysis.—Proportionally, of course, only a small number of teen-agers, compared with the total size of the larger adolescent society, were active participants in any one of the Labor Day Crowds. But Ocean City, the town itself, was a place to which the vast majority looked for the kinds of outlets valued in teen-age culture. Over the years preceding the first riot, it had acquired a reputation for indulging their activities; and it had only been during the preceding two summers, after the election of a new mayor, that greater and greater numbers of constraints on their freedom in the resort had been put into operation, their "indulgency pattern" abrogated. The occurrence of the first riot triggered a response, therefore, for which the basis had earlier been laid down in a vague culture of discontent. Most of the youth seemed to feel that what had happened was not only proper, in the sense that the precipitating event (the arrest) justified the riot, but that the riot was one way of making the resort officials miserable for having dealt with the teen-agers so objectionably. The town, they felt, deserved to be disrupted by the riot. It seemed a great way to "cap off" their summer. And besides that, so it was argued, teen-agers had legitimate grievances; some argued, for instance, that the curfew declared by the mayor was

unconstitutional, a clear violation of their citizens' rights. And others alleged police brutality from the local cops. The riot seemed to right the wrongs apparently so unjustly pushed upon them; it was a matter of fun and justice.

Though perhaps the principled response was to have been expected on the basis of recent changes in the resort itself, it involved more than simple pleasure in getting back at the local authorities. The disposition to react favorably to the first and subsequent riots also had sources in several other quarters, among them being a tendency to value occasions that provided "thrills." Engaging in disorderly crowds and illegal sorts of activities were prominent types. The teen-agers were restless, bored by the activities open to them in the resort. The riot was their own special creation. It certainly departed from the humdrum temper of the usual things to do and places to go.

To the teen-agers, part of the importance of going to Ocean City was that it put them "on their own," away from parents and familiar authorities, with the chance to exercise their growing sense of autonomy. In the absence of parental controls, one expression of their youthful quest for independence was the desire to incorporate the spirit of rebellion into the activities which they pursued. It is quite clear, for example, that Ocean City, by any objective yardstick, would appear to be a rather deadly dull sort of resort; from the points of view of the teen-agers, about the only things to its credit were the beach, the absence of parents, and the presence of a large number of peers, some of whom looked old enough to buy beer.

Yet they came to the ocean with exaggerated expectations of fun. Confronted with a rather restricted structure of opportunities, the desire to milk every drop of fun from the available possibilties made it imperative to take a few risks, for example, being caught drinking, swimming at night, or "necking" on the beach. In other words, each of the things that seemed harmless enough in itself as a type of fun, had to be taken underground. The result, one might argue, seemed to be the development of a tendency within their peer culture to provide what they called "thrills" with a sort of valued functional autonomy. Rebellion for the sake of excitement, at first implicit, became a reward in itself, a thrill that augmented the cruder creature pleasures or intrinsic fun inhering in the acts involved.

The riot intruded into this atmosphere as a super-thrill, and the response to it made out of it a compelling symbol of the short-run orientation to risk taking—and through that, to anti-authoritarianism—that surrounded the significance of Ocean City in the teen-age culture. It was a perfect inflection of the short-run orientation of teenage culture, an orientation exaggerated in Ocean City, yet also pervasively rooted in more routine contexts because of the legal status of the teen-age pursuit of fun. Subsequent to the first riot, whenever a teen-ager talked about Ocean City, conjuring up memories associated with the place, the most arresting image available was that provided by the riot itself.

The association of the riot imagery with that of Ocean City shared another connection through the dynamics of status within the adolescent society. For example, it became a mark of some status to associate oneself with the riots whenever summer activities were dug up for review. In particular, in the course of our interviews with participants, they frequently remarked that those most preoccupied with the significance of the riots were younger members of various peer groups. Since considerable attention came to be focused on the riots, those who were maneuvering to gain the acceptance of their peers, especially these younger teen-agers, in associating themselves with attention-gathering events, tended to exaggerate their importance and to maintain them as focuses for conversation. In this way the riots did gain significance; they became attention-gathering

events which were looked forward to as an exciting way to end the summer with a climax that had status-conferring potential.

The connection of participation—and, in particular, the mode of participation—in the riot and the status system of the adolescent society was important in another respect, too. A peculiar fact about the composition of the crowds that rioted was that they had emerged from a pre-existing milling process set up quite normally at Ninth Street among naturally formed cliques. The teen-agers, in migrating to Ocean City, had come predominantly in small groups of friends, a large proportion in fact with their high school fraternities and sororities. They had, in other words, transported their status systems with them, and in the city they tended to carry out the majority of their activities within these peer groups. In consequence, pre-existing status differentiations were a part of the group process within which the riotous moods had emerged and been cultivated. Leaders who exemplified the status qualities rewarded in the routine functioning of their peer groups, confronted with the riot situation, did not instantly cease to be aware of their status or to be looked up to. The riots, in fact, by providing a forum for challenges to their ascendancy from spontaneous leaders, provoked status threats.[6] It became clear during the interviews that many of the "established elites" of adolescent society felt that they themselves had to co-opt the functions of spontaneous leaders in the riot situation, to demonstrate that their worthiness of deference did not break down under pressure, that they had leadership skills in aggressive crowd situations as well as in stable peer competitions. The interesting result was that spontaneous leaders, and those fomenting the mood of the riots, as well as those who set up precipitating routines to generate the subsequent crowd situations and rumor processes, were not really so spontaneous as might be expected. In effect, a large number of them were high status teen-agers who, recognizing threats to their status in the crowd dynamics, had adopted leadership roles for the larger crowd. They became the ritual leaders among their peers, augmenting the security of their own status by helping to precipitate the outburst of later summers, outbursts in which they were experienced participants.

The question of the amount of "turnover" in participation in the yearly Labor Day crowds also has an interesting bearing on factors contributing to the events. Quite clearly, there was both considerable turnover in the crowd membership and yet also a significant number of hard-core repeaters, with the leaders and sociometric stars of peer culture being especially visible. The rate of turnover seemed, in fact, to give each annual crowd an interesting balance between enthusiastically curious younger newcomers and the older repeaters. The balance seemed to contribute to the fact that each year the crowd situation could maintain a high degree of fascination for those assembling to participate in it. Over all, the strength of the riot imagery was doubtlessly tied to the fact that it was an age-graded phenomenon, with new cohorts of teen-agers continuously entering into the peer group milieu.

In the years that followed the first riot, the crowds that formed at Ninth Street on Labor Day even changed somewhat, however. In the first place, everyone came expecting to get another riot going and also holding a somewhat more realistic cognitive map of the likely consequences of the crowd involvement. Also, the police never really allowed another full-fledged riot to get underway. Interestingly, although they would allow the crowd to form and allow its moods to grow somewhat excited, they would maintain it in captivity. And sometimes, recognizing a repeater or trouble maker, they

[6] Compare the description of status-threat dynamics given by James F. Short and Fred Strodtbeck, *Group Process and Gang Delinquency* (Chicago: University of Chicago Press, 1965), chap. viii.

would call him aside and attempt to mediate their hopes for order to the crowd. The repeaters, the ritual leaders singled out by the police, had become genuine intermediaries with the authorities.

Each annual crowd had become, in addition, a cultured crowd, expressing its grievances against local authorities. Participation was fun; it was exciting and thrilling. But it was also principled. Any of the teenagers could rationalize the significance of the crowd formation in terms of higher values and belief systems. They talked of constitutional rights, of the injustice of treatment accorded to teen-agers; they placed their action within the context of larger, sometimes philosophical issues like freedom. And, most interestingly, they invoked a kind of creolized sociology to explain the social sources of their discontent. There were few teen-agers who could not, either by themselves or through a buddy, speak in glittering generalities provided by such catch phrases as "rebellion against authority." Most of their principles of explanation on the sociological level, naturally, were self-fulfilling ones. In the end, their interpretation both explained what was going on while justifying it and simultaneously named what had happened so as to program it, to re-induce it.

IV

Conventionalization.—The significance of these crowd actions filters through three rather gross perspectives, namely, those of the police, the teen-agers, and the larger critical public. It is partly through understanding the convergence of these three outlooks, their mutual influence and compromise, and their meeting in a commonly shared higher outlook, that the partial formalization, the patterned repetition and conventionalization of this collective behavior can be explained. The following remarks attempt to show how the three outlooks tend to interact in patterned ways, permitting a sort of de facto conventionalization that produces crowd behavior resembling ceremonial.

Police action in most contexts—but vividly so during encounters with such disorderly publics—is conditioned by a constabulary ethos, and in particular by an operational rule that might be called the "principle of minimum violence." In a large proportion of cases, for instance, the police do not feel justified or compelled to gun down participants in crowd actions; rather, they seek to "capture" them—to get them back "in hand"—and then to prod such assemblages into a process of decomposition. Principles of minimum violence for crowd control minimize not only injury but also the ordinary feedback of public outrage arising from allegations of illegitimate brutality or coercion. Such a standard set of control strategies also imparts a regularity to the process of crowd decomposition. Viewed more holistically, the result is an interchange[7] with latently *ritual* properties (i.e., a sequence of actions patterned to return a disrupted social equilibrium to its stable condition).

The police casually call this phase in crowd control "cooling off." In effect, it functions as a special sort of communication structure. Using the example of the beach riots, one can roughly conceptualize the notion in terms of a set of interdependent multiple exchanges. In the encounter between the crowd and the police, crowd members put up expressive resistance and call insults to the police, who in turn accept them in exchange for the minimally coercive re-establishment of public order. Between the police and the public audience, a sort of professionalized degradation tolerance is exchanged with audiences for augmented informal authority and legiti-

[7] The notion of "interchange" in the sense used here is developed by Erving Goffman, who uses the concept to denote the sequence of acts precipitated by threats to "face" and terminating with the return of the encounter to a condition of ritual equilibrium; see, "On Face-Work: An Analysis of Ritual Elements in Social Interaction," *Psychiatry*, XVIII (August, 1955), 213–31.

macy. And between the public and the crowd, the interchange passes a sort of sympathetic indulgency for a spectacle that functions indirectly as a public drama. In the loose analogy, the rates of exchange in these transactions are biased in favor of the most disinterested parties, namely, the police in the crowd-police interchange, the public in the police-public interchange, and the crowd in their interchange with the public.

One can phrase a proposition about the conventionalization of this threefold interchange matrix by holding the bias of two interchanges relatively constant and allowing the third to vary. For purposes of exposition, we shall assume that the interaction of crowds and police cadres follows a fairly standard pattern (as argued above) and that the interaction of police organizations and critical publics also takes place in a relatively fixed pattern (also a reasonable assumption for our purposes, given the fairly constant quantum of police anxiety to public criticism). The third variable interchange, that between critical publics and disorderly crowds, is less constant. The bias favors crowd members only when public sympathy is on their side, something that does not always happen, for example, when crowd members are an ethnic minority, a revolutionary movement, etc. This conceptualization produces several interesting consequences. In unsympathetic cases, such as in the case of the minority riot, the antipathy of the public to disorderly crowds, or its indifference to them (for example, when they are not visible or are selectively unperceived), renders the attitude of the audience to the use of public authority congruent with that of the police. The crowd confronts a monolith of opposition, with police and public being perceived similarly, the police acting in behalf of the public, representing it, and even molding part of the public response. On the other hand, when public sympathy resides with a crowd action, the opposite pattern of influence operates. The crowd and the public attitudes to the police are balanced, and the crowd is then acting in a way representative of the public, expressive of public values. In this case, the crowd-police interchange becomes a kind of symbolic drama, with the public audience participating vicariously in the action of the crowd, its own reactions to authority in this case being influenced by the posture of the restless public.

Propositions about the likely course of development of the expression of crowd unrest follow from these notions. When the crowd action provokes a public reaction, that reaction is likely to have an *evaluative* quality; the crowd action is thought to be more or less understandable, reasonable, good or bad. When public reaction is indifference, the *moral* issues implicit in the crowd-police interchange fail to become explicit. When the reaction is antipathetic, the expression of collective unrest confronts a hostile audience and is likely (if it does not simply die out) to be taken underground, to become the basis for transformations of collective energies into formal private developments, such as sectarian or revolutionary groups. When it is sympathetic, a public development, something like a conventionalized or a ceremonialized interchange, is more likely to occur. The general proposition, therefore, is that a process of conventionalization is most probable when (*a*) there is a strong positive public reaction to the expression of unrest in captured crowd behavior, and (*b*) the unrest underlying the collective action has very generalized sources (i.e., is constantly replaced, as in the continuously present quantum of unrest among adolescents).

A ritual interchange which dramatizes the expression of unrest and gains a sympathetic climate of public reaction sets the stage for transformations in subsequent interchanges. How this is accomplished is illustrated in part by the case of the beach riots. Active crowd aggression was controlled by the police cadre by capturing

the crowd and allowing it to "cool off." The process of cooling off, latently shaped by the permissive police tactics, provoked a predictable transformation in crowd action. The "cooling off" phase of crowd captivity left open an avenue in the development of crowd life that permitted a more sublimated order of collective behavior. The transformations parallel those accompanying the shift from an active to an expressive mood, in this case the decentralization of the collectively aggressive mood into one decreasingly sustained by expressive individual acts thrown up in opposition to control. Police, in managing the cooling off, tolerate abusive demonstrations of contempt for authority by crowd members, viewing such acts as necessary concomitants of their ultimate goal of restoring order. For the crowd participants, such acts are not rationally goal-directed; they are somewhat capriciously constructed, bizarre and extravagant devices of rationalizing impunity.

Just as the cooling off phase allows crowd members to disinvolve themselves, to take an objective view of the crowd mood and the impending fate of the encounter, and then to reassert the rationality of departure, so it spontaneously provokes "evaluative" reactions among them which objectify the essentially dramatic-symbolic dimensions of the encounter. The central dimension, a sort of ordered anarchy or bounded excess, involves reference to precisely the pattern of values from which such interchanges with authorities as that embodied in the cooling off could have been deduced; and from these values, the acts of the crowd members acquire their own belated rationale. Cooling off permits the rationalization of impunity in acts that go unsanctioned; it thereby enables a rebellious mood to express itself in minor, ritually rebellious acts.[8] Occurring inside the ritual interchange, rebellious expressions remain controlled. But the acts themselves, nevertheless, stand in stark contrast to routine, accentuating its ordered properties and the values it embodies, thereby centralizing those values into the public conscience. The interchange becomes a public stage on which the drama of legitimately ordering central values is enacted.

On the part of crowd participants, a latent consequence of expressing unrest inside the ritual boundary is the *naming* of that unrest: individual acts are focused against a structure of control which absorbs them meaningfully, and the interchange becomes a vehicle for the expression of very generalized rebellion. The individual acts of crowd participants come to *name* the dispositions and moods and, hence, to acquire the symbolic capacity to re-induce these moods and dispositions, out of which the more excited collection behavior was built. Once having been so infused with meaning —on the one hand, objectively externalizing formerly ill-defined restlessness and, on the other, directing it onto a target that imposes definition upon it—such expressions gradually become understandable. The ritual action shapes the symbolic development and in the process becomes a drive-object itself.

Not only does the ritual action gratify its participants, but it also serves functions for the agents of control and the audience. Three outlooks converge in a balanced perspective of expectations. Authorities augment their informal status, their legitimacy, by tolerating innocuous derogating gestures from retreating crowd members. Subjecting themselves to degradation for the more urgent purpose of restoring control permits them to maintain an image of the public performance of their functions that is connected with the notion of their approachability, their discretionary flexibility

[8] This formulation derives from Max Gluckman, "Les rites de passage," in Max Gluckman (ed.), *Essays on the Ritual of Social Relations* (Manchester: Manchester University Press, 1962), pp. 1–52. Also, V. M. Turner, "Symbols in Ndembu Ritual," in Max Gluckman (ed.), *Closed Systems and Open Minds* (Chicago: Aldine Publishing Co., 1964), pp. 20–51. We have also adopted the notion, developed by Gluckman, of "rituals of rebellion," in conceptualizing our observations; see his *Order and Rebellion in Tribal Africa* (New York: Free Press, 1963), pp. 1–49, 110–36.

in tempering authority. The public reward of their indulgence of symbolic attacks also serves to crystallize the manner in which they will handle similar interchanges in the future; they become disposed to permit the conventionalized recurrence of similarly rewarded degradations. And the public, sympathetic with the control of it in tempered encounters, likewise becomes disposed to assimilate its conventionalized recurrence.

The case of the beach riots is an illustration of the convergence of these general factors. The annual reconstitution of the interchanges is similar to the conventionalized ceremonial tone of more formalized collective symbolic dramas. We have used the term "ceremonialization" to denote the elevation into *public* awareness of the symbolic dimensions of the ritual interchanges and thereby the explicit definition of such otherwise latent interchanges as structures of action capable of reifying the legitimate ordering of important collective values. Processes of ceremonialization, like the one expressed by the crowd of teen-agers, depend upon deeply rooted restlessness that externalizes itself initially in sacrilegious or anti-authoritarian collective disorder; the process of "cooling off" inside an interchange with agents of control infuses collective action with a condensed structure of significance, much like a metaphor. This significance is only partly a product of the compulsion to express discontent. It arises gradually through stratifying meaning and emotion in an intense collective action which itself becomes symbolic of the functional ordering, rationalization, and maintenance of central values. The ceremonialization of such interchanges, in other words, is the process of intermittently publicizing a framework of meaning that is composed of beliefs and issues and conceptions which, except through the intensity of such inherently symbolic and deeply motivated acts as those in rituals of rebellion, are otherwise rarely subjected to meaningful consideration in routinized everyday contexts. For participants in a crowd, the larger issues may be quite different from those initially raised for police or for wider audiences; but for all persons made aware of the enduring sources of these recurrent interchanges, some principles of general explanation are invoked, some discontinuously explicit beliefs and values recentralized into the public conscience.

V

Conclusions.—Hopefully, this analysis will serve to qualify the mortal image of collective behavior prevalent among sociologists. Our imagery of most crowd behavior ends when the police arrive, for it is then that crowds are made more securely moribund. Yet it is then, too, as we have attempted to suggest, that some of the more important developments of crowd action take place.

The analysis of the adolescent crowds and of their control by the police has led us to suggest, in a very approximate way, some of the processes involved in crowd conventionalization, but there is a clear need for further research in this area. The social control of crowd action, the management of collective unrest, is an ordinary occurrence in many stable organizations; schools, churches, and asylums, for example, are notoriously inhospitable to mass action; yet, as complex organizations, they frequently set up controlled environments in which the management of sponsored crowds is made possible by transforming them, if only temporarily, into audiences, repenters, electorates, and so on. Crowds are gullible things, baited and then captured for numerous special purposes, often as sources of support for many of the most central social institutions. Clues to further understanding the dynamics of such crowd situations, we believe, are to be uncovered in explorations of symbolic dimensions imparted to crowd action by institutional controls.

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Anomie: Comparisons between the Old and New Middle Class[1]

Joel I. Nelson

ABSTRACT

Research is reported on the relation between anomie and membership in the old and the new middle class. The old and the new middle class are defined in terms of two dimensions: (1) access to large-scale industrial bureaucracies—a factor relevant to mass-society theory—and (2) ownership as opposed to management of capital—a factor relevant to a more traditional class-oriented, economic theory. The data are generally more consistent with an economic viewpoint than a mass-society viewpoint: at low and moderate income levels owners tend to be more anomic than managers; bureaucratic affiliations are not, however, related to anomie. An attempt is made to trace the differences in anomie between owners and managers to varying mobility commitments. Owners tend to be less mobility oriented than managers. When commitments to mobility are controlled, the differences on anomie between the two groups attenuate to a point where they are no longer statistically significant. This result is discussed within a more general theoretical perspective.

The distinction between businessmen of the old and the new middle class is a point of departure for a variety of contemporary discussions of alienation. Whether the works considered are as diverse in theoretical posture as C. Wright Mills's *White Collar*[2] or Kornhauser's analysis of marginality and mass behavior,[3] the conclusions are similar and clear: the old middle class, in particular the small businessman, is viewed as a vestige of a former stage of industrialization. As a reaction to its anachronistic position, the old middle class develops a deep-rooted sense of alienation. Alienated, it is susceptible to the Fascist appeal.

This paper reports research on one aspect in this hypothesized chain: the relationship of a particular variant of alienation—anomie—to membership in the old and the new middle class. An attempt will be made to identify the roots of alienation in terms of the relationship of persons to (1) bureaucratic power and (2) capital and to fit the findings into a broader context of aspirations and alienation.

THE OLD AND THE NEW MIDDLE CLASS

A frequent characterization of the plight of the old middle class is summarized in Kornhauser's *The Politics of Mass Society*. Kornhauser notes:

> Small business . . . increasingly is marginal in modern society and as a result has been more susceptible to mass movements . . . squeezed between the pressures of big business and big labor, the class interests of small business are inherently ambiguous, finding allies neither in the classes above nor in the classes below. The central difficulty facing small business is the relative absence of realistic possibilities for improving its long run economic position in a world increasingly dominated by large-scale organization.[4]

The central source of alienation is identified by Kornhauser as inadequate access to bureaucratic organization and power. This is in keeping with his general contention that traditional indicators suggested by classical economic perspectives—income or occupational prestige—are not as useful

[1] Funds for conducting this study were provided by the Agricultural Experiment Station, University of Minnesota (Project No. 2716H). Paper No. 6499, Scientific Journal Series, Agricultural Experiment Station, University of Minnesota.

[2] C. Wright Mills, *White Collar* (New York: Oxford University Press, 1956), pp. 3–59.

[3] William Kornhauser, *The Politics of Mass Society* (New York: Free Press, 1959), pp. 194–211.

[4] *Ibid.*, p. 202.

for understanding alienation in a mass society. What is really at issue is the individual's integration into the agencies exercising power and control—in this instance, the large-scale industrial bureaucracy.

This particular variant of mass-society theory has received empirical confirmation in Trow's research on support for McCarthyism in Bennington, Vermont.[5] Trow's work documents the small businessman's overwhelmingly positive views toward McCarthy in comparison with the lesser support given by white-collar and blue-collar workers. Educational differences among the three aggregates do not affect the thrust of this comparison. The study also illustrates that small-business support for McCarthy is greatest among persons with traditional nineteenth-century suspicions of big labor and big business.

These findings have been interpreted by Kornhauser and Trow as consonant with the mass-society perspective that access to bureaucratic organization is the precipitating circumstance differentiating the economic potential of the old and the new middle class. Yet, on closer examination, it is not at all clear whether the arguments are in keeping with an orthodox mass-society viewpoint.[6] In effect, there may be at least two critical factors distinguishing the old and the new middle class: (1) access to the large-scale industrial bureaucracy—a conception relevant to mass-society theory —and (2) the ownership as opposed to the management of capital—a conception relevant to more traditional class-oriented, economic theory. In each of the arguments presented above, as well as in Mills's treatise and a recent study by Bonjean,[7] there is a singular failure to systematically delineate the two factors.

BUREAUCRATIC ORGANIZATION AND CAPITAL: SOME CONSIDERATIONS

Table 1 presents the logical cross-tabulation of these factors. Comparisons have generally been drawn between cells 1 and 4—thus capturing a relevant historical transition at the cost of blurring two critical dimensions. If the hypothesized development of alienation in the "old middle class" is, however, to be more fully understood, it is necessary to weigh separately the relevance of both mass and class considerations.

TABLE 1

ANALYTICAL DIMENSIONS OF THE OLD AND THE NEW MIDDLE CLASS

Size of Economic Organization	Relations to Capital	
	Owners	Managers
Small enterprise.	1	2
Bureaucratic organization	3	4

A number of hypotheses regarding membership in the old and the new middle class and alienation can be derived from the schema presented in Table 1. First, the lack of an affiliation with large-scale bureaucratic organizations can result as the major condition contributing to alienation, regardless of an individual's relation to capital. A finding of this sort would qualify a number of existing interpretations by illustrating that designations of owners and managers are essentially deceptive; what is at stake, as an orthodox mass-society theorist would hold, is access to bureaucratic organization rather than the ownership or management of capital. Alternatively, dif-

[5] Martin Trow, "Small Businessmen, Political Tolerance, and Support for McCarthy," *American Journal of Sociology*, LXIV (November, 1958), 270–81.

[6] Gusfield has made this general observation in connection with a review of mass-society literature: see Joseph Gusfield, "Mass Society and Extremist Politics," *American Sociological Review*, XXVII (February, 1962), 19–30.

[7] Charles M. Bonjean, "Mass, Class, and the Industrial Community: A Comparative Analysis of Managers, Businessmen, and Workers," *American Journal of Sociology*, LXXII (September, 1966), 149–62.

ferences in alienation might occur among owners and managers regardless of bureaucratic affiliations. If sustained, this finding would direct attention to the risks inherent in capital investment as a source for alienation in the old middle class as well as place the differences in alienation between the old and the new middle class more squarely in line with traditional economic theories of class position. Finally, both the ownership of capital and the lack of an affiliation with bureaucratic organizations might each contribute to alienation, thus differentially distributing levels of alienation across each of the four cells outlined in Table 1. This finding would support many typical characterizations of alienation in the old and the new middle class and more carefully specify its sources as well as its intermediate levels in the middle combinations illustrated in cells 2 and 3. In addition, the finding would be consonant with both mass and class consideration by illustrating that risk capitalism in a non-bureaucratic setting provides the weakest buttress against alienation.

The data to be reported in this paper entail comparisons on one variant of alienation—anomie—among the circumstances described in Table 1 in cells 1, 2, and 4; a tentative and not altogether satisfactory approximation of cell 3 will also be offered. The research should be viewed as a step toward refining the distinctions between the old and the new middle class—particularly the differential implications for alienation of (1) the ownership and management of capital and (2) access to large-scale bureaucratic organizations.

RESEARCH PROCEDURES

Sample.—The data for this paper are from a survey of the reaction of businessmen to economic trends in twenty-eight Minnesota communities.[8] The communities ranged in size from rural townships of under 1,000 persons to a metropolitan area of over 1,000,000. Approximately half the respondents were from communities of more than 40,000 people.

In each community, various business enterprises—grocery stores and supermarkets, pharmacies, jewelry stores, gasoline stations, and the like—were sampled. In smaller communities, every such business was selected; in larger communities, businesses were selected by means of area sampling.[9] Seven hundred and four persons—either the owner or the manager of a particular store—were interviewed.[10]

Classification: the old and the new middle class.—Owner-manager status was determined by responses to a question on whether the head of the store owned the business or managed it for someone else. Twenty-three per cent of the sample were classified as managers and 77 per cent as owners.

The managerial sample was further delineated according to the particular store's affiliation with a large-scale bureaucratic enterprise. The distribution of these affiliations was bimodal. At one end were managers of independent stores (cell 2, Table 1). The extent of bureaucratization was minimal—few if any persons were supervised, and the manager was responsible only to an absentee owner. On the other end were managers in moderate- and large-sized bureaucratic organizations (cell 4, Table 1).

[8] The study was directed for the Agricultural Experiment Station of the University of Minnesota by John Photiadis, University of West Virginia. In keeping with the original intent of the study, communities were stratified according to size and randomly sampled from population growth and decline areas in the state.

[9] In towns with fewer than 5,000 persons every business was selected; in larger towns every second or third business was selected. In urban areas, with more than 40,000 persons, sampling areas were designated with reference to (*a*) downtown districts, (*b*) main business thoroughfares, (*c*) residential neighborhoods, and (*d*) shopping plazas. Depending on the size of the community, every second or every third business was sampled from each designated area.

[10] Less than 10 per cent of the originally designated sample refused to be interviewed.

The smallest of these organizations was composed of twenty stores scattered throughout the state of Minnesota; the majority, however, were affiliated with large-scale distributors, for example, supermarket chains—Kroger stores and Red Owl—or the giant retail-merchandise outlets—J. C. Penney, Montgomery Ward, Woolworth, and Sears, Roebuck.

A parallel distinction regarding bureaucratic affiliations was attempted for the owners. Independent businessmen were classified as owners within a non-bureaucratic work setting (cell 1, Table 1). These enterprises had no working affiliations with other retail stores; their mode of operation tended to be of the "mom-and-pop store" variety. Owners with bureaucratic affiliations (cell 3, Table 1) were more difficult to classify. Ideally, the classification should contain the increasingly rare owner of a large-scale corporation. The approximation in the present research entailed placing owners in this category if they worked under a corporate franchise. Automobile dealers, nationally and regionally franchised gasoline stations, and drugstores were examples of the enterprises classified as such. Although the particular work settings were contained in at most a moderately sized store, a nexus with the large-scale bureaucracy was nonetheless present: to varying degrees there existed controls on investment, some use of the capital of the parent organization, training programs, regionally and nationally advertised products, as well as periodic exposure in the mass media. The affiliation also afforded some of the economic practices that accrue to large-scale organization—systematic price-fixing, reduced wholesale costs, and other mechanisms to buttress the owner's risk in a continually fluctuating market. While such connections by no means squarely place these respondents in a large-scale bureaucratic context, they at least provide a degree of affiliation not present among independent entrepreneurs.[11]

Alienation.—The dependent variable in this research, alienation, was measured with a battery of five items contained in Srole's anomie scale.[12] Each item was posed to the respondent with six alternative responses ranging from "strongly agree" to "strongly disagree." All responses were scored from 1 to 6 and the scores for each item simply cumulated into a final index. It should be emphasized that anomie is only one variant of alienation; the findings reported in this paper should not, therefore, be generalized beyond this particular empirical referent.

RESEARCH FINDINGS

Anomie in the old and the new middle class.—The hypothesis specifying pronounced anomie in the old middle class is only partially confirmed. Table 2 reports the mean anomie scores by relationships to capital and bureaucratic affiliations. The findings indicate that owners are uniformly more anomic than managers; bureaucratic affiliations, however, exert no effect on anomie. The differences between owners and managers (13.2 vs. 11.5) are significant at the .01 level ($t = 3.00$, two-tailed

[11] Unfortunately, neither Dun & Bradstreet nor the office of the Small Business Administration keeps statistics on the comparative rate of failure for businesses which do and do not operate under a franchise. The following statements offer some basis for believing that the standards, control, marketing procedures, and general advice offered by the franchise organization substantially reduce the economic risk of the franchisee in comparison with the completely independent small businessman: Edwin Lewis and Robert S. Hancock, *The Franchise System of Distribution* (Washington, D.C.: Small Business Administration, 1963), p. 91; J. A. H. Curry *et al.*, *Partners for Profit: A Study of Franchising* (New York: American Management Association, 1966), p. 94. For an account of the plight of the small businessman's attempt to establish a successful firm—untutored by any knowledge of sound business practices—see Kurt B. Mayer and Sidney Goldstein, *The First Two Years: Problems of Small Firm Growth and Survival* (Washington, D.C.: Small Business Administration, 1961).

[12] Leo Srole, "Social Integration and Certain Corollaries: An Exploratory Study," *American Sociological Review*, XXI (December, 1956), 709-16.

test). The findings thus provide little support for the mass-society view that exclusion from the large-scale corporate bureaucracy is an important source of alienation.

The data in effect suggest that results in other research of pronounced alienation in the old middle class may be more a function of the ownership of capital than of marginal connections to the corporate economy. If anything, the findings direct attention to a more economic interpretation

TABLE 2

ANOMIE MEANS BY RELATIONS TO CAPITAL AND SIZE OF ECONOMIC ORGANIZATION

Size of Economic Organization	Relations to Capital	
	Owners	Managers
Small enterprise......	13.3 (N=138)	11.4 (N=63)
Bureaucratic organization..............	13.0 (N=394)	11.5 (N=99)

than is usually emphasized by mass-society theorists. While there is no indication in this study that owners are faring less well than managers (the two categories have nearly identical levels of income), it does appear that anomie is differentially related to levels of economic attainment within each group. This is shown in Table 3, where anomie scores for owners and managers are shown at various levels of income.[13]

Contrary to many past reports, these data indicate that owners *are not* uniformly more anomic than managers. Table 3 illustrates that the higher anomie for the owners is due largely to the scores of the low-income group and, to a lesser extent, to the middle-income group. The similarity of scores for the high-income owners and managers suggests that ownership of capital per se is an insufficient condition for anomie; *apparently, anomie is a function of depressed economic circumstances and owner status.* Further, the owners' anomie scores show an orderly progression from the high- to the low-income levels, a progression simply not as apparent for the managers. While the product-moment correlation between income and anomie is negative for both groups, the coefficient for the owners is —.30 compared to —.06 for the managers. In terms of anomie, owners appear to be more *reactive* to their economic position than managers.

Reactiveness, mobility aspirations, and the development of anomie.—Reactiveness is a function of many things, among them

TABLE 3

ANOMIE MEANS BY INCOME AND OWNER-MANAGER STATUS

Income	Relations to Capital	
	Owners	Managers
$6,999 and less.......	15.3 (N=201)	12.6 (N=48)
$7,000–$9,999.......	12.6 (N=125)	11.4 (N=53)
$10,000 and above...	11.5 (N=199)	11.5 (N=47)
Total.............	13.2	11.8
No. of cases.......	525	148

the extent to which persons are constrained to remain fixed in a particular social niche. This perspective is well illustrated in Durkheim's analysis of altruistic suicide.[14] In his discussion, Durkheim suggests that this particular form of reactiveness is found in the tightly integrated community—a setting with severe constraints on geographical mobility. However, he makes clear that constraints on movement alone are not sufficient stimuli for suicide; crisis, for example the breach of a taboo, is an equally important precipitating condition. When both factors are coupled, suicide as a form

[13] The attenuation of cases in comparison with the data reported in Table 2 is due to the respondent's unwillingness to reveal his income.

[14] Émile Durkheim, *Suicide* (New York: Free Press, 1966), pp. 217–40.

of reactiveness is a probable result precisely because physical and psychological opportunities for leaving the crisis are nil.

Contemporary research has partially substantiated Durkheim's perspective on reactiveness as a function of crisis and limited opportunities for movement. For example, in *Automobile Workers and the American Dream*,[15] Chinoy notes that the automobile industry's poor pay and restrictive wage scale have little relevance for the morale of the very young, for those who believe advancement within the corporation possible. Even in the moderate-age group, where aspirations are shifted outside the organization to prospects of developing a small business, economic deprivation has little effect on morale. It is among the somewhat older workers, however, who see for the first time the realistically bleak prospects for movement, that economic deprivation results in reactiveness in the form of sagging morale, apathy, malaise—all indicators of a profile of social alienation.

The effects of the reverse side of the coin, economic deprivation and high commitments to mobility, have been documented by Wilensky and Edwards.[16] In their work on skidders, the downwardly mobile, they find that economic deprivation does not result in reacting against the social order for those with a firm commitment toward recouping their loss. The skidder, in brief, defines his position as temporary, thus blunting potentially hostile reactions in the face of occupational crisis.

It may be possible to account for the previously observed differences in anomie by suggesting that owners are more committed to their present position than managers. A number of considerations regarding the relative mobility aspirations for owners and managers can be entertained. First, economic survival for owners conceivably turns not only on the efficiency of their operation but on the establishment of extensive community relations. Credit, good will, faithful clientele, a friendly turn of a word are all part and parcel of a successful enterprise. Mobility could mean renouncing the very community ties on which economic survival is predicated. Managers may also feel it necessary to establish good will and successful working relations in a community; nonetheless, these are policy decisions of the store, decisions which may affect the store's success but not the manager's capital. Equally relevant is the fact that managers are mostly paid fixed wages, thus eliminating the high risks of capital investment demanded of owners in mobility considerations.

There may be some basis, then, for hypothesizing that owners, either by virtue of opportunity, or desire, or both, tend to be more committed than managers to their present occupational position. Unfortunately, no data were available on the actual mobility constraints of owners and managers; instead, differential commitments to mobility were tested with six items from Reissman's scale.[17] The items raised the question as to whether the respondent would leave the community, learn a new routine, etc., in order to make a substantial improvement in his position. Each response was placed in one of three precoded categories: (1) Wouldn't matter at all; (2) Would be a serious consideration but wouldn't stop me; and (3) Might stop me from taking a better job.

The findings in Table 4 are in sharp contrast to the popular conception of the

[15] Ely Chinoy, *Automobile Workers and the American Dream* (New York: Random House, 1965), pp. 110–22.

[16] Harold Wilensky and Hugh Edwards, "The Skidder: Ideological Adjustments of Downward Mobile Workers," *American Sociological Review*, XXIV (April, 1959), 215–31.

[17] Leonard Reissman, "Levels of Aspiration and Social Class," *American Sociological Review*, XVIII (April, 1953), 233–42. Two of the items showed no differences between owners and managers and were eliminated from the cumulated index; the items asked whether respondents would keep quiet about their "political views" and "religious views" to improve their business position.

spirited, individualistic businessman. On each item, the data show that owners tend to be more unwilling to alter existing relations than managers. Controls by age, education, or income do not affect these results. The differences between groups on the cumulated scale are statistically significant at the .001 level ($t = 6.40$, two-tailed test).

While the data do not reveal what is at the root of the owner's low commitments to mobility, they at least suggest that the

TABLE 4

MOBILITY COMMITMENTS BY OWNER-MANAGER STATUS

MOBILITY ITEMS	RELATIONS TO CAPITAL	
	Owners	Managers
Leave family.........	1.8	1.5
Move about country..	1 8	1.5
Leave community....	2.1	1.9
Leave friends........	2.3	1.9
Learn new routine....	2.7	2.5
Work harder.........	2.7	2.3
Total.............	13.4	11.6
No. of cases........	525	148

pronounced anomie among owners in poor economic straits may be a partial function of these commitments. The limited number of cases in the managerial group, as well as the fact that the mobility scores for owners and managers were too highly skewed in opposite directions, precluded the possibility of testing this proposition by partialing the data into various contingency tables.[18] As an alternative, standardization was selected as a means of controlling the effects of mobility commitments, thereby capitalizing on the entire distribution of managers.[19] This procedure more closely follows the logic of adjusting scores, that is, of weighting averages as in a partial correlation, than the logic of literally holding variance constant as in a complex contingency situation. The relevant question was: Would the owners' anomie scores more closely resemble the managers' if they were similarly distributed in their mobility commitments?

To standardize the data, the owners' and managers' anomie scores were computed at every level of mobility commitment. The owners' mean anomie score at each level of mobility was weighted according to the number of cases of managers at the identical level of commitment. This was done separately for each of the three income groups reported in Table 3. Weighted means were computed in the usual fashion. The results of the procedure are reported in Table 5.[20]

It can be seen in Table 5 that some of the differences between owners and managers on anomie still persist, particularly among those in the lowest income levels. The owners' higher anomie scores observed in Table 3, however, have now been sufficiently reduced to the point where the differences between the two groups are no longer statistically significant ($t = 1.43$, $P < .20$, two-tailed test). While all of the variance between owners and managers on anomie has obviously not been explained, at least some portion apparently can be accounted for by varying commitments to mobility.[21]

[18] The data in Table 4 do not provide full appreciation of these differences in the distribution of the mobility scores; however, if mobility scores were divided at the median for the low-income group, the mean mobility score would be 9.9 ($N = 112$) for the low-aspirant owners and 10.9 ($N = 17$) for the low-aspirant managers—a difference nearly as large as the one originally observed.

[19] Morris Rosenberg, "Test Factor Standardization as a Method of Interpretation," *Social Forces*, XLI (October, 1962), 53–61.

[20] The resulting scores should be interpreted as a hypothetical distribution representing the mean level of anomie that would occur at each income level if owners were distributed in mobility commitments in a manner identical with the distribution of managers. If the data were standardized the other way around, i.e., if the managers' scores were standardized on the owners' scores, the essential patterns and differences shown in Table 5 would remain substantially the same.

[21] It is necessary to emphasize that neither education nor age substantially alters these findings. In

DISCUSSION

The data, in brief, suggest that owners are more likely to be anomic than managers because of their differential commitments to social and geographical mobility. Regardless of the structural or selective factors at the root of these commitments, the emergent conception of opportunity and alienation appears congruent with findings advanced in other research—notably in the studies by Wilensky and Edwards and by Chinoy. The details of the present report differ in many ways from their research, but the underlying theme is the same: depressed aspirations seem to rigidify an individual's position to the point of developing acute sensitivity to economic rewards; when rewards are low—and the position permanent—it is cause for despair. On the other hand, definitions of a position as temporary seem to provide the individual with some autonomy from the possible repercussions of inadequate economic rewards. For high aspirants, the link between poor income and anomie consequently becomes tenuous.

If this theme is at all credible, two lines of inquiry may be profitably pursued. First, it would be necessary to link the variability in aspirations to members differentially distributed in the social structure. While much work on this point has been advanced in regard to traditional stratification variables, less is known about aspirations for non-vertical groupings—for example, the old and the new middle class. Equally important would be an attempt to identify the precise process by which inadequate income or other indicators of deprivation are translated into alienation. This could serve to link alienation to other forms of deviant behavior as well as to explain why alienation—rather than (or in addition to) alcoholism or mental illness—emerges as a reaction.[22] The exact process must in this paper remain a matter of speculation, but it nonetheless appears reasonable to suggest role compartmentalization or containment as a conceptual approximation of an intervening mechanism.

TABLE 5

ANOMIE MEANS BY INCOME AND OWNER-MANAGER STATUS WITH OWNERS' MEANS STANDARDIZED ON THE MOBILITY DISTRIBUTION OF MANAGERS

Income	Relations to Capital: Owners	Managers
$6,999 and less	14.0 (N=201)	12.6 (N=48)
$7,000-$9,999	12.2 (N=125)	11.4 (N=53)
$10,000 and above	11.2 (N=199)	11.5 (N=47)
Total	12.5	11.8
No. of cases	525	148

This suggestion derives from the observation that owners and managers differ in their ability to contain economic deprivation within the personal spheres of their lives. For owners, deprivation seems to spread as a form of antagonism and rejection of the larger society; for managers, deprivation is apparently *contained* more completely in their economic role. It would be interesting to know whether personal responsibility for failure is higher in the managerial group. The utility of this conception must, of course, await additional

regard to education, owners and managers had nearly identical levels of schooling—approximately twelve and one-half years. Differences in age were more pronounced: the approximate mean age for owners was forty-six years and for managers forty years. Controls for age (and education) did not, however, alter the differences between aspirations and anomie for owners and managers; similarly, first-order partial correlations with age (and education) as a control revealed little uniformity in the relations between income, anomie, and aspirations—other than the somewhat consistent finding that income and anomie tend to be more highly correlated among owners.

[22] Kornhauser reports a number of correlations indicating a negative relation between personal deviance and extremist behavior (*op. cit.*, p. 91).

research; the notion of role compartmentalization may, however, provide a more general phrasing for a theory of alienation and also permit a conceptual bridge to other types of deviant response to stress.

Regardless of the exact processes involved, the ideas emerging from this research are more in keeping with an economic interpretation of the differences between the old and the new middle class than with the mass-society emphasis on access to bureaucratic organization. If mass-society theories are to gain credence, various reactions—for example, the alienation of the old middle class—must be shown to rest on conditions distinctively different from economic deprivation. This research attempted to approximate a test of one of these conditions—links to bureaucratic organizations. Other conditions, as for example, the role of intermediate organizations, need to be more thoroughly explored.[28] Mass-society theory is in the grand tradition of historical sociological analysis and provides provocative insight into the condition of contemporary man. Much empirical work is needed before the theory's explanatory value can be placed in proper perspective.

UNIVERSITY OF MINNESOTA

[28] For a review of this literature, see Arthur Neal and Melvin Seeman, "Organizations and Powerlessness," *American Sociological Review*, XXIX (April, 1964), 216–26, as well as William Erbe, "Social Involvement and Political Activity," *American Sociological Review*, XXIX (April, 1964), 198–215.

BOOK REVIEWS

As a Tale That Is Told: The Autobiography of R. M. MacIver. Chicago: University of Chicago Press, 1968. Pp. x+269. $7.50.

Whatever is the formula for reviewing the autobiography of a colleague? Shall one comment on the man's scholarly product, in this case some twenty books and many an article and published lecture on social and political problems? No. His books have been reviewed. They are well known. Shall one tell of the man's public services? Robert MacIver has served on many a body appointed to study social problems and offer programs of social betterment. Or shall it be an account of his administrative positions in universities and how he filled them? From the 1920's until the 1960's—and his eighties—he was generally administering something academic. One shouldn't be astonished if he has even now taken on some new post.

Suppose we take the tack of looking at the man's career, of placing it in the career of sociology itself. Robert MacIver was born into a pious Calvinist household in a remote Scottish isle at about the same time as Ernest Burgess was born in a Canadian Congregational parsonage, the time when many another social scientist saw the light of day and passed his youth in simple and pious surroundings. At sixteen MacIver went to university, probably because of the piety of his background; he was nevertheless happy as a lark to be free of parental and village supervision. What a very American beginning—in Scotland! What a very sociological experience!

Had he been born in America he might actually have made a start in the church before turning to graduate work in sociology. As it was, he became a sociologist without a license, with two degrees in classics and none in sociology. He notes that he has never taught a subject in which he had any formal training. He started lecturing in politics at Aberdeen but soon fell afoul of the professor of moral philosophy, who had sponsored him in his daring introduction of a new subject.

He got a new start at the University of Toronto in 1911. He did not succeed in teaching sociology there, as he had hoped to do, but became chairman of the department of political economy and had a hand in organizing a school of social work. In 1927 he went to Columbia to teach politics in the graduate school and sociology in Barnard College (more or less succeeding William F. Ogburn, who went to Chicago in that year, adding a new facet to the sociological enterprise there). But Columbia did not offer firm ground in sociology either; soon a committee was appointed to do away with it. John Dewey, it seems, saved the day. Which is perhaps a bit ironical, for it was the University of Chicago department that had fostered the work and views of Dewey as especially appropriate to sociology.

The epoch of Ward, Small, Giddings, Sumner, and the other pioneers of American sociology was just past when MacIver came to Columbia in 1927. Only one of those pioneers, Small of Chicago, built an enduring department that went on without hiatus to the varied activities of later American sociology. Sumner at Yale had a successor who out-Sumnered him in being against tinkering with the mores and in his contempt, at the same time, of the people who practice and uphold the mores. Sumner at least declared that lynching was not in good taste and was indeed a perversion of the usual course of developing the mores. The Yale department took on new life later. At Columbia, the Giddings department rather petered out. The transition from beginnings under charismatic and doctrinaire founders to well-established institutions was no less difficult in sociology—which defined the process for others—than in other new movements and enterprises. MacIver's advent in Columbia marked the turning point there. But with growth there—as elsewhere—came conflict over points of view and method in which the humanistic MacIver lost. At about the time of his coming—in the 1920's—a large number of men who entered sociology via the University of Chicago after a try at some other occupation fanned out over the country

as members and chairmen of new departments in private and state universities and in the smaller colleges. It was not that Chicago had built up a monopoly; it was that the others had not shown much energy for initiative. Harvard finally decided to have something more than the Cabot chair of social ethics; it was rumored that the man to start any new subject had to come from east of Cambridge. Such a man, from Russia, was found in Minnesota; it was Sorokin, one of the great scholars of his generation. Eventually in the 1920's and 1930's all major universities founded or expanded their sociological enterprises. Before long, sociologists, perhaps those with paying clients more than others, began to talk of the profession of sociology, and even of licensing. The original interest of Small and his contemporaries in methodology, a sort of philosophy of social science, turned into development of a battery of research tools; at its best it has become very sophisticated consideration of both the logic and the techniques of social science and of their relations to each other. At its worst, it is rote application of a few "tests of significance" whose mathematical properties are not known to the users.

While MacIver may have lost in his own department, he has won on a larger front. Always a strong advocate of social justice and academic freedom, always aware of the trends and problems of the times, he has a much larger role than most academic people in studies designed to inform and guide public policy. He is an outstanding citizen of both the academic and the larger community.

Although he tells rather discreetly that part of his tale which has to do with the inner workings of universities, he notes that we of the academic world are the only profession whose affairs are run by people not in and of it. After the "long reign" of Nicholas Murray Butler, the board of Columbia University eventually asked that an advisory committee of faculty suggest the names of suitable successors. A list was prepared. "We heard nothing more until at a considerably later date it was announced that the new president would be General Eisenhower. It appeared that a top-rated industrial leader had informed our board chairman that 'Ike' might be available—and that was the end of the search" (pp. 159–60). That is not without significance at this moment when the constitution of universities is being called into question by a good many people. Robert MacIver himself has had a hand in university administration at several institutions and in several capacities; the last was the New School for Social Research, one of those movement-enterprises that has resisted complete institutionalization longer than most, that is, complete conformity to prevailing patterns of academic organization. If they do conform now, the pattern may be gone. The same is true of Robert MacIver; he won't conform now. But he may just as likely be considered a voice of the future on the viewpoint and responsibilities of the academic man as the teller of a tale.

If you don't know the man, read the book to get acquainted with him; if you do know him, read it and know him better.

Everett C. Hughes

Brandeis University

The Emergent American Society, Vol. I: *Large-Scale Organizations*. Edited by W. L. Warner, Darab B. Unwalla, and John H. Trimm. New Haven, Conn.: Yale University Press, 1967. Pp. xvii+667. $15.00.

The proliferation of empirical studies dealing with organizations requires new categories of distinction as well as criteria of evaluation. Apart from the substantive issues or the theoretical questions investigated, there are some obvious methodological categories in terms of which organizational studies may be distinguished. Thus, the period of the famous case studies of organizations during the early fifties was followed by a series of self-consciously "comparative" studies which were still tied, however, to typologies and the comparison of multidimensional type cases or sets of such cases. This approach, in turn, is now being superseded by a more quantitative orientation, a kind of organizational-survey research in which a large number of organizations, as well as a large number of variables on each, constitute the material. The data may be derived from individuals' responses about organizational properties, from aggregate statistics such as census data, through the collection or construction of collective properties of organi-

zations based on published material, or, finally, through direct surveys of organizations by means of mail questionnaires and contacts with key "informants."

Warner, Unwalla, and Trimm, in their roles of both editors and authors, approach the study of organizations both comparatively and quantitatively in that they make use of all of the techniques mentioned above, including an occasional illustrative case study. In the main, however, they, and the other chapter authors, rely heavily on a few key items of information derived from published statistics and census data, usually based on a large number of organizations within various categories.

But if for purposes of evaluation one uses such criteria as "descriptive" versus "analytical" versus "testing of significant ideas" versus "addressing important problems," the book does not reach the "take-off" point crucial analysis, explanation, and problem-solving. Instead, it remains on the level of "detached" description. In short, despite the impressive marshaling of potentially interesting data on organizations, there is no systematic analysis to speak of nor any significant involvement with theoretical or substantive questions. On the contrary a great mass of data is forced into a comparatively simple conceptual scheme to illustrate—or "account for"—an a priori view of the "emergent American society."

That view radiates a certain naïve optimism concerning the functional integration and adaptability of American society. For example: "All sizes and varieties of organizations exist and function successfully in this rapidly developing primary national community" (p. 20), or "adequate hierarchies of large-scale organizations provide systems of coordination to maintain the common enterprise" (p. 19), or "states and communities willingly turn to the federal government to help solve their local problems" (p. 21).

In the introductory chapter, Warner essentially states his major guiding assumption, namely, that as society and its institutional spheres grow in size, they become more complex. Complexity, in turn, is seen in terms of vertical and horizonal extension, that is, in terms of structural differentiation. Various forms of lateral extension are distinguished, such as *territorial, biological* (volume or size), *technolgical* (actually: spread of social objects or material culture), and *structural* (differentiated subunits). But the emphasis is on vertical-hierarchical differentiation as the central fact of large organizations. Structural-horizontal differentiation is partially subsumed under the notion of rank differentiation; that is, the relationship is assumed as given rather than taken as problematic. While hierarchically structured organizations are seen as performing coordinating functions for the larger, increasingly complex surrounding society, that society itself generates new organizations, which is why complex societies have many organizations.

Chapters ii–vii (almost half the book) deal with corporations. Chapters ii–v were written by Warner and Unwalla. The authors here are largely concerned with describing corporations in terms of various size measures, such as number of employees and volume of sales, and the relation of these indexes to the number of hierarchical levels, location of headquarters, and the like. But there is no attempt at systematic explanation or even exploration, such as studying the effect of division of labor on hierarchical differentation independently of size or of the possible intervening effects of mechanization and automation.

Chapter v is to me the most interesting in that it considers questions seldom dealt with outside microeconomics, namely, the phenomenon of interlocking corporate directorates. Apart from C. W. Mills's treatment of this facet of power elites (Mills is never mentioned), there is relatively little knowledge of exactly how much interlocking of top positions there is, even within the business elite. Thus, while for most industries considered, the *intra-industry* degree of interlocking is highest (directors of corporations within a given type of industry will tend to sit on each others' boards), there are still considerable differences in the pattern of distribution. For example, financial institutions such as banks and insurance companies, as well as metal-manufacturing corporations, rank high in the degree of *interindustry* shared directorships; construction and agriculture rank low.

Paradoxically, the case study of General Electric in chapter vi (written by George Downing, a former GE executive) is one of the few instances in which an attempt is made to formulate the conclusions in terms of gener-

alizations or hypotheses, even though they have a low degree of precision and specificity and lack the brilliance of a Barnard.

Chapter vii (by William E. Henry) is a sensitive treatment of executive personality and how it is affected by structural factors, such as the extension of horizontal relations or the increasing intra-organizational heterogeneity in big corporations. Complex organizations require complex, flexible executive personnel.

The second part of the book (chaps. vii–xii) deals with national associations (Frank C. Nall, II) and the organizational growth and change in institutional areas such as trade and business (Warner and Desmond D. Martin), labor (June M. Collins), religion (Gibson Winter), and education (Robert J. Havighurst). There are two final chapters on "big government" by Warner and John H. Trimm. The presentation follows the same pattern and organizing ideas used in the description of business corporations: first we learn about the growth in the relevant population base, then in the number and size of organizational units within the respective sector, and then in the hierarchical differentiation and bureaucratization within these units. The latter processes are time and again interpreted as a function of size and growth; that is, hierarchies develop because increasing size and complexity generate problems of co-ordination and control.

While this implicit functionalism is occasionally made explicit by reference to Parsons' or Warner's earlier work, there is little critical analysis or questioning of these assumptions, nor is there an attempt to show how some of these developments might be dysfunctional and outright harmful for the legitimacy and viability of American society. Hierarchical authority is certainly not just the functional response to complex problems of co-ordination but is also designed in the interest of existing power differentials and is often maintained and elaborated so as to justify extra-organizational status systems, as Weber, Bendix, and Udy, among others, have pointed out.

In sum, this book is a useful reference source for a description of some of the major parameters of organizations, such as size, complexity, and bureaucratization, and their simple interrelations. But it tells us little about the interaction of variables in relation to each other and to some measure of effectiveness or about the causal structure of the organizational phenomena described. For such an analysis, it will be necessary to do systematic organizational-survey research that explores and documents the multivariate nature of modern organizations with a view toward explanation and evaluation.

WOLF HEYDEBRAND

Washington University

Class, Status, and Power: Social Stratification in Comparative Perspective. Edited by REINHARD BENDIX and S. M. LIPSET. 2d ed. New York: Free Press, 1966. Pp xviii+677. $9.95.

This second edition is virtually a new book, apart from the added selections and the much greater total length. A majority of the earlier excerpts were discarded, and a majority of the present ones are new. Like a stratification system, the profile is familiar and stable; the membership is new.

As other reviewers note, the present edition pays much greater attention to an examination of status systems of other societies; indeed, there is now a distinctly comparative tone even though strictly comparable data are still scarce. At the same time, the number of correlates or derivatives of status receiving attention is greater; the "richness" of status as an element in social organization is more fully savored, if only by fugitive illustrations of many components.

In view of the promiscuous and confused intellectual origins of our present "theories of class structure," it may have been imprudent to open a discussion of this mixed-up topic with materials that convey an impression of some kind of conceptual firmness. An attempt to clarify the overlap or discrepancy among the three key terms of the book's title might have proved pedagogically preferable.

One can hardly avoid noticing the editors' disinterest in problems of measurement, so to speak, despite a larger ratio of data to commentary in this edition. Neither Lorenz nor Gini appears in the index; "inequality" is not indexed, and the references to "equality" are not illuminating. This deficiency, as I judge it, shows up in other curious ways also. The selec-

tions run the combinations among effects and components of status, but it would be difficult to undergird a serious analysis of the relative degree of "inequality" in occupational prestige compared with income, in educational opportunity contrasted with health. Perhaps we are observing here mainly the sociologist's liking for statistics and timidity about measurement.

Although this edition is more receptive to comparative and historical materials, the separate section on that dual topic is misnamed. The "historical" element is mainly casual illustrations from several societies and earlier centuries; this hardly creates a historical dimension for any society. The comparative element, useful as the assorted illustrations will be, seems to have been inserted in an equally desultory manner. These particular shortcomings could have been remedied in a measure by the supplying of a modest bibliography embodying a detailed topical ordering. Thereby the students' preoccupation with the contemporary—despite the scarcity of discussions about "race"—could have been satisfied and their interests led into broader viewpoints.

Other reviewers will no doubt parallel my comment that there is not much source material provided for analysis of the relation between "status groups." I refer less to hoary exegesis about "class conflict" than to the more explicit collections of information thrown up by contemporary liking for empirical data.

What I am saying, to be honest, is that no single book can more than touch on the bold outlines of this enormous field of phenomena, controversy, and data. No more useful compilation is at our service. Perhaps the size of this volume speaks mainly to our inability to conceptualize this dimension of social organization more sensitively and with less obligation to our disciplinary predecessors. On the other hand, the shelf full of books on poverty issued in the last few years adds surprisingly little tightly packaged thinking and not many astonishingly novel facts to what we knew about that topic. Partly this is because what should be a truly interdisciplinary field of inquiry remains shattered and inchoate.

C. ARNOLD ANDERSON

University of Chicago

Transformation of the Negro American. By LEONARD BROOM and NORVAL GLENN. New York: Harper & Row, 1967. Pp. xii+194. $1.75.

Negro Youth at the Crossways. By E. FRANKLIN FRAZIER. Introduction by ST. CLAIR DRAKE. New York: Schocken Books, 1967. Pp. xxxv+294. $2.45.

Negro and Jew: An Encounter in America. Edited by SHLOMO KATZ. New York: Macmillan Co., 1967. Pp. xvi+138. $1.45.

The Middle-Class Negro in the White Man's World. By ELI GINZBERG and ASSOCIATES. New York: Columbia University Press, 1967. Pp. viii+182. $5.00.

Black Power: The Politics of Liberation in America. By STOKELY CARMICHAEL and CHARLES V. HAMILTON. New York: Random House, 1967. Pp. xii+185. $4.95.

Broom and Glenn have written a concise but judicious review of main trends since the Civil War leading up to black activism of the 1960's. They identify four main themes in Negro adjustment to subordination. *Compliant adaptation* was perhaps the only practicable route during the nineteenth century but was institutionalized through the distinctive patterns of Negro church and family and left as residue a somewhat distinctive subculture that would impede later efforts toward integration. The early twentieth century saw alternation between *gradualism,* involving effective isolation from the main stream of American life, and *nationalism,* most potently expressed in Garveyism and the Black Muslim movement. Overt pressure toward *integration* is largely a post-World War II phenomenon. After three chapters reviewing the evidence concerning advances and remaining inequalities in education, occupation and income, and business and professions, the authors assess the nature and sources of recent organized protest. Much emphasis is placed on the interaction between the more intense dissatisfaction of middle-class Negroes, who nevertheless are likely to pursue constructive programs of reform, and the tendency for hopelessness among lower-class Negroes to find expression in less disciplined and destructive activity.

Reading this book today is a lesson in the rapidity of contemporary change. In spite of

a three-page addendum to the 1967 edition, the volume was completed before the new era of politicized violence and the ideological repudiation of integration by the most conspicuous leaders of Negro protest. The resistances to further Negro advances are correctly anticipated by Broom and Glenn: the new pattern of guerilla warfare could have been foreseen by few. In a sense, each of the other books is documentation for a phase of this transformation.

In the late 1930's the American Council on Education commissioned four studies of Negro youth, each in a different region. E. Franklin Frazier used Washington, D.C., and Louisville, Kentucky, to characterize the border states. He eschewed standard instruments and presented a humanistic account of experiences and reactions to growing up as a Negro that was based upon direct observation and loosely structured interviews. The reissue of this volume today is especially interesting. Whether one agrees, as St. Clair Drake asserts in the new Preface, that "the psychological roots of the demand for Black Power are laid bare in this volume," many of the main themes of today's protest do appear in subdued form.

The observation that border youth struggle with *rage* while southern youth struggle with *fear* seemed but a minor theme in 1940, when no breakdown of control was imminent. Frazier's analysis of the lower-class Negro youth offers the most clues to our current scene. Preoccupation with being Negro and wariness in relations with whites characterized all classes thirty years ago, but the lower-class youth were already looking toward illegal means of livelihood as an accepted way of life and complaining of police brutality. A clue to subsequent changes is supplied by Frazier's observation that lower-class youth learned subordination to whites from their parents but learned to express hostility against whites through neighborhood relations.

On the contemporary scene we look first at two minor eddies in the current of race relations. A by-product of the civil-rights struggle has been a sudden airing of the anti-Semitism long prevalent in Negro ghettos. With their exemplary record in civil-rights causes and their vital memory of persecution, many Jews have had difficulty accepting and understanding this phenomenon. Furthermore, the fear has grown up that there may be a Jewish backlash, with retaliatory race prejudice. In an effort to assess the importance and nature of this problem, twenty-seven public figures were asked to prepare statements in answer to a set of eleven questions. In the volume edited by Shlomo Katz the replies shed a little light, offer few solutions, and often reflect little intimate understanding of the situation. But they supply a valuable record of how people deal with a distressing anomaly of intergroup relations. Responses range from Floyd McKissick's charge that Jews take unjustified credit upon themselves for the civil-rights movement to Joel Carmichael's declaration that Jews have already demanded too much of themselves. Writers tend either to adopt the view that anti-Semitism in the ghettoes is incidental to the landlord and merchant role or, like Howard Fast, to find the source in the Negro's immersion in a deeply anti-Semitic Christian tradition. One of the few statements that reflect first-hand experience in the ghetto comes from Paul Jacobs, who illustrates, on the basis of ghetto dialogues, how exclusively the Jew is seen as salesman and landlord and how sharp a distinction is made between Jew and white man. It would be interesting to know whether the frequent theme of discounting the potential clash between Negro and Jew would have been less prominent in a set of statements prepared after the recent Arab-Israeli war.

Ginzberg's staff interviewed 120 male students—high school seniors, college sophomores, and college seniors—in New York and Atlanta to find out how middle-class Negroes are responding to the new opportunities open to them today. This investigation was intended only as a pilot study, and the findings are reported in impressionistic non-specifics. The main conclusion is that most of these youth are rather confident that they can succeed on their own merits in spite of the barrier of race and have not allowed their race to greatly affect their educational, occupational, or life-style goals. Although a few of these young men offered cynical responses, none of the explosive anger of "black power" is manifest. It would be dangerous to take this superficial study very seriously. What must survely have been missed is a soul-wrenching ambivalence, against which the middle-class Negro's overassertion of opportunity is the last remaining defense. Perhaps this work asks only a former generation's questions and fails to see that pride and self-

respect rather than economic opportunity constitute the problem of today's middle-class Negroes, the problem that drives many on the campuses and in civil-rights movements to form common cause with the economically deprived lower-class black.

If Ginzberg's youth are asserting opportunity in order to save themselves from taking the last step into alienation, if the Jews find that no one cares very much what the juggernaut of Negro protest is incidentally doing to them, if the faint overtones of the thirties have now become principal themes, then black power is the key for today's racial accommodations. Carmichael and Hamilton's book admirably mixes scholarly dissection of American society with the promotion of a mystical symbol. The critique is sound though one sided, well documented from reputable sources, and contains little that is not commonplace to sociologists. But as a document the volume expresses magnificently the dilemmas of an angry minority too small to hope for a seizure of power and too weak to endure the prospect of being cut adrift and illustrates some of the universal features of social movements during the stage when indignation is at a peak but no acceptable program has evolved to serve as the movement's aim.

Black power is a symbol in search of concrete meaning. It is characteristic of the stage in the movement that this should be so, for varied meanings can be tried out. Furthermore, the constituency must be welded into a firm unit and mobilized for action before it can hope to withstand the inevitable disappointments attendant on specific programs. But the most striking impression gained from this book is that it was written for a white audience and not for blacks. Hamilton's scholarship is not needed to document what black men know from personal experience; if blacks use the work, it will be principally a source of ammunition in parleys with whites. And if, as the authors assert, there is a fundamental conflict between black culture and white culture that requires the complete remaking of American society, the critiques and the faintly suggested solutions are taken directly from the lexicon of traditional white liberalism. The mixture of frustration over the working of American institutions with a dedication to many of the most respected American values suggests a Wesley threatening but not quite ready to leave the established church until forced to do so.

If other movements offer any clue, the stage of loud screaming and violence cannot last very long before reaction sets in within the constituency as well as in the larger society. Since objective change is unlikely to come rapidly enough, we surely will look forward to some decades of atlernating quiescence and intensification of the kind of attack represented by black power. Perhaps the key element not touched by any of the foregoing works is the uncertain coalition between the new youthful protest in white America and black power. As youth pass from their screaming stage into the development of a serious program of reforms, a firm coalition may be the most favorable condition for substantial reform in the Negro's position.

RALPH H. TURNER

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Public Opinion and Canadian Identity. By MILDRED A. SCHWARTZ. Berkeley: University of California Press, 1967. Pp. xviii+263. $7.50.

This is an excellent, unusual, and important book. It is excellent because it is well researched, tightly organized, and modest in its claims. It is unusual because it is consistently readable, even to this reviewer, who is a historian not at all fond of the style of most sociologists. It is important because it tells us something we need to know about Canada and then goes on to tell us something more about nation-buliding and crises of identity within pluralistic societies.

Professor Schwartz is a Canadian who, after teaching at the University of Alberta, Calgary, now teaches in the University of Illinois at Chicago Circle. This book, which grew from her doctoral dissertation at Columbia, on "Canadian National Identity as Seen through Public Opinion Polls: 1941–1963," draws upon some twenty years of Gallup Poll data but wisely recognizes, as researchers have not always done, that using data for purposes other than their collectors intended is a dangerous task. She has used a large range of secondary literature, scholarly and popular articles, and models constructed by other

scholars to provide a firm underpinning for her structure. Indeed, at times the book seems to be a running conversation with many of the major figures of present-day political science and sociology, from her mentor Juan Linz and Seymour Martin Lipset (who wrote a perceptive Foreword for the volume) to Robert Lane, Robert Dahl, and Karl Deutsch. The use of the data is scrupulous, and the findings are undoubtedly correct.

The findings are not, for all that, particularly new. Anyone who has been a close student of the developing Canadian debate over national identity or who has read Canadian newspapers consistently—a source upon which Professor Schwartz does not draw for her observations on public opinion—already will have arrived at similar conclusions. Yet it may be true that, while many knew, by the defective process of intuition and reading, that there is an absence of consensus in Canada about external, domestic, and symbolic issues and that such an absence implies that Canadians have no clear image of what it means to be a Canadian, some value lies in having this knowledge given concrete form by quantitative analysis. Certainly the observations in this book—ranging from the flag debate, to the continued relevance of the Queen (a relevance Professor Schwartz might have placed higher had she read the works of W. L. Morton), to problems of political parties rent by ethnic and regional cleavages—are valid, significant, and necessary.

Necessary because one hopes that the fruits of Professor Schwartz's research will reach many American scholars who have erroneously thought Canada to be much like the United States. Clearly, whether one's evidence is intuitive, historical, or quantitative, this is not so, and it cannot be said too often for too many diverse audiences. This book, which helps forward the methodology of comparative macroscopic analysis, should also help forward American awareness of Canada. The chief conclusion, which is further supported by Professor Schwartz and her co-author, F. C. Engelmann, in *Political Parties and the Canadian Social Structure,* which appeared later in the same year, is that political parties traditionally provide solutions to national problems by cutting across other group loyalties and that in Canada the principal parties are powerless to overcome the cleavages created by language, race, geography, and history. This augurs poorly for Pierre Elliott Trudeau, who must attempt to speak to these points in Parliament and on the hustings; one hopes that Professor Schwartz is mistaken (as I believe she surely is in her aside about the National party in New Zealand and her low assessment of the role of the military as an influence on national identity), but one fears that she is not. It is a measure of the success of this book that the fear is increased upon reading it.

ROBIN W. WINKS

Yale University

The Year 2000: A Framework for Speculation on the Next Thirty-three Years. By HERMAN KAHN and ANTHONY J. WIENER. New York: Macmillan Co., 1967. Pp. 431. $9.95.

Although literally lousy with good ideas, this is a non-book; I wish the authors had taken the time to write a book instead. The topic—what will, can, or may happen in the next thirty-three years—does not provide by itself a coherent enough and intellectually satisfying structure for their scattered, often brilliant and well-elaborated, but quite discontinuous ideas. A good editor might have helped, too. As it is, the reader has to work hard to grasp *The Year 2000*—but it is worth working hard for.

Wherein (apart from scope) does sociology differ from the "scenarios" of *The Year 2000?* We try to extract from the past, and from the present, regularities supposed to help us understand both and to help predict the future. In practice social scientists generally use data, including assumed future facts, to test the correctness of their "laws." In contrast, historians, using laws, of course, tell about the past for its own sake. Kahn and Wiener are more like historians, though of the future, than like social scientists. Historians give us alternative interpretations of the same present data: alternative pasts; Kahn and Wiener offer alternative futures. Like historians, they are consumers, not producers of social science: they do not try to establish or test "laws" by means of the facts; they try to predict facts by means of the "laws" (correlations). To do so, they go where sociologists usually fear to tread: by narrowing the range of concrete possibilities while specifying some likely ones, by

filling some of the empty boxes we provide. We usually feel that neither data nor theories suffice. I think we are probably right but Kahn and Wiener are interesting.

Since scientists by establishing recurrent correlations attempt to make it possible to predict the future, but do not predict it, *The Year 2000*, which does, is nearer utopian novels and essays (such as those of H. G. Wells), which also do. It differs from these because Kahn and Wiener do not paint alternative futures in terms of their fantasies and values but attempt to extrapolate (using quite acceptable techniques) from the present and to look at possibilities *sine ira et studio*.

Sociologists have long pointed out that we all constantly predict the future by making implicit assumptions in our own behavior (based on past experience) about the behavior of our environment. In times past, only a few social institutions, such as governments, were interested in explicit predictions; these were usually unsystematic conjectures about specific events within an otherwise constant framework: who will win the war, what effect will it have on us, etc.? We need more systematic predictions now for two major reasons.

1. Owing largely to technological developments, the implicit past assumption of reasonable constancy or slowness, of *pari passu* development, of recurrence and direct applicability of past experience, no longer holds: the pace of change is so fast, and in some respects so discontinuous, that explicit anticipations of major changes are likely to be more correct than implicit assumptions of no change or of slow, continuous development or of change only in specific sectors within a constant framework. Now the framework of our lives changes perceptibly, as does what happens within it.

2. The same developments which led to the rapidly accelerating pace of change also led to the growth of many institutions whose policies must anticipate the future explicitly. Their only way to maintain the *status quo* (in terms of income or power) for themselves is to anticipate the *status quo post* and do so continuously; and they must anticipate that the *status quo post* significantly differs from the environment which led to the *status quo*. (This anticipation, and consequent actions, may also influence the *status quo post*.)

Thus there are on the one hand more changes to be anticipated and on the other more policy-making bodies that have to anticipate. Wherefore "scenarios" of the future will be part of the future more than they have been of the past. The Kahn-Wiener work has the virtues and vices of a pioneer effort. Although annoying in some respects, it is extremely instructive, and I recommend it highly—both to those interested in propaedeutic and to those interested in prophylactic activities. Since both activities are possible, we are confronted, not with a prediction of an actual future, but with the elaboration of a series of potential or optional futures.

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The Social Meanings of Suicide. By JACK D. DOUGLAS. Princeton, N.J.: Princeton University Press, 1967. Pp. 398. $8.50.

This book will be cited widely as an intended refutation of Durkheim's work on suicide, but its implications are much broader. The author's arguments illustrate the perspective of an emerging school in American sociology—phenomenology or ethnomethodology. Consequently, though this review is negative, the book's significance is not questioned.

Much of Douglas' critique of Durkheim's work is harmless, especially his showing how previous studies of suicide influenced Durkheim. That demonstration is a contribution to the sociology of sociology, but it has no direct bearing on the validity of Durkheim's theory, nor does Douglas' concern with labeling Durkheim (e.g., realist, idealist, positivist).

As the author moves from epistemological issues, his criticism becomes trenchant but not original. He points out, as others have before him, that Durkheim supported his theory primarily by rhetoric rather than by formal tests. Douglas also follows a familiar path in characterizing the theory as difficult, confusing, inconsistent, unsystematic, contradictory, vacillating, vague, and uncertain. However, he then accuses sociologists of misinterpreting Durkheim, which is startling to say the least. But Douglas' own interpretation is

interesting. He stresses that Durkheim's designation of the "cause" of variation in suicide rates vacillates; at some points in *Suicide* it is behavioral (e.g., actual interaction) but elsewhere normative (more generally, the internal aspects of social life and culture). Douglas resolves the vacillation by maintaining that Durkheim really designated nonbehavioral phenomena as the cause; and he narrows the phenomena by focusing on "social meanings." The endeavor illustrates the sterility of exegetical sociology. The central question is not what Durkheim really "meant" (only pedants should care) but, rather, how the theory can be reformulated to derive testable generalizations.

Douglas does not define "meaning," let alone formulate a related measure and demonstrate its relation to the suicide rate. Instead, he states: "I shall simply rely upon a cultural understanding of the term" (p. 245). This reviewer participates in that culture, but I am distressed by the seeming infinite meanings of social meanings. Douglas' statements suggest that the suicide rate is low when there is general perceptual uniformity, marked semantical agreement, a high degree of normative consensus, uniform adherence to a set of values, or intense proscription of suicide, to mention only five possibilities. Ambiguity is compounded by emphasis on the "social meanings of suicide," which indicates that the rate somehow depends on the perception of suicide in the population.

The problems in attempting to fathom Douglas' statements are alleviated by two realizations. First, "the meanings of suicide are very complex and obscure, not alone to the theorists, but to the social actors involved as well" (p. 158). And, second, since Douglas eventually buries the problem of explaining variation in the suicide rate, it is pointless to treat his observations as a theory on that subject.

As the initial step in rejecting the problem, the author questions the reliability of official suicide rates. That assertion has a long history. It is an article of faith in psychiatry, and many sociologists accept it uncritically. I argue that the state of knowledge precludes defensible conclusions on the subject. In using official statistics to test theories, *absolute* reliability is not the issue. Rather, the crucial question is interpopulation *variation* in *relative* reliability (the proportion of actual cases of suicide reported as such). However, since "true" incidence never has been known for any population, conclusions about relative reliability are conjectural. Moreover, research to estimate true incidence in even a few populations would be a thankless task, and the conclusions reached could not be generalized justifiably to all suicide rates.

Douglas makes an admirable effort to assemble evidence on the reliability question. Unfortunately, however, he ignores his own admission (p. 167) that we have very little evidence and subsequently indulges in non sequiturs. As one instance, Douglas maintains (p. 176) that Durkheim's rejection of official statistics on suicidal motives invalidates his use of incidence figures. Even the fact that commentators differed to imputing a motive to Marilyn Monroe's alleged suicide receives three pages of attention. Douglas' argument is that individuals differ in their interpretation of the "meaning" of suicide and, consequently, disagree in identifying cases. Here the "meaning" of suicide is explained to include alleged causes and motives. He does cite a few studies that have compared two sets of "suicide statistics" for the same population. Differences of 20 per cent are not unusual, but the inference is more debatable than Douglas pretends. For example, in Prussia in 1883 the official number of suicides increased from 4,984 to 6,171 after a change in the method of collecting statistics. What does that event reveal about all suicide rates? The question is especially important since Douglas does not mention instances where "counts" have been much closer, though at least one is known. Even a case like Prussia does not permit an incontrovertible conclusion as to how much either figure is unreliable.

Douglas admits but later ignores that his argument appeals primarily to plausibility. To illustrate, observed stabilities in suicide rates over time may be construed as evidence of their reliability. Douglas counters with the assertion that "the probabilistic nature of the categorizations is sufficient in itself to account for the regularity of official suicide statistics" (p. 200). I understand the argument but fail to see how the explanation is more plausible than postulating a somewhat stable "true" in-

cidence, and all the more since the probabilistic *postulate* does not explain how the official rate of some populations (e.g., a country or a marital status) can be ten times that of others.

Elsewhere Douglas attempts to wring evidence from the argument that: (1) the formal definition of suicide (e.g., Durkheim's) does not correspond to definitions employed by officials in categorizing cases and (2) officials do not agree among themselves. I agree that such divergence exists, but how much is another matter. Certainly Douglas does not document the suggested degree of divergence, and a demonstration of marked divergence would not be conclusive evidence that even one suicide rate is unreliable. What Douglas overlooks is that different intensions of a concept do not result necessarily in extensional divergence.

Most sociologists who use official suicide rates express doubts about reliability, and, unlike Douglas, they are willing to treat the issue as an open question. They use official rates for a good reason—there is no feasible alternative. Of course, they assume that the relative reliability of the rates makes them better than nothing, but Douglas demands a substantiating demonstration. I submit that if sociologists had to demonstrate the reliability of their data, macroscopic research would terminate. Moreover, there can be no conclusive finding on the issue. Since the true incidence of suicide is unknown, then statements about the relative reliability of the official rate are dubious. Thus, Douglas cites (p. 172) one investigator's conclusion that the official number of suicides in the population studied represented about one-half the actual number. How could one know that with any certainty whatever? True, various methods could be employed to estimate the true incidence, and such studies are needed. But *no* study can resolve the more basic issue raised by Douglas, one which negates both the reliability question and variation in rates as a sociological problem: "There does not exist such a thing as a 'real suicide rate.' Suicides are not something of a set nature waiting to be correctly or incorrectly categorized by officials. The very nature of the 'thing' is itself problematic so that 'suicides' cannot correctly be said to exist . . . until a categorization has been made. Moreover, since there exists great disagreement between interested parties in the categorization of real-world cases, 'suicides' can generally be said to exist and not exist at the same time, though this might seem a rather incongruous way of putting it" (p. 196, n. 40). Incongruous is putting it mildly.

It would be pointless to attempt refutation of Douglas' brand of solipsism and sociologists cannot resolve the related issues any more than philosophers. But a consideration of the implications is needed. For one thing, as Douglas makes quite clear (p. 339), the perspective is not limited to the study of suicide; that is, the argument applies to any *social* act. As for suicide, the basic issue is not the reliability of official rates, but, rather, is epistemological—there are as many suicides and rates as there are divergent categorizations of deaths in the population. Even that statement may be objectionable to the phenomenologist or ethnomethodologist. Individuals who characterize a death as suicide may not really "mean" the same thing, and that poses an insoluble problem for anyone addicted to the positivistic vices of classifying, counting, and comparing. True, we can ask the "labelers" what they mean by suicide, but they will employ other concepts in answering, and the investigator cannot know what they "really mean." Regardless of the basis of a rate, it cannot be compared with that of another population if, as Douglas insists, the social meanings of suicide are markedly different between populations. Hence the extension of ethnomethodology ends with the particular and the unique.

Given Douglas' perspective, his negative evaluation of sociological theories on suicide is not surprising, focusing as it does on the failure of theoreticians to consider social meanings. Since he denies conventional evidence, Douglas is scarcely concerned with the predictive power of theories. A critique of one of the theories is based on an article published in 1958 and ignores a later (1964) monograph that reports hundreds of tests of the theory; the one coefficient of correlation mentioned (p. 89) is incorrect. Some research findings are considered later in connection with theories, but they are explained away by postulating conditions that lead to the concealment of suicides. Strangely, Douglas ignores the possibility that the very conditions which sup-

posedly lead to the concealment of suicides may be the cause of a low "true" rate. True incidence is, of course, a concept by postulation, but the notion in any form is alien to Douglas' perspective. However, paradoxically, he speaks of rates of concealment, and does so without presenting any data whatever.

Douglas dismisses findings that show an inverse relation between measures of status integration and suicide rates; he assumes that the measures are correlated inversely with urbanization, one of the conditions that supposedly produces a *low* rate of concealment. If so, why is the current official suicide rate of the United States not appreciably different from that at the turn of the century? Similarly, if the middle class has lower official suicide rates (a debatable point in itself) because of higher rates of concealment, what of the very low rate for Negroes? Since Douglas invents concealment rates as he needs them, he can explain these seeming exceptions readily. However, the implications transcend particular populations. Douglas' perspective negates conventional etiological research on suicide. If a death is a suicide only when so labeled, then the central question is not why individuals end their existence. Douglas argues that labelers disagree to the point that there are not and cannot be "real" cases of suicide; but he does not recognize that only one question survives the argument: Why do the labelers agree or disagree?

Rather than switch his analysis to a study of labelers, Douglas writes as though a sociological study of *suicides* is possible. But the procedure for identifying cases is not specified, and the change in terminology from suicide to suicidal "phenomena" or "actions" is not helpful. In any event, the problem of variation in the rate never appears in the last part of the book, which is concerned with the social meanings of suicide. The statements do suggest that suicides (or "suicidal situations") can be explained, but the author's conception of explanation is not made explicit. Evidently "suicidal situations" are explained by ascertaining (imputing?) the meaning for the deceased. Douglas identifies four "common patterns"—suicide as a *means* of transforming the soul, transforming the substantial self, achieving fellow feeling, and getting revenge. If the explanation of a suicide involves something more than ascertaining meaning, it is not stated. Nor does the author specify how "meanings" are to be ascertained or how interpretive differences are to be resolved.

The empirical applicability of the concept suicide and the reliability of related statistics receive considerable attention in this book. Since sociologists should be actively concerned with such problems, this negative review may appear merely partisan. But the assessment does not question the significance of the problems considered; rather, it is Douglas' extending them to an irresolvable philosophical issue (solipsism). He could have contributed much more by bringing systematic evidence to bear on a question: In each of a series of populations, how much do individuals disagree in labeling each of all deaths (or a random sample) suicide or non-suicide? Several variables could be introduced in such research—classes of labelers (e.g., sociologists employing a formal definition of suicide, officials employing their own conception of suicide), the amount of knowledge of a case, and the social relation of the labeler to the deceased. The findings would be not only relevant for Douglas' argument but also of interest beyond the question of the reliability of official suicide rates.

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Economic Failure, Alienation, and Extremism. By MICHAEL AIKEN, LOUIS A. FERMAN, and HAROLD L. SHEPPARD. Ann Arbor: University of Michigan Press, 1968. Pp. vi+213. $7.50.

This mongoraph, in examining the relation of unemployment to social isolation and political attitudes, deals with questions of central importance for the sociology of work. Following the publication by Harold L. Sheppard, Louis A. Ferman, and Seymour Faber of *Too Old To Work, Too Young To Retire* (Washington, D.C., 1960), this is the second report on the unemployment and re-employment experiences of workers left jobless by the closing of a plant, the Packard Motor Company, which shut down in 1956. The aim is to explore social and psychological adjustments to job displacement. Thus the research extends the earlier studies of the Great Depression con-

ducted by Jahoda-Lazarsfeld and Zeisel, Bakke, and others by focusing on a more affluent recent era in which joblessness, though comparatively rare, still persists under certain conditions and in selected segments of the work force.

The authors (a teacher of sociology, a research director for a labor- and industrial-relations institute, and a staff social scientist in an institute for employment research) obtained their data by questioning three hundred ex-Packard workers two years after the shutdown. This sample is reflective of their research interest in economic failure, since only about half the workers were re-employed at the time of interview and the over-all average period of unemployment was ten months. The authors do not claim representativeness for the sample; they note that (apart from 22 per cent non-response) the sample comprises the older workers with high seniority within the plant and that, in comparison with other automobile workers in Detroit, the Packard workers at the time of shutdown were generally less educated, more highly skilled, and older (over two-thirds were at least fifty years old).

A number of potential concomitants of job displacement are examined from these data. One key variable is economic deprivation (fewer savings, more debts, reduced expenditures on essentials), which appears to relate to unemployment, on the one hand, and, on the other hand, to weak social integration in terms of dissatisfaction with life, anomie in the Srole sense, and infrequent visiting with friends and relatives. This emphasis on economic resources as an influence on workers' attitudes and social participation, an emphasis often neglected in sociological studies, is a strong point in this research.

A second set of variables, representing miscellaneous political attitudes (alienation, economic liberalism, political extremism, and antidemocratic attitudes), is then investigated as the possible outcome of either the social isolation or the economic insecurity attendant upon unemployment. However, the associations observed among political attitudes and such explanatory variables seem disappointingly slight and inconsistent (e.g., see p. 128). Moreover, as the authors caution, the data afford only an incomplete test of the theory of Kornhauser and others, that social isolation increases susceptibility to political extremism, because the secondary group structures stressed in the theory were not covered in the questionnaire. In particular, the omission of questions on union participation seems unfortunate in view of the presumed tendency for such participation to strengthen political interest (p. 102). Omissions of this kind appear to arise because of the *post hoc* nature of much of the theoretical explanation given to the findings on political response.

Unhappily, the study loses power as an intensive, exploratory analysis not only through conceptual inadequacies but also through application of quantitative procedures that are often inappropriate for the purpose at hand. Many of the measures, instead of utilizing the full pattern of the workers' answers to the varied questions, are scores based upon arbitrarily assigned weights and averaged as if they were ratio scales. The analysis attempts to assess the relative ordering of independent variables according to their explanatory importance by using a multiple-classification analysis (pp. 172–74) based on a least-squares approximation, assumes additivity of effects, and uses an F-test for significance (though test results are typically ignored in the interpretation of findings). Most of the data are presented in tables, with only occasional quotation from respondents and interviewers to illumine the statistics.

For the future, one might wish for an accumulation of case studies that penetrate strategic disturbances and changes in the social system, as this one probes into the stress induced by automation. To maximize the clues and insights contributed by such case studies, however, will require clarity of conceptualization in advance of the research and procedures that are at once descriptive, detailed, intuitive, and qualitative.

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The College Environment. By ALEXANDER W. ASTIN. Washington, D.C.: American Council on Education, 1968. Pp. xi+187. $3.00 (paperbound).

Among the growing company who study American colleges, Alexander Astin is notable

for his pioneering use of regression models to estimate "college effects"—the distinctive impact of colleges on students' sentiments and activities. Using a large probability sample of American universities and four-year colleges, Astin's earlier work, controlling for student input, shows quite convincingly that these schools differ in their effects upon students.

But neither his research nor any other comparison of large numbers of schools has traced successfully the environmental sources of these variations—this despite the extreme heterogeneity of American college programs and organizational forms. Lacking the detailed ethnographic material available to a Newcomb, the large-sample studies have been handicapped by measures of college environments either too gross or too distant from the socializing mechanisms at work in colleges. As a result, these studies undoubtedly have masked the impact of interschool variation in those environmental traits most salient for students, for example, the normative orders of peer relations and classroom life.

In time, comparative designs must supplant case studies; thus it is essential to measure more adequately salient aspects of college environments. Astin addresses this task in the present monograph. Unfortunately, the measures he has produced are unlikely to satisfy sociologists or improve college studies.

In August, 1962, Astin mailed a long Inventory of College Activities (ICA) to a probability sample of 60,505 students representing 246 accredited colleges and universities. These students had completed the freshman year that spring. Answering the Inventory's pre-coded questions, this sample, as informants, described the conduct of certain of their classes, the nature and enforcement of college rules, the "college image," their living arrangements, and other of the school's physical facilities. They also described their own activities in college—both solitary and in the company of their fellows.

Response rates were good and generally free of significant bias. From the questionnaires received, item scores were computed for each college and used to derive thirty-five factor scales. These scales purport to measure the major environmental stimuli in peer, administrative, and faculty behavior, in physical surroundings, and in "image" that help to form students' beliefs and channel their behavior.

The scales are technically adequate and ingeniously built. *The College Environment* reports a neat exercise in data reduction. But it accomplishes little more. Having constructed his scales, Astin correlates them with a myriad of other data about the colleges, generally of the same order as the items in the factors, but also such familiar gross measures as school type, source of control, and region. These correlations are to aid in interpreting the factors. They are consistent and the factor structures clear. But they do not answer substantive questions about sources of these stimuli in the social organization of colleges, their incidence or prevalence among American colleges, or their probable effects on students. Yet Astin devotes the bulk of his monograph to a dry recitation of these correlations, and there rests his case.

Moreover, despite their technical quality, the factor scales themselves should not appeal to the thoughtful scholar. First, the Astin study, for all its methodological sophistication, was essentially a fishing expedition. On inspection of the ICA, one finds that Astin got out what he put in. Productive studies of college effects will be guided by theory, which inevitably will dictate measures different from these. Cookbooks of variables probably inhibit hard conceptual thought, although perhaps not in quarters where it will bear fruit. We may expect the Astin scales to be used widely in rote research.

Second, college effects are neither unitary nor monolithic. Very different sets of variables may affect, for example, students' marital choices and commitments to intellectual endeavor. Both may be college effects, yet require different conceptions of college influence. Astin's scales seem to be constructed with some conception of outcomes in mind, but these are not revealed to the reader.

Third, using either these measures or others similarly constructed, one has no assurance that, except in small, culturally homogeneous colleges, they represent the effective environment for some or even most students. The whole college is probably not the appropriate analytical unit for a complex university like Minnesota or a variegated liberal arts college

like San Francisco State. Units of smaller scale and large samples are not incompatible.

Fourth, while truth may reside in the eye of the beholder, there is something to be said for independent measures of environmental traits. Not all environmental effects in fact occur as stimuli. Thus many structural constraints are never felt by those persons who are constrained.

The major work in measuring college environments and demonstrating their links to college effects still remains.

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Organisation and Bureaucracy: An Analysis of Modern Theories. By NICOS P. MOUZELIS. Chicago: Aldine Publishing Co., 1968. Pp. ix+230. $5.00.

Systems of Organization: The Control of Task and Sentient Boundaries. By E. J. MILLER and A. K. RICE. London: Tavistock Publications, 1967. Pp. xviii+286. 50*s*.

Organization and Environment: Managing Differentiation and Integration. By PAUL R. LAWRENCE and JAY W. LORSCH. Boston: Division of Research, Harvard Business School, 1967. Pp. xvi+279. $6.50.

Organisation and Bureaucracy.—A century of concern with organization and bureaucracy is summarized in this short volume (180 pages without the footnotes), which doubled as a Ph.D. dissertation at the London School of Economics. Nicos Mouzelis does not advance the theory of organizations, gives no exhaustive review of the literature, nor does he present a compilation of the major empirical findings (there's not a statistic in the book!). His modest aim in this formidable task is to provide the "novice in this field" with an orientation and some guidance through the "labyrinth of organisational writings." Specifically he does succeed in presenting the major approaches to the study of organizations, in critically examining them, and in tracing their development and recent convergences.

The author sees two main lines of development in organizational theory. The first has concerned itself with the phenomenon of bureaucracy. Among the classical writers mentioned, Marx, Weber, and Michels identified bureaucracy as an instrument of capitalistic oppression, a rationally efficient and technically superior administrative apparatus, or a tool for oligarchic control. The post-Weberian theorists of bureaucracy, including Merton, Gouldner, Selznick, Blau, Duverger, and Lipset, Trow, and Coleman, have tried to refine the concept and re-examine some of the "classical" problems with empirical research.

A second major development, the "managerial tradition," includes the scientific-management theories of Taylorism; the "universal" principles of the formal administration theories fathered principally by Henri Fayol; the human-relations approach with its various subschools, for example, those of Mayo, Rothlisberger, Warner, and Chapple and Arensberg; and finally the decision-making theories of Simon and Cyert and March. These four areas of study are categorized in one tradition because of their common focus on the individual worker or work group, whose activities, attitudes, motivations, and decisions must be manipulated in one way or another for the benefit of management and organizational goals.

Part III notes the convergence of these two traditions toward a more sociological and "organizational" focus. Here, as in general sociology, there are two theoretical tendencies; one emphasizes systems and integration, and the other stresses the conflict and division aspects of organizations. Parsons' general theory of organization is the most significant contribution in the former area, and Dalton's and Crozier's writings illustrate the power and conflict school.

Mouzelis knows and handles the literature well and accurately brings the reader up to the early sixties. As summarizer, synthesizer, and historian of modern theories, he serves his novice well. The more initiated student of formal organizations will appreciate the critiques of his favorite theorists; Mouzelis cuts clean and bold. Along with order, he does add critical insight to his borrowed materials. The clear treatment of Simon's and Parsons' theories stands out in this series of neat summaries and balanced critiques.

The author calls for a synthesis of the systems-integration theories with the power

and conflict models of organization, a greater emphasis on the organization as a whole, a closer link between general sociological theory and organization studies, more comparative research, and a wider treatment of the gut issues of alienation, democracy, and power struggles. We eagerly await his first research monograph.

Systems of Organization.—"Human-relations school" theorists from the Tavistock Institute, E. J. Miller and A. K. Rice draw from several of their case studies and their "severely practical" experiences as management consultants. Theirs is an open-system theory; every enterprise is basically an import-conversion-export system that constantly exchanges materials with its environment.

The conceptual scheme is developed in Part I; each concept and new term is clearly defined and used consistently thereafter. The main themes are illustrated in the next section when the scheme is applied to a sales force, to a dry-cleaning firm, to the family business (these very weak chapters could have been omitted), to the construction of a new steelworks, to research institutes, and finally to an airline operation. The heuristic nature of the theory comes across exceptionally well in the discussion of the introduction of computer systems in a steelworks.

The authors see a basic "project type" organizational pattern emerging in today's enterprises. Project-type organization draws on a "pool" of individuals; this pool can serve as a "scientific" or "professional" home base and provide a most important sentient group for those who form it. (A sentient group is the group "to which individuals are prepared to commit themselves and on which they depend for emotional support.") Individuals are temporarily organized to perform a specific task or take part in a given project. The particular form the organization takes must meet the requirements of *this* task and its technology, and deal with *these* human needs and constraints. Specialists or production workers can return to their "base" for retraining when necessary; they may even affiliate with a given enterprise only on a contract basis.

The moral of the Miller and Rice story, which they present with insight and authority, is that a going concern in a changing world should organize one thing only: flexibility. They warn their clients and readers that "the indispensability of the whole institution may depend on building dispensability into the parts" (p. 261).

Miller and Rice are by no means sloppy theorists; they build systematically and make careful distinctions, for example, in defining the boundaries around the individual, the group, the subsystems, and the enterprise as a whole. But would that theirs were truly a *general* theory. It is satisfying in human terms; life would be a lot nicer if their description did fit all enterprises. But does it, or can it, and actually should it?

Basically they are arguing from a number of case studies and are willing to extrapolate to all formal organizations. But the theory remains untested; its variables are only partially operationalized; there have been, of course, no comparative studies applying the scheme to a sufficiently large number of enterprises. Project organization applies to research institutes, to some of the activity systems in airlines, and to industrial firms with rapidly changing technologies. But can it, or should it, apply in enterprises whose primary tasks are standardized, routine, and meticulous? Can bureaucracy be so easily done in?

The authors have pulled together many important threads, not only from the human-relations traditions, but also from the recent sociological interest in technology, task requirements, and organizational environment. Project-type organization does form a basis for a theory of *some* organizations—an increasing number, we hope.

Organization and Environment.—Like their British counterparts, Professors Lawrence and Lorsch also write with a dual perspective based on a "long standing concern for both theory and practice." They, too, hope to "contribute to an emerging new theory of organization."

On the basis of their original study of ten firms, along with a review of six studies with diverse research orientations but very similar empirical and theoretical conclusions, the authors present a "contingency theory" of organizations. They use an open-system model, but the systemic nature of the organization as such is not as carefully outlined as it has been by Miller and Rice. Instead, Lawrence and Lorsch focus their attention on discovering the optimum interplay between the functional de-

partments within, and the relevant external environments outside, an organization, in order to determine what makes for high performance. Miller and Rice, on the other hand, ignore the Weberian problem of efficiency and effectiveness. In this regard the Harvard and Tavistock teams nicely compliment each other's work and replicate it in other ways.

This book is definitely written for administrators and managers, and the authors strive mightily to get across their main point: there is no *one best way* to organize. Different firms must deal with different environments, for example, a changing, dynamic environment for the plastics industry; a stable, certain environment for the standard-container industry; and an environment somewhat between these two for the packaged-food industry. Likewise, the functional departments that comprise the firms deal with different sectors of their environments; production deals with the techno-economic, research with the scientific, and sales with the market sector. The sectors themselves will be marked with varying degrees of uncertainty and change. Thus there is a varying need for differentiation of structure and orientation of departments within different firms. Differentiation produces the need for proper integration; integration is achieved only with proper conflict-resolution processes. We have, then, all the important variables for a contingency theory of organizations. Performance depends on the goodness of fit of the internal variables with the environmental variables and on the proper mechanisms of conflict resolution to integrate the necessary differentiation. The researchers quantify all the variables and present their arguments very convincingly.

But a case study is a case study is a case study. When will reviewers stop calling attention to its limitations? Never, as long as we're interested in a *general* theory of organizations. Lawrence and Lorsch go some important steps beyond, but essentially they base their arguments on the comparisons of one high-performing with one low-performing firm in three industries ("pre-testing" in six plastics firms). Once again we're left begging the question. And, as the authors readily admit, the test of the theory is yet to come. For example, they use the number of hierarchical levels and the span of control to measure formal structure. The former variable should never be used unless it is controlled for size, and the latter has been found to fluctuate so variously that its correlates, too, must be carefully controlled. Such controls are impossible with so few cases.

A less minor and more bewildering question arises from the six tables in chapter v, all of which report significance levels. I will leave it to the statisticians to decide if one can compute significance levels with a sample of two cases. My statistical imagination boggles at that possibility. Traditionally, human-relations theorists have studied the behavior of individuals in the context of organizations; and, as Mouzelis noted, the transition to a truly organizational focus has not been easy. Perhaps the case base for these tables is all the respondants in each firm (the *N*'s are not reported). But in that case, the unit of analysis is the individual managers and not the organization or departments as a whole, as the text and the theory clearly state.

This methodological confusion detracts only slightly from the real argument presented in the book. (And the managers won't notice it.) The argument stands on the basis of their "case studies" and the growing evidence supplied by the many other researchers cited. But it does make clear that the hard work "toward a contingency theory of organizations" is just beginning. If the theory lives up to half the utopian expectations discussed in the final chapter, it will have been a milestone in organizational studies; even now it is an important contribution.

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Lawyers' Ethics. By JEROME E. CARLIN. New York: Russell Sage Foundation, 1966. Pp. xxxix+ 267. $6.75.

In this compact and lucidly written book, Carlin examines the impact of the social context of private law practice on lawyers' compliance with legal ethics. Basing his analysis primarily on data collected in 1960 from interviews with 801 New York City attorneys, he shows how variations in patterns of professional-norm conformity are related to some of the structural and cultural characteristics of urban law practice.

Carlin measured norm conformity by eliciting lawyers' reactions to thirteen items, each of which depicted a hypothetical situation involving an attorney, "Lawyer A," in an ethical problem. The respondents were asked how often in the past five years they had encountered such situations, what they had done (or, for those who had not faced the problem, what they would do), and whether or not they approved of "Lawyer A's" behavior. By comparing their responses with the official standards of the bar, Carlin is able to estimate the degree to which practitioners in his sample accept professional norms and the extent to which their reported behavior conforms with these expectations. The data reveal considerable variation in the acceptance of and adherence to official norms; and they also indicate that these differences in conformity are closely linked to the stratification within the practicing bar.

Using as his major point of departure the finding that attorneys in large law firms tend to conform more than those who practice alone or in small firms, Carlin draws on a large array of data to describe, in imaginative and systematic detail, many of the conditions that support and undermine professional ethics. What emerges is a vivid description of radically different professional environments, subjecting practitioners to quite diverse kinds of pressures from clients, cases, courts, competitors, and colleagues and tending, in combination with attorneys' ethical dispositions, to largely determine their professional conduct. Professional norms, for example, have comparatively little influence in an environment where ethically unconcerned lawyers, supported by ethically permissive office mates, represent exploitable and expendable clients in the lower courts of New York.

If Carlin's analysis is anywhere near the mark, a substantial minority (he estimates 22 per cent) of all active practitioners in the New York boroughs of Manhattan and the Bronx engage in unethical behavior, though, as he points out, relatively few attorneys in New York are officially disciplined. Attributing this state of affairs, along with the uneven distribution of legal services, the unprofessional conditions of lower courts, and even the selective development of the law itself, to the system of stratification in the bar, Carlin concludes his book with several policy recommendations. First, he calls for more involvement of large-law-firm lawyers in the lower levels of the administration of justice. Second, he believes that the lower courts and agencies need to be reorganized to eliminate corrupting influences. Finally, and most important, he claims that broadening the availability of legal services would have the effect of "increasing the financial security of lawyers practicing at the lower levels of the bar, thereby strengthening their capacity to conform to legal norms."

These recommendations are probably premature. It is by no means evident that most attorneys in large law firms could readily transfer their experience and skill to cope effectively with personal injury, matrimonial, criminal, or landlord-tenant cases in local courts. Nor is it clear that simply increasing the economic security of marginal practitioners will substantially improve their ethics. I suspect that the problem of norm conformity in law practice, as in other professions and occupations, is far more complex than Carlin's suggestions imply. If we are to achieve a better sociological understanding of law practice, and especially if we are to counsel the bar regarding its ethical difficulties, we need to extend our knowledge of professional behavior far beyond what is presently available. In particular, we need to investigate further the circumstances facilitating and inhibiting the institutionalized evasion of norms, the ways in which practitioners cope with equally salient but conflicting norms, and the effects of different role orientations on the perception of and adherence to professional norms, to cite only a few examples of needed research suggested by Carlin's excellent and pioneering monograph.

HUBERT J. O'GORMAN

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The Literary Profession in the Elizabethan Age. By PHOEBE SHEAVYN. Revised by J. W. SAUNDERS. 2d ed. Manchester: Manchester University Press; New York: Barnes & Noble, 1967. Pp. x+248.

So you think you have it bad. To know what bad means, you should have been a writer in

the Elizabethan age. He had it bad. The censorship, the need to cater publicly to the vanity of the patron, the prevalent and unchecked plagiarism, the general repressive attitudes would be intolerable to English authors today. Phoebe Sheavyn documents these conditions in *The Literary Profession in the Elizabethan Age,* which first appeared in 1909 and was revised for this second edition by J. W. Saunders, although the original author, at 101 years of age, has seen the proofs.

The Elizabethan writer lived in a period of inflation marked by the growth and power of a business middle class. This financial circumstance had an effect on the literary man, since the commodity he was selling was not in great demand and his main income, which came either from a job or from his patron, was fairly well fixed. Only a few writers had lifelong patronage; the rest had to compete continuously for handouts, for most patrons had many protégés and the amounts of support they extended were limited.

Not only did the writer have to scrape continuously for his livelihood, but his fight for survival was further handicapped, for he was living in an age of suspicion. In fact, it was rare for an author to escape governmental entanglement. Four institutions monitored the writing of the day, and the Court of Star Chamber was particularly noted for its vigilance and arbitrary severity. The use of the professional informer was not uncommon, and the death penalty for writing against the establishment—either the church or the state—was a distinct possibility.

Most writers lived in London, where the publishers had their shops. The relations between many of these literary men and these Elizabethan entrepreneurs were bad. The publishers had a virtual monopoly over their clients' work; often books were "patented to particular booksellers as their sole right." The writer sold his manuscript to the bookseller, and that was where his financial reward stopped. This situation apparently led to theft, piracy and the illicit sale of manuscripts. Occasionally a playwright, in retribution, would sell his manuscript twice—once for the stage and once for the press. While during this period authors were frequently colleagues and often collaborated on plays, partially because of lack of funds, this was also a period of literary fraud. Plagiarism was common, and there was no recourse to law.

But all was not evil. The occupations at which writers worked were mainly middle class and usually attached to the court. A simple analysis of a table which appears in the Appendix of this book and which lists the major occupations of 200 authors reveals that 19 per cent were courtiers, 24 per cent were courtly satellites (a term "used to describe any man who served the Court in one or more of a group of occupations" ranging from groom to poet laureate), 10 per cent were country gentlemen and had independent means, and for only 8½ per cent was writing considered their major occupation. At least, because records are not available for all the 200 authors, 152 attended one or more universities. Elizabeth I and Henry VIII, who are listed among the authors, did not matriculate. Most of the other writers ended their days with the title of gentleman or knight.

This book was not designed for sociologists; certainly using our terminology there was no literary *profession* in the Elizabethan age. The historical evidence leaves much to be desired; the generalizations based on this evidence appear to be too broad and sometimes seem contradictory. However, despite its shortcomings, here is an interesting social history of the writer in a turbulent period, and Phoebe Sheavyn has given us a quaint, valuable book. I like it.

Erwin O. Smigel

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Tzintzuntzan: Mexican Peasants in a Changing World. By George M. Foster. Boston: Little, Brown & Co., 1967. Pp. xii+372. $6.95 (clothbound); $3.95 (paperbound).

A major feature of this book is the attempt by George M. Foster to apply his concept of the Image of Limited Good to a Mexican village he had studied first between late 1944 and mid-1946 (*Empire's Children: The People of Tzintzuntzan* [Publication No. 6; Mexico City: Smithsonian Institution, Institute of Social Anthropology, 1948]). Since 1958 Foster has made annual visits to this village.

By Image of Limited Good he means

that behavior . . . is patterned in such fashion as to suggest that Tzintzuntzenos see their social, economic, and natural unverses—their total environment—as one in which almost all desired things in life such as land, other forms of wealth, health, friendship, love, manliness, honor, respect, power, influence, security, and safety exist in *absolute quantities insufficient to fill even minimal needs of villagers.* Not only do "good things" exist in strictly limited quantities, but in addition *there is no way directly within the Tzintzuntzeno's power to increase the available supplies.* . . . Consequently, there is a primary corollary to the Image of the Limited Good: if Good exists in finite quantities, and if the system is closed, it follows that *an individual or a family can improve its position only at the expense of others.* . . . That someone's advantage implied someone else's disadvantage seems to me to be the key to understanding the Image of Limited Good [pp. 123–24].

The author proposes to test this model in several ways: first, by looking at examples of behavior; second, by noting whether cultural forms and institutions conform to types that might be logically predicted; third, by seeing whether the processes of social and cultural change occurring in the village can be explained by the model. Should the model work in Tzintzuntzan, the writer correctly assumes that it would have wider applicability to other peasant societies.

The use of examples to test the model are found in chapter vi, which is titled "The Image of Limited Good." This follows five background chapters on "The Peasant Community," "The Historical Roots," "Tzintzuntzan and Its People," "Interpersonal Relations: Family and Compadrazgo," and "How the People Behave." An excellent seamstress is unwilling to teach her fine stitches to embroiderers in other villages in return for marketing help given by Centro de Educación para el Desarrollo de la Comunidad en America Latina; an expert potter will give a six-week course to teachers at a special session three hundred miles away but will not tell the professional potters there how to use his glaze-binder; a farmer who acquired a very good maize would not share his seed with other local farmers. Friends, lovers, and children are afraid that their love objects will share the limited amount of love with another; people consider it foolish to give blood transfusions; they see domestic authority as limited to the husband and not to be shared by the wife if he is to maintain his *machismo.* Tzintzuntzenos, individualistic in attitudes and behavior, see themselves engaged in an unrelenting struggle with their fellows and the world at large for their share of scarce values. Only the nuclear family gives some degree of mutual support. Co-operation in communal activities becomes pointless, since more Good than already exists cannot be produced: indeed, co-operative efforts are personal threats. The good society, therefore, conforms to an equilibrium model which maintains the status quo and permits no significant changes in the traditional allocations of Good.

The Image of Limited Good, as already indicated, implies a closed system. But if one can show that he has gained wealth or other locally scarce values from outside—such as the *braceros* working in the United States—then he does not threaten the local equilibrium, for he has not deprived a fellow villager. Use is also made of the help of an outside patron or treasure tales explaining some unexpected rise in fortune by a family. Central to the concept is the belief in the importance of fate and luck, where hard work and thrift are moral qualities of only the slightest functional value.

In chapters vii–xi, Foster looks at structural (cultural and social) features against which to test his model. These chapters deal with "The Fear of Envy," "Government, Decision Making, and Conflict Resolution," "Health and Equilibrium," Religious Ritual: Its Latent Functions," and "The Dyadic Contract." Sociologically, the treatment of local government, religion, and the dyad are most relevant.

Part II shifts from "The Traditional Community" (Part I) to "The Changing World," which consists of seven chapters devoted to "Change: Culture, Personality, and Infrastructure," "A Population Explosion: Causes and Consequences," "Economic Development Problems," "Innovators and Innovation," "Society and Religion: The Changing Prestige System," "An Experiment in Development," and "What Is the Future?" In these chapters, Foster takes up the problems of guided social change that he has treated so effectively in his other writings. It becomes clear as he traces the changes through time in Tzintzuntzan that the Image of Limited Good, which fits a closed social system, may prove a good starting point for understanding

local behavior but that it is no longer a sufficient explanation. Like Banfield's concept of *amoral familism*, it has explanatory power but would indicate that, in their contacts with the outside world, the villagers must either accept a duality in their orientation—one approach to fellow villagers and another to outsiders—or else find themselves, as Foster suggests, hopelessly out of date in their methods of coping with the twentieth century. The applicability of Foster's model to other peasant societies has already received extensive treatment in the *American Anthropologist* and is beyond the scope of this review. But for a given time and place, in a particular village in Mexico still relatively isolated, his Image of Limited Good proves an important analytical tool.

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Education and World Affairs

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Demography of Organizations[1]

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ABSTRACT

By studying data on the Baltimore city and Baltimore county (suburban) school systems, we try to specify conditions under which an organization is able to control the social composition of the net flow of the population across its boundaries. The Baltimore city system has had a demographic goal of desegregated schools for some ten years. This paper tries to specify some of the reasons that this goal has not been achievable.

We will argue that the capacity of an organization to control its demographic composition or, to put it another way, to pursue demographic goals decreases to the degree that (*a*) its jurisdiction is defined territorially, (*b*) the jurisdictions of its subunits are defined territorially, (*c*) the size of the aggregate net flow for the system as a whole is large and in a direction opposite to that implied by the demographic policy goals, and (*d*) the size of its demographic problem is large. We will show that all of these conditions apply to the Baltimore city system in pursuing a goal of desegregation and that consequently its administrative policies have been unsuccessful.

I. INTRODUCTION

Organizations, like communities or nations, are made up of people. These people enter the population of the organization, stay awhile, and leave. Organizations grow and decline by a net flow across their boundaries. At any given time the composition of the organization will be determined by the past history of the social composition of net flows into it. In short, all of the variables of demographic theory that apply to communities also apply to organizations. Yet there is very little work on the demography of organizations. Why should this be so?

The main factor in the poverty of demographic studies of organizations is that the demographic structure is thought of as a dependent variable—as something the organization decides about. The reason that universities have more Ph.D.'s and more people of ages 17–25 than do factories is not that such people just happen to enter. Because the university wants a faculty and student body, it controls the net flow so as to maintain these bodies. Consequently, it makes more sense to study what demographic structure the organization wants, presumably determined by its goals, than to generate that structure from an analysis of net flows as community or national demographers do.

One major kind of organization in American society has formulated demographic goals which it has been unable to achieve: the public school systems of central cities. The demographic goal is a desegregated student body. But the net flows that must be managed to achieve such a desegregated school system apparently are not under the control of school administrative policy. This poses the general problem of the *degree* to which net flows of different kinds of people are, or are not, dependent on administrative policy in different kinds of organizations. In this paper we want to consider data on desegregation from Baltimore city and county school systems to illuminate the determinants of the dependence or independence of organizations' demographic structures on administrative policy.

A distinctive feature of public school systems as organizations is that their jurisdictions are defined territorially. The Baltimore city school system is restricted to the population of the territorial city. By tradition and convenience, the jurisdiction

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of elementary schools is defined territorially as a neighborhood, although that of secondary schools is, in Baltimore, defined less narrowly. As we will show, the degree of demographic independence of secondary schools because of this is considerably greater and hence much more affected by the system's demographic goal of desegregation.

The degree of control of net flows into subjurisdictions (for example, into schools as subjurisdictions of the school system) depends on the size and direction of net flows at the aggregate level of the system as a whole. In American society at the present time, the demographic goal of desegregation has been pursued almost exclusively by one kind of policy, that of introducing Negroes into white schools. We will show that the pattern of school system level of net flows (specifically, more Negro students who flow entirely into the city) is such that the central city of Baltimore has virtually run out of white schools to use for desegregation. The remaining white schools are either private or suburban. Since Negroes are not entering those white schools and since the absolute Negro school population is increasing, the effect is to push on all the white places in desegregated public schools, causing them to become resegregated. Thus the net system-level flows constrain the net flows of the subparts of the system to be all in one direction, or to be in part exaggeratedly in that direction in order for some subparts to remain steady. In short, the net flows of Negroes and whites of individual schools have to add to the net flow of Negroes and whites of the city system as a whole, as long as that jurisdiction is defined territorially. This elementary demographic fact seems not to have penetrated the analysis of desegregation of schools. Such analyses usually fail to answer the question of where to put those Negroes who will not, under the new policy, flood the desegregated school.

Given the basic constraint of territorial jurisdiction, the degree of control of the demographic structure by administrative policy depends on the size of the problem presented by the demographic composition within the jurisdiction. We will show that the suburban system has, by some criteria, desegregated more than the city system in Baltimore. But this is because the size of the problem is smaller. The size of the problem of reaching this particular demographic goal can be calculated rather precisely.

Thus we will argue that the degree of demographic independence of an organization decreases to the degree that (*a*) its jurisdiction is defined territorially, (*b*) the jurisdictions of its subunits are defined territorially, (*c*) the size of the aggregate net flow for the system as a whole is large and in a direction opposite to the direction implied by the demographic policy goals, and (*d*) the size of its demographic problem is large.

II. TERRITORIAL JURISDICTION

The more narrow the territorial jurisdiction the organization has the less likely an organization is to encounter a heterogeneous demographic structure. People do not geographically distribute themselves in a random fashion; thus the demographic structure varies by territorial units. Neighborhoods, for example, are somewhat homogeneous with respect to social class and ethnicity. Moreover, on a broader basis, ethnic groups cluster in certain types of areas of the United States, such as Negroes in southern rural and central city areas. This of course, provides the mechanism on which our first assertion rests. If a certain demographic composition is wanted, the more widely defined the territorial unit in which these characteristics can be sought, the more control the organization has over obtaining its desired structure.

This is not to say that an organization cannot achieve a desired net flow within a small or restricted territorial jurisdiction. Many organizations take advantage of homogeneous clusterings that do occur. This would be true of a kosher grocery store in a Jewish neighborhood. What it does say is that if a desired demographic goal is not

achievable within a given territorial unit but is attainable within a broader territorial jurisdiction, then the organization that is territorially restricted has limited control over its demographic structure. We will show that this is the case for Baltimore public school systems.

Nationally a desegregated structure of students in schools has been defined as desirable and necessary. Table 1 presents data for the Baltimore city school system and the Baltimore county school system which will indicate the success each has had in achieving a demographic composition within its school system which would result in the desired desegregated system. The Baltimore county system includes most of the suburbs of Baltimore in a single school district.

After approximately ten years of moving toward a specified demographic composition, both school systems have accomplished very little. Progress in desegregation is almost entirely concentrated in the city system, with Baltimore county now having a percentage of students going to desegregated schools less than the city achieved immediately after the Supreme Court decision. The progress in the city is entirely due to a decline, almost the elimination, of the segregated white school. Forty-eight per cent of all students were in segregated white environments in 1955 (not shown in Table 1);[2] by 1965 less than a fourth were in such environments. Thus the city has very few white students left in a segregated environment with which to achieve desegregation. The city has *retrogressed* in the proportion of all its students going to the most disadvantageous type of segregated school, the predominantly non-white school. The importance of these segregated non-white schools has actually increased in the city. In sum, the city system has been losing the war against segregation of Negroes, although it has made excellent use of its whites during this period to create desegregated environments.

Most of the segregated white schools are in the suburbs. Of the county student population, 92 per cent still go to segregated white schools. The reason the city has failed to make a substantial impact is obvious from the figures on the county system. The whites that might be used to create desegre-

TABLE 1

SEGREGATION PATTERNS FOR BALTIMORE CITY AND BALTIMORE COUNTY SCHOOL SYSTEMS,* 1956 AND 1965

Year	Percentage of Students Attending			Total No. of Students (Base)
	Segregated Negro Schools†	Desegregated Schools‡	Segregated White Schools§	
City:				
1956......	37.5%	16.0%	46.5%	153,206
1965......	47.9	28.5	23.6	190,926
County:				
1956......	4.9	0.4	94.8	67,280
1965......	0.8	6.9	92.3	108,646

* Special schools, such as those for mentally and physically handicapped, are excluded.

† Schools having 0–10 per cent white students.

‡ Schools having 11–89 per cent white students.

§ Schools having 90–100 per cent white students. Note that a few Negroes (less than 3 per cent of them) attend "segregated white" schools. From a social policy point of view, of course, these few Negroes should not be counted against the school system, as they are here. We will ignore them here and in Table 2.

[2] The figures for 1955 are not reported in Table 1 because Baltimore County data are incomplete for that year.

gated environments live in the county, and segregation is taking over in the city from this effect of narrowly defined territorial units for each school system.

Under the present political system of each public school system having its boundaries defined territorially, it is not possible for either school system to achieve desegregation. With a wider territorial unit of a metropolitan nature, the desired structure would be achievable for each public school system.

It can also be shown that within the city system the secondary schools are more desegregated than elementary schools. (Schools stricts the school system's ability to control the demography of elementary schools, and hence they show less progress being made in desegregation. Secondary schools, on the other hand, have a wider territorial base. (In Baltimore, the jurisdictions of high schools extend, theoretically, throughout the city.) They serve students who come from several neighborhoods and thus can be made much more heterogeneous in social characteristics. Of course, if the elementary school children now in school stay where they are, there will be no way to maintain the higher level of racial mixture in secondary schools in the future.

TABLE 2

SEGREGATION PATTERNS BY SCHOOL LEVEL FOR BALTIMORE CITY, 1956 AND 1965

YEAR	PERCENTAGE OF STUDENTS ATTENDING			TOTAL NO. OF STUDENTS (BASE)
	Segregated Negro Schools*	Desegregated Schools†	Segregated White Schools‡	
Elementary:				
1956......	40.1%	14.8%	45.1%	103,954
1965......	52.9	19.5	27.5	124,416
Secondary:				
1956......	32.2	18.3	49.5	49,252
1965......	38.7	45.1	16.2	66,510

* Schools having 0–10 per cent white students.
† Schools having 11–89 per cent white students.
‡ Schools having 90–100 per cent white students.

were classified as elementary schools if they had any elementary grades in them. All others were classified as secondary schools.) (See Table 2.)

Although there are other factors, such as degree of parental interference, influencing the demographic composition of schools by level, our contention is that, in addition, territorial jurisdiction influences the social composition of the school. Elementary schools are neighborhood-bound and much more likely than secondary schools to be composed of students with homogeneous social characteristics. There has been hesitation on the part of school officials to implement a policy of large, heterogeneous elementary schools. This re-

III. SUBJURISDICTIONS

The degree of control of net flows into subjurisdictions depends on the size and direction of flows at the system level.

The number of Negroes in the Baltimore city school system has increased from 65,000 in 1955 to 117,000 in 1965. Whites numbered 92,000 in 1955 and 74,000 in 1965. Since the main schools that accommodate these Negro students are the public schools, this constrains the subunits of the city school system to provide places for some 50,000 Negroes. Table 3 gives data for Baltimore city on the effects of this pressure on the composition of individual schools.

The only place where there has been

substantial progress is in desegregating the segregated white schools existing in 1955. Desegregated schools are those having at least 10 per cent of each race. Of the thirty-seven desegregated in the last period (of those for which we have 1955 data), twenty-nine (78 per cent) have been created out of previously segregated white schools. More than three-fourths of all the desegregated school situations in the city are the result of introducing Negroes into previously segregated white environments. Upon closer inspection we find that the change in the number of desegregated schools took place by losing nine schools from desegregated to segregated non-white and gaining two for a net loss of seven into the segregated non-white category, but gaining twenty-nine and losing none to segregated white.

During the eleven-year period, ten schools were desegregated and then resegregated. They started off with more than 90 per cent white and ended up with less than 10 per cent white.

Although the city administrative policy has been such as to create desegregated schools, the net flow into the city school system of Negroes (with a small net flow of whites out) has been large enough to cause some of these schools to become resegregated. The end result is the creation of a demography of the schools different from that aimed at by administrative policy.

Table 4 shows the same data as Table 3, but for the county system from 1956 to 1965.

TABLE 3

PATTERNS OF CHANGE IN BALTIMORE CITY SCHOOLS BETWEEN 1955 AND 1965

	Number of Schools (Panel A) (1965)			
	Segregated Negro*	Desegregated†	Segregated White‡	Total
1955:				
Segregated Negro*....	50	2	0	52
Desegregated†.......	9	6	0	15
Segregated white‡....	10	29	18	57
Total............	69	37	18	124
	Percentage of Schools (Panel B) (1965)			
1955:				
Segregated Negro*....	96.2%	3.8%	0	100%=52
Desegregated†.......	60.0	40.0	0	100%=15
Segregated white‡....	17.5	50.9	31.6%	100%=57

* 0–10 per cent white.
† 11–89 per cent white.
‡ 90–100 per cent white.

There has been only a very small net flow of Negroes into the county school system. There were 3,660 Negroes in 1956 and 3,900 in 1965. However, there has been a substantial increase in the white student population from 63,600 in 1956 to 104,700 in 1965. The data in Table 4 present the details of what this net flow at the system level meant as far as creating a desegregated county system.

In contrast to what has happened in the city system, there has been no resegrega-

tion of county schools. The one school which was desegregated in 1956 remained a desegregated school in 1965. In addition, there have been nine formerly white schools desegregated. By this criterion (see also the next section), the county has made more progress in creating desegregated subunits in the system. On the other hand, we observe that only about one-eighth of the predominantly white schools have been desegregated by increasing the proportion of Negroes as compared with half of the predominantly white schools desegregated in the city, and an additional sixth were desegregated and then resegregated (see Table 3). In sum, the county in the ten-year period has desegregated 12.2 per cent of its predominantly white schools. The city in the eleven-year period has desegregated 68.4 per cent of its predominantly white schools. We do not know what happened in the school which started black and became white, but its very existence shows that the county is subject to different forces than the city is.

These two tables present evidence of different types of structures being maintained in the two systems. The city system is desegregating all-white schools, some of which are becoming resegregated because of the net flow of Negroes into the city system. The county has maintained its all-white schools and has done considerably less, proportionally, in desegregating its white schools. The net flow across its boundaries has resulted almost exclusively in white students being added.

TABLE 4

PATTERNS OF CHANGE IN BALTIMORE COUNTY SCHOOLS BETWEEN 1956 AND 1965

	Segregated Negro*	Desegregated†	Segregated White‡	Total
	Number of Schools (Panel A) (1965)			
1956:				
Segregated Negro*	2	0	1	3
Desegregated†	0	1	0	1
Segregated white‡	0	9	65	74
Total	2	10	66	78
	Percentage of Schools (Panel B) (1965)			
1956:				
Segregated Negro*	66.7%	0	33.3%	100% = 3
Desegregated†	0	100 %	0	100% = 1
Segregated white‡	0	12.2	87.8	100% = 74

* 0–10 per cent white.
† 11–89 per cent white.
‡ 99–100 per cent white.

IV. SIZE OF THE PROBLEM WITHIN THE JURISDICTION

The above statistics have indicated very different proportions of Negro to white students for each system. Given that each public school system is a territorially defined unit, how do these different proportions affect the problem of desegregating their school system? Tables 5 and 6 give an assessment of just how difficult the problem of desegregation is for each school system. The tables report two summary measures of seg-

regation based on a determination of how far the school system is from an "ideal" racial balance in the school system. For both measures, the "ideal" is defined as an exact proportional distribution of both races throughout the system. Of course this means that city children will go to schools that are about 60 per cent Negro, while county children will go to schools that are about 4 per cent Negro. That is, the measures define desegregation as non-discrimination within the system, rather than defining it as successful racial mixtures as Tables 1–4 do.

The first summary measure is the dissimilarity index, D.[3] The second is the replacement index, R[4] which is $2Dpq$, where p is the proportion of Negroes in the school system and q equals $1 - p$. Both of these indexes of segregation measure the "size" of the administrative problem of desegregation (of non-discriminatory distribution of students *within* the system) but with different assumptions. Both measure the proportion of students that would have to be moved to achieve a perfectly balanced racial distribution. Thus D measures the percentage of the smaller ethnic group to be moved, under the assumption that the original schools can easily be expanded to take sufficient numbers of the minority to balance the proportions throughout the system; and R measures the percentage of the school population who would have to move, under the assumption that if a student is to be moved into a school he must replace another student who has moved out. Realistically, the R index is a better measure of the practical problem.

TABLE 5

SEGREGATION INDEXES FOR BALTIMORE CITY SCHOOL SYSTEM, 1955–1965

Year	Replacement Index	Dissimilarity Index
1955..........	.45	.94
1956..........	.44	.90
1957..........	.43	.87
1958..........	.42	.85
1959..........	.43	.86
1960..........	.42	.84
1961..........	.41	.82
1962..........	.40	.82
1963..........	.40	.81
1964..........	.40	.82
1965..........	.39	.82

The size of the problem for each system has changed relatively little in the last ten years according to the replacement measure R. There is, however, a vast discrepancy between the size of the problem of desegregating county schools compared with desegregating city schools. Desegregating the county school system could be achieved much more easily than it could be in the Baltimore city system. Of course, by the criteria used earlier, perfectly non-discriminatory distribution of county students would result in their all going to segregated white (less than 10 per cent Negro) schools. Segregation of Negroes in the county has never been as acute a problem as it has been in the city, in the limited sense that one could achieve a within-county non-discriminatory distribution with much less movement of students.

TABLE 6

SEGREGATION INDEXES FOR BALTIMORE COUNTY SCHOOL SYSTEM, 1956–1965

Year	Replacement Index	Dissimilarity Index
1955..........	*	*
1956..........	.10	.91
1957..........	.09	.82
1958..........	.07	.74
1959..........	.07	.71
1960..........	.07	.70
1961..........	.05	.69
1962..........	.05	.67
1963..........	.05	.65
1964..........	.05	.63
1965..........	.04	.59

* Data incomplete for 1955.

In 1965, 4 per cent of the county's school

[3] Otis D. Duncan and Beverly Duncan, "A Methodological Analysis of Segregation Indexes," *American Sociological Review* (April, 1955).

[4] Karl E. Taeuber and Alma F. Taeuber, *Negroes in Cities* (Chicago: Aldine Publishing Co., 1965), p. 30, n. 1.

population was Negro, while the city had 61 per cent Negro students. The city has had a rapidly increasing proportion of Negroes, while the county population of Negro schoolchildren has shown almost no change in absolute numbers. Because the county population of whites has been steadily increasing (in 1965 it was 1½ times that in 1956), the proportion of Negro schoolchildren attending county schools has decreased. These, of course, indicate opposite trends.

If a system has very few students of one race, then integrating them with a second race is an easy task simply because shifting a few students will achieve an integrated situation throughout the school system. A small proportion of Negroes (or whites), therefore, automatically means a small problem, regardless of the racial distribution existing within the system.

Given that a school system has a certain proportion of students of each race, then that proportion is a determiner of the size of its desegregation problems. The maximum problems of integration occur when the races are in the ratio of around 50:50. The city has remained in this maximum problem area for the eleven years of the study. In 1955 the ratio of Negro to white students was 40:60; in 1965 it was 60:40. Given the present trends, Baltimore city will soon have a "smaller" segregation problem in one sense: it is moving toward an all-Negro school system, and with few whites in the city to integrate with Negroes, there will be a decrease in the size of the "problem" of integration left for the city school system.

As far as size of the problem is concerned, just the opposite has been true in the county. The ratio of Negro to white students has always been very small and has become increasingly smaller. Thus, even though both systems are territorially restricted, there has been a difference in the progress made in desegregating their systems because of the difference in the size of the problem. The same administrative policy in each system would produce quite different results. The county can achieve, under the present conditions, the desired internal racial "balance" with only a fraction of the effort necessary for the city system to do so. It has made some of that fractional effort.

V. SUMMARY

In this paper we have addressed the general problem: under what conditions an organization can achieve a desired demographic structure. We have narrowed our focus to the discussion of three limiting conditions of demographic control.

1. Territorial jurisdiction hampers an organization's demographic control by restricting the recruitment to its ranks of certain parts of the population. If segments of the population with characteristics necessary for attaining the demographic goal are outside the range of the territorial jurisdiction, then an organization has no opportunity of securing them. We have presented data for Baltimore city and county school systems which support this first assertion. Neither the county nor the city has made much progress toward achieving a desegregated school system within their present territorial boundaries. However, given different jurisdictions, it would be possible to achieve such a demographic structure.

2. Subjurisdictions reflect the net flows into the organization at the system level. Under the condition that these system flows have demographic characteristics opposite to those needed to meet the organization's goal, then, of course, the achievement of that goal is undermined. The city has an influx of Negroes at the system level, and the consequence of this is that desegregated schools are becoming resegregated, and predominantly Negro schools are increasing. The county shows an influx of whites at the system level, the result of this being that a large number of segregated white schools have been maintained to accommodate these newly acquired whites. The

demography of both of these subjurisdictions is in the opposite direction of that of a desegregated school system.

3. The discrepancy between the desired goal and the present demographic structure, or the size of the demographic problem for an organization, is a determinant of the degree of control which an organization must exercise to achieve that goal within its presently defined jurisdiction. We have been able to assess precisely, by summary measures, the size of the problem for one specific type of demographic structure. Data for the county and city show vast differences in the degree of control necessary to achieve desegregated structures in the two systems.

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Economics, Family Structure, and College Attendance[1]

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ABSTRACT

Family economics and economic values are posited as accounting for both socioeconomic and family structural variations in college attendance. To explore these assumptions, data from 1,442 young married adults are examined. Father's occupational status is found to be highly related, and the individual's sex slightly related, to college attendance, favoring white-collar offspring and males, respectively. Three family structural factors—sibship size, sex ratio, and child space—are related to educational attainment, favoring those from smaller families, with several years separating them from their nearest siblings, and in the case of blue-collar females, those with few male siblings. Contrary to several studies, no patterned birth-order relation to college attendance appears. Methodological issues in both the present and past studies are discussed, and the economic interpretation is suggested as a basis for further research.

College attendance is a key to the dominant definition of success in American society. Families differ greatly, however, in the amount of economic resources they have at their disposal and also in the proportion they set aside for the higher education of their children. In this paper we shall argue that family economics and economic values are a crucial determinant of variations in college attendance not only by socioeconomic status but also by such family structural factors as size and birth order.

Intelligence, physiology, socialization, and aspirations, as well as economics, have been used at one time or another to explain educational attainment in the United States. Differences in physiology and socialization have been introduced primarily to explain variations in college attendance by birth order, but such variations are themselves still under debate.[2] Intelligence has been found to have an effect independent of economics, the less intelligent being less likely to attend college. However, this effect is randomly distributed *within* socioeconomic status and family structural categories. We, therefore, suggest that family economics and economic values and, as a by-product, the development of educational aspirations in the offspring, account in large measure for variations in college attendance by SES and family structure.

To explore such assumptions, the family structural factors of sibship size (number of children), birth order, sex ratio, and age space between children are introduced as variables intervening between family economics and college attendance. The three major tasks of the investigation include: (1) *presenting guiding hypotheses* regarding family structure and college attendance drawn from economic assumptions; (2) *introducing* the *variables* of sex ratio and child space along with the usual structural factors of sibship size and birth order; and (3) *criticizing* the *methods* of previous studies of birth order (ordinal position) and educational attainment, especially those of Schachter and Bayer.[3]

[1] This investigation is part of a study supported by Public Health Service fellowship (MH-15,571) from the National Institute of Mental Health, under the direction of the senior author. The authors wish to acknowledge the helpful criticisms of Anthony F. Costonis.

[2] For a discussion of physiological and socialization theories explaining differences in educational attainment by ordinal position, see Alan E. Bayer, "Birth Order and Attainment of the Doctorate: A Test of Economic Hypotheses," *American Journal of Sociology*, LXXII (March, 1967), 541–42.

[3] Stanley Schachter, "Birth Order, Eminence, and Higher Education," *American Sociological Review*, XXVIII (October, 1963), 757–68; Alan E. Bayer, "Birth Order and College Attendance," *Journal of Marriage and the Family*, XXVIII (November, 1966), 480–84.

ECONOMIC ASSUMPTIONS AND GUIDING HYPOTHESES

Families, we have said, have differing amounts of resources and allocate differing proportions of these resources for educational purposes. Working- and lower-class parents not only tend to have fewer resources at their disposal, but their values cause them to be less likely to set aside money for their children's college educations. Despite civil rights and fair employment laws, males still tend to be favored over females in our educational-occupational system. Thus, differences in educational attainment by socioeconomic status are great and by sex somewhat smaller, favoring white-collar and male offspring, respectively.

How, then, might the allocation of family economic resources determine the relation between our four family structural variables and college attendance?

1. *Sibship size.*—The larger the family, the more difficult it is for a given child to be allocated sufficient resources to go to college. In addition, the smaller the family, the more likely it is that the parents have purposely controlled the number of children for the sake of their individual achievement, including college attendance.[4] Two qualifications seem necessary: first, parents do not, of course, pay the *entire* bill for *all* of the undergraduate education of their children.[5] Thus, economic assumptions based upon resource allocation can never reduce to zero a child's chances as predicated upon sibship size. Second, the question arises as to the point at which sibship size begins to decrease a given offspring's educational opportunity. Does a middle-class child with two siblings have less chance of attending college than one with no siblings? Or does it take five children to work to the disadvantage of any one? Although some studies seem to answer the former question affirmatively, further evidence is appropriate, since intuitively the number of children would not seem likely to become a negative factor in the economically affluent middle classes until the family is rather large.

2. *Birth order.*—Four papers published during 1966–67 have yielded three divergent conclusions regarding birth order and college attendance. Two of these, duplicating or summarizing the results of earlier studies, have found a definite educational advantage for earlier-born over later-born children.[6] A third reports that when family size is held constant there are no significant relationships between ordinal position and educational attainment.[7] The fourth study indicates that the single or only child has the greatest chance of attending college, middle children have the least chance, with oldest and youngest children about equal in likelihood of college attendance when sex and socioeconomic status are controlled.[8]

These various findings are difficult to explicate from an economic theory because alternative results may be accounted for by assumptions drawn from the same theory. As Bayer points out, the firstborn may be allowed to utilize a disproportionate share of family resources allotted to education. Or later-borns may be in a position to profit from the ability of older, economically established siblings to aid in their education.[9] An intriguing finding regarding birth order and education is reported by Glen Elder. He

[4] James H. S. Bossard and Eleanor Stoker Bell, *The Large Family System* (Philadelphia: University of Pennsylvania Press, 1956), pointed out this relation between family size and achievement aspirations for their children on the part of the parents, as have other writers more recently.

[5] Bayer, "Birth Order and Attainment . . . ," *op cit.*, p. 549, notes that, while parents do bear the major responsibility for financing undergraduate educations, at the graduate level such support is relatively infrequent.

[6] Schachter, *op. cit.*; William D. Altus, "Birth Order and Its Sequelae," *Science*, CLI (January, 1966), 44–49.

[7] Ben Barger and Everette Hall, "The Interrelationships of Family Size, and Socioeconomic Status for Parents of College Students," *Journal of Marriage and the Family*, XXVIII (May, 1966), 186–87.

[8] Bayer, "Birth Order and College Attendance," *op. cit.*

[9] The alternative economic hypotheses for explaining birth-order differences in college attendance are spelled out in Bayer, "Birth Order and Attainment . . . ," *op. cit.*, p. 543.

notes that in the middle-class family the oldest child, especially if a male, tends to bear a major portion of the parents' aspirations for their offspring. However, in the working-class family the older children frequently must drop out even before completing high school in order to help the family economically. Educational advantage in the lower socioeconomic strata is thus to the later-borns. Ordinal position works inversely in higher and lower socioeconomic status families in its relation to education.[10]

While Elder's results are interesting, the divergent conclusions of the various authors, as well as the antithetical assumptions regarding birth-order effects which may be drawn from an economic explanation, make it impractical for the authors to posit a single guiding hypothesis at this point.

3. *Sex ratio.*—The effect of sex ratio as a family structural variable is predicated upon the current advantage of the male in our educational-occupational system. That is, the family with limited economic resources to allot to education tends to allocate more for the education of male offspring than for females. Thus, a daughter with few brothers is more likely to receive the opportunity for a higher education than is a daughter from the same size family, but most of whose siblings are male. The sex-ratio effect, if such exists, is a function of the number of male offspring competing for funds and would seem to be more determinative of a female's chances of going to college than a male's. Furthermore, since it is based upon a concept of limited resources, it is more likely to affect the blue-collar female than the white-collar female.

4. *Child space.*—The number of years separating children may have an influence upon educational opportunity which could be deduced solely from the economic concept of limited and allocated resources. Obviously, if two children are four or fewer years apart in age, the likelihood of their being in college simultaneously is much greater than if they are separated by five or more years. We might phrase our general expectation as follows: the greater the age separation between offspring, the greater the likelihood that a given offspring will attend college.

These, then, comprise the economic assumptions and guiding hypotheses which are central to the present study. The methods for investigating these phenomena must now be described.

SAMPLING AND METHODOLOGY

The data which form the basis of this paper were collected as part of a large kinship study carried out in Greensboro, North Carolina. The sample was drawn by means of a random sampling technique, with several restrictions upon inclusion. All the respondents are white, married, have been married only once, and for twenty years or less. Each respondent was asked about his own educational and family history, as well as about the sibling closest to him in age. Those for whom adequate information was thus provided include 779 respondents and 663 "age-near" siblings, making a total sample of 1,442 young adults with a median age just under thirty-five. Well over half of these individuals were born between 1925 and 1935, with a range from about 1910 to 1945. All those included in the analysis have completed their educational careers, the results thus being based upon fact rather than aspirations.

The dependent variable is any college attendance at all, not graduation from college. Variables to be related to college attendance include the following, in the order in which they will appear in the results: (1) father's occupational status, as indicated by the simple white-collar, blue-collar dichotomy. The former encompasses professional, managerial, sales, clerical, and technical occupations, while the latter includes skilled craftsmen and foremen, semiskilled and unskilled workers. Individuals from white-collar families number 615, while there are 827 with a blue-collar background. (2) The sex of the individual shows the sample consisting of 771 females and 671 males. (3) Sib-

[10] Glen H. Elder, Jr., *Adolescent Achievement and Mobility Aspirations* (Chapel Hill, N.C.: Institute for Research in Social Science, 1962), chap. iii.

ship size is defined as the total number of an individual's brothers and sisters, counting himself. (4) The birth-order or ordinal position of the young adult is categorized into the only or single child, oldest, next to oldest, middle, next to youngest, and youngest. (5) Sex ratio within the sibship is defined as the number of male siblings in relation to the total number of siblings. (6) Finally, child space is the number of years separating two siblings. In this analysis the space between the age-near siblings is used and is dichotomized into five or more years apart and four or fewer years.

RESULTS

Socioeconomic status, sex, and college attendance.—A crucial determinant of educational aspirations, and therefore attainment, in American society is the occupational status of one's father, since this symbolizes the family's economic achievement values. The great disparity in college attendance which is based upon socioeconomic status is apparent in the 76 per cent of white-collar offspring in our sample who attended college, as compared with 27 per cent of the blue-collar.

When educational attainment by sex is examined, controlling for the father's occupational status, the results are once again consistent with previous literature. Within each status category, the male is slightly more likely to continue his education beyond high school than is the female. Percentages are 78 for white-collar males and 74 for the females, 29 for blue-collar males and 25 for the females. An important difference between males and females which is obscured by simply considering *any* school attendance beyond high school involves the completion of a four-year college degree. In this case, the percentages favoring the male are close to 15 rather than 4.

Sibship size.—Table 1 shows college attendance variations according to the first of our family structural variables, sibship size. The effect of size as a limiting factor on the higher education of certain offspring varies according to sex and occupational status. In white-collar families the detrimental effects of size do not appear until the female is one of four siblings, or the male one of five. From these sizes on, each additional sibling appears to make it less likely that a given one will attend college. At the smaller sizes, factors other than size would be necessary to explain why about one in six white-collar offspring does not continue his or her education beyond high school.

TABLE 1

COLLEGE ATTENDANCE BY SIBSHIP SIZE, CONTROLLING FOR SEX AND FATHER'S OCCUPATIONAL STATUS

Sex, Father's Occupational Status, and Sibship Size	Number of Individuals	Percentage with Some College or More
White-collar males:		
1	16	81%
2	101	86
3	60	78
4	47	89
5	29	62
6+	30	33
Blue-collar males:		
1	19	47
2	64	39
3	62	29
4	69	35
5	52	29
6	36	28
7+	86	13
White-collar females:		
1	43	86
2	108	80
3	83	82
4	39	69
5	21	57
6+	31	42
Blue-collar females:		
1	20	40
2	70	30
3	70	30
4	69	33
5	72	17
6	55	20
7+	85	16

In blue-collar families the only child seems to have an educational advantage, regardless of sex, with the next clear break coming between four and five for the females and not until seven or more for the males. In the moderately large-sized family—five or six blue-collar and four or five white-collar

TABLE 2

COLLEGE ATTENDANCE BY ORDINAL POSITION, CONTROLLING FOR SEX, FATHER'S OCCUPATIONAL STATUS, AND SIBSHIP SIZE

Sex, Father's Occupational Status, and Sibship Size	Ordinal Position	Number of Individuals	Percentage with Some College or More
White-collar males:			
1..........	Only	16	81%
2..........	Oldest	53	85
	Youngest	48	88
3..........	Oldest	20	80
	Middle	29	76
	Youngest	11	82
4..........	Oldest	13	92
	Next	16	88
	Next	14	93
	Youngest	4	75
5+........	Oldest	7	29
	Next	20	60
	Middle	12	50
	Next	13	38
	Youngest	7	43
White-collar females:			
1..........	Only	43	86
2..........	Oldest	52	81
	Youngest	56	79
3..........	Oldest	26	77
	Middle	40	85
	Youngest	17	82
4..........	Oldest	8	63
	Next	14	86
	Next	8	75
	Youngest	9	44
5+........	Oldest	9	78
	Next	10	30
	Middle	13	38
	Next	12	42
	Youngest	8	63
Blue-collar males:			
1..........	Only	19	47
2..........	Oldest	35	43
	Youngest	29	34
3..........	Oldest	23	22
	Middle	27	37
	Youngest	12	25
4..........	Oldest	23	35
	Next	23	26
	Next	15	40
	Youngest	8	50
5-6........	Oldest	17	41
	Next	25	20
	Middle	15	27
	Next	18	28
	Youngest	13	31
7+........	Oldest	10	20
	Next	19	21
	Middle	24	0
	Next	27	11
	Youngest	6	33

TABLE 2—*Continued*

Sex, Father's Occupational Status, and Sibship Size	Ordinal Position	Number of Individuals	Percentage with Some College or More
Blue-collar females:			
1..........	Only	20	40
2..........	Oldest	32	28
	Youngest	38	32
3..........	Oldest	22	36
	Middle	39	28
	Youngest	9	22
4..........	Oldest	12	33
	Next	21	29
	Next	19	26
	Youngest	17	47
5-6..........	Oldest	21	19
	Next	35	17
	Middle	27	22
	Next	29	17
	Youngest	15	13
7+........	Oldest	4	25
	Next	15	20
	Middle	32	22
	Next	21	10
	Youngest	13	8

children—there appears to be substantial discrimination in favor of the male children receiving the higher education.

To what extent does sibship size influence college attendance? The over-all pattern is an inverse relation between size and college attendance. This effect is great enough that at the size extremes it overrides or equalizes socioeconomic status in determining whether a given offspring will attend college. That is, the blue-collar "only" male is more likely, and the blue-collar male from a two, three, or four-child family is as likely, to go to college as a white-collar male who is one of six or more children.

Birth order.—We come now to the much-studied and controversial variable of ordinal position. At this point we control sex, socioeconomic status, and sibship size. The results presented in Table 2 reveal no consistent relationship between ordinal position and college attendance. The rank ordering of the various positions within the three control variables appears to be virtually random. Thus, we agree with Barger and Hall, and disagree with the conclusions of Schachter, Bayer, Elder, and others. To this

disagreement we shall return in the Discussion section below.

Sex ratio and child space.—Previous research has discussed neither sex ratio nor child space in their relation to educational attainment. However, our economic assumptions included the assertion that males have first claim upon family resources allotted for education, with the expected result that an individual, particularly a lower socioeconomic status female, should be less likely to attend college if there are several male siblings than if there are few or none. This effect, based upon the number of male siblings within a given sibship size, is manifested in the Greensboro sample only among the blue-collar females (see Table 3). Throughout this table we observe a consistent and considerable sex-ratio effect, in addition to that based upon sibship size. As the number of brothers increases, college attendance by the blue-collar females decreases in prevalence. In view of the fact that the assumptions underlying a sex-ratio effect include limited resources, it should not be too surprising that white-collar females manifest no such pattern.

TABLE 3

COLLEGE ATTENDANCE OF BLUE-COLLAR FEMALES BY NUMBER OF MALE SIBLINGS, CONTROLLING FOR SIBSHIP SIZE

Sibship Size	Number of Male Siblings	Number of Individuals	Percentage with Some College or More
1.........	0	20	40%
2.........	0	31	45
	1	39	18
3.........	0	20	50
	1	33	18
	2	17	29
4.........	0–1	33	39
	2–3	36	28
5.........	0–1	29	17
	2	25	16
	3–4	18	17
6.........	0–2	33	27
	3–5	22	9
7+.......	0–2	15	27
	3–5	59	17
	6–7	11	9

The second factor which Bayer and others have stated to be worthy of analysis is the effect of child space on educational attainment. This is once again predicated upon the importance of family economic resources and their allocation, since closely spaced siblings would be competing for monetary support more than siblings separated by a greater number of years. This guiding hypothesis is investigated using only the 344 individuals who are from two-child families, and dichotomizing the age space into 5+ and 4− years.[11] It is evident in Table 4 that within each sex and status category the individual whose sibling is five or more years separated from him is more likely to have attended college. The significance of such results may actually be that with each additional year separating two offspring the chances of either or both attending college increase.

TABLE 4

COLLEGE ATTENDANCE OF INDIVIDUALS FROM TWO-CHILD FAMILIES BY AGE SPACE BETWEEN SIBLINGS, CONTROLLING FOR SEX AND FATHER'S OCCUPATIONAL STATUS

Sex and Father's Occupational Status	Years Different in Age	Number of Individuals	Percentage with Some College or More
White-collar males...	5+	30	97%
	4−	72	83
White-collar females..	5+	41	86
	4−	67	76
Blue-collar males....	5+	27	44
	4−	37	35
Blue-collar females...	5+	24	33
	4−	46	30

DISCUSSION: BIRTH-ORDER STUDIES

The results of the present study manifest a virtually random distribution of college attendance according to ordinal position within categories of sex, socioeconomic

[11] The reason for limiting analysis to the two-child families is that very few of those from a 3+ child family are five or more years separated from their nearest sibling. However, the same general results occur within the larger family sizes, likelihood of college attendance being greater among those separated by at least five years from any sibling.

status, and sibship size. This finding differs radically from the conclusions of several important studies, including those of Schachter and Bayer. Schachter's study, supported by Altus, found that, within categories of sibship size and sex, firstborns were overrepresented in his college sample.[12] Bayer controlled the additional variable of socioeconomic status and did not restrict himself to college students, as Schachter and Altus had done. Rather, he observed the percentages of high school seniors in a given year who continued their education beyond high school graduation. In his most comprehensive table he noted that only children have the greatest educational opportunity, middle children the least, with oldests and youngests in between.[13] By analyzing our data as did Schachter and Bayer, and by comparing these results with those of Table 2, we shall attempt to resolve some of the disagreements concerning birth-order effects.

Without presenting a table structured as did Schachter, our data analyzed as his are manifest no pattern favoring early-borns over later-borns. Using only the 688 individuals who attended college, we find a 12 per cent advantage for the *youngest* child in the two-child family, a slight advantage for the oldest in the three-child family, and virtually no differences by ordinal position from the four-child family on. Our data simply do not support Schachter's findings and interpretations.

How might we explain the divergence of our results from those of Schachter? Two possible explanations for his findings may be postulated: (1) upgrading of the occupational structure; and (2) cohort effect. From 1920 to 1950 an over-all upgrading of the occupational structure in terms of the white-collar, blue-collar dichotomy has been noted.[14] Kahl labels this "technological mobility." At any point in time, the later one's birth-order position, the older his parents are apt to be. In addition, due to technological mobility, the older one's parents, the more likely it is that their occupational status is (or was) blue-collar. We have seen that the likelihood of an offspring of blue-collar parents attending college is much less than that for white-collar offspring. Thus, the overrepresentation of firstborns in college in previous, cohort-based studies may be partially a consequence of the gradual upgrading of the occupational structure in recent decades, since firstborns at a given point in time are more apt to have white-collar parents than are later-borns in the same sample.

Even though Schachter made numerous attempts to eliminate the influence of the complex of demographic factors which might have contaminated his results, it is possible that there is spuriousness in addition to that resulting from technological mobility. For example, if for some reason, perhaps a differential fluctuation in fertility rates, the proportion of firstborn offspring during the World War II baby boom favored the white-collar groups, this would help to explain Schachter's results. Comparing the proportions in various ordinal positions attending college in a given year, such as 1961, to the proportion of early-borns born in 1942 may not be a complete enough control. We would need to know what percentage of earlier-borns in 1942 are represented in each socioeconomic category. In short, the complexity of demographic factors affecting cohorts may not have been satisfactorily handled in Schachter's study.

In Bayer's study many of the problems which have plagued previous birth-order studies have been taken into account. However, in an insightful commentary, Hermalin has pointed out the cohort and demographic complexities which Bayer has failed to control.[15] In addition to this critique, it seems to us that further methodological difficulties

[12] Schachter, *op. cit.*

[13] Bayer, "Birth Order and College Attendance," *op. cit.*, p. 483, Table 3.

[14] Joseph A. Kahl, *The American Class Structure* (New York: Rinehart & Co., 1960), pp. 254–56.

[15] For this critique and Bayer's response, see Albert I. Hermalin, "'Birth Order and College Attendance': A Comment," and Alan E. Bayer, "Reply to Mr. Hermalin," *Journal of Marriage and the Family*, XXIX (August, 1967), 417–23.

are causing our results to diverge considerably from his. We shall, thus, begin by presenting our data in Table 5, using the methodology which Bayer employed in his key table. The subsample consists of the 1,055 high school graduates only, and the relation between birth order and college attendance may be observed while sex and father's occupational status are being controlled.[16] When the Greensboro data are structured as Bayer's were, the results are very similar to his: only children are most likely to attend college and intermediate children least likely. How, then, is the divergence of Table 5 from Table 2 to be explained?

Two primary explanations for the differences between these two tables, or between Bayer's and our results, are offered: (1) the structure of Bayer's table; and (2) the omission from his analysis of those who did not complete high school—due to his use of high school seniors. Bayer did not separate birth-order positions while controlling for sibship size, as he claimed. Rather, he combined firstborn regardless of sibship size, as well as intermediate and last-born. When our data were analyzed in Table 2 with sibship size controlled, the intermediate child's disadvantage disappeared.

How did not controlling for sibship size affect Bayer's findings? It is quite obvious that there are no intermediate children in the two-child family, but there are an oldest and a youngest. Consequently, by not separating out two-child families, Bayer allowed sibship size, which favors offspring with fewer siblings, to make the intermediate child appear disadvantaged. Yet, even when oldests and youngests of two are removed from Table 2, the intermediates still are less likely to have attended college. This result, however, is still spurious and can be explained as follows: regardless of sibship size there is always just one oldest and one youngest child per family. In any random sample each higher sibship size is apt to be represented by a smaller number of individuals. This means that oldest and youngest children are overrepresented in the *smaller* sibship sizes, for example, three or four. In the case of the intermediates, however, each increase in sibship size adds one more intermediate offspring to those available for

TABLE 5

PERCENTAGE OF HIGH SCHOOL GRADUATES ATTENDING COLLEGE BY ORDINAL POSITION CONTROLLING FOR SEX AND FATHER'S OCCUPATIONAL STATUS

Sex and Father's Occupational Status	Ordinal Position							
	Only (1 of 1)		Firstborn (1 of 2, 3, 4+)		Intermediate-born (2 of 3, 3 of 4, etc.)		Last-born (2 of 2, 3 of 3, 4 of 4, etc.)	
	Number of Individuals	Percentage with Some College or More	Number of Individuals	Percentage with Some College or More	Number of Individuals	Percentage with Some College or More	Number of Individuals	Percentage with Some College or More
White-collar males	14	93%	85	88%	92	78%	65	89%
Blue-collar males	16	56	68	54	107	40	53	43
White-collar females	39	95	89	83	83	78	83	81
Blue-collar females	13	62	53	45	137	36	58	43

[16] The only difference between the structuring of our Table and Bayer's Table 3 is that we dichotomize occupational status so as not to reduce the number of cases per cell too much, while Bayer divided his sample into four socioeconomic status categories.

sampling. Thus, the probability of intermediate children from larger families being included is increased. When all children other than first- and last-born are grouped together and labeled "intermediate," they tend to represent the *larger* sibship sizes and thus appear spuriously disadvantaged educationally. Bayer's findings regarding intermediate children are largely a function of his categorical divisions and tabular structure, which obscure sibship size effects.

A second, but much more tentative, explanation for Bayer's results centers in his use of high school seniors. This, of course, automatically eliminates all those teen-agers who dropped out of school prior to twelfth grade (27 per cent of our sample). At least in this sample there is a slightly greater tendency for intermediate children, especially in blue-collar families, to stop their education with high school graduation than is true of oldests and youngests. That is, oldests and youngests are somewhat more likely to drop out prior to, or continue after, high school graduation. It may be that oldests drop out to help support the family, as Elder indicates. But whatever the reason for this, by using high school seniors the differential effects of earlier dropout are unaccounted for in figuring the percentages attending college. However, our differences in this regard are small enough as to require further evidence on high school dropout rates by ordinal position.

The general conclusion must be that both Schachter's and Bayer's findings of birth-order differences in college attendance may be attributed to the sampling and methods of analysis which they employed. Ordinal position does not appear crucial in determining one's chances for a higher education.

SUMMARY AND CONCLUSIONS

In a random sample of young, white, married adults in Greensboro, North Carolina, and their age-near siblings, we have investigated the relationship of several family structural variables to college attendance in order to explore an economic explanation for differential educational chances. The major results include the following:

1. The occupational status of the father is the most pervasive influence, strongly favoring the offspring of white-collar parents.
2. Sex differences in college attendance slightly favor males, although percentage differences are more dramatic when completion of four years of college is considered.
3. Sibship size is an important factor related to educational chances, those from large families being substantially less likely to have attended college than those from smaller families.
4. No consistent pattern of birth-order differences is manifested when the appropriate controls for sex, socioeconomic status, and sibship size are used.
5. Sex ratio, or the number of males in a given sibship size, is related to college attendance only in the case of blue-collar females, with each additional male sibling appearing to lessen her chances of attending college.
6. Child space, or the number of years separating siblings, has the effect in the two-child family of limiting the chances of either or both siblings attending college when they are close together in age. This effect is less pronounced among blue-collar females than among the other sex-stratum categories.

At the beginning, several economic assumptions were postulated as likely to explain family structural variations in college attendance. Over and above the importance of differential motivation or ability in the case of particular individuals, it seemed that the amount and utilization of family resources should have a great effect upon college attendance. While it was not possible, given the character of our data, to test these economic assumptions directly, the results appear most readily explicable in such terms. First, an important element in the white-collar, blue-collar dichotomy, which is so crucial to educational opportunity, involves values governing, and access to, financial resources. Second, the blue-collar female, who among our sex-stratum categories is in

the worst bargaining position for family resources, is greatly affected by the number of her male siblings. Finally, among the family structural variables investigated, the most difficult to predict or explain by economics seemed to be birth order. Yet this is the one variable which showed no patterned relation to college attendance in our sample. Family size, sex ratio, and child-space effects are all difficult to explain in a framework other than an economic one.

The foregoing argument, which our data support, does not invalidate physiological or socialization hypotheses for explaining some aspects of human behavior and personality, but it does call into question these other orientations in explaining family structural variations in college attendance in American society. This study in no sense completes the task of specifying the factors which are related to college attendance. It does point to important methodological considerations and posits an economic hypothesis within which other factors, such as motivation and intelligence, operate. A larger and non-localized sample would be invaluable in making possible a multivariate analysis involving our four family structural variables. Furthermore, direct questions—even in retrospect—regarding the reasons for one's level of educational attainment are clearly needed if we are to move from assumptions and guiding hypotheses to the testing of an economic theory of educational opportunity.[17]

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[17] We are currently carrying out such a study with a larger and non-localized sample, using retrospect and seeking reasons for educational level.

Discrepancy in Socioeconomic Level of Aspiration and Perception of Illegitimate Expediency

Wan Sang Han

ABSTRACT

This paper provides a more rigorous and definitive test of the relationship of aspiration level and the perception of illegitimate opportunity than has yet been provided. It takes account of two kinds of distinctions: first, the multidimensionality of aspiration level and second, the difference between level of wish (ideal aspiration) and level of expectation (realistic aspiration). The latter distinction is a familiar one in the research literature. The former distinction refers to the fact that socioeconomic level of aspiration has different attributes—for example, improvement in material possessions, high educational attainment, or higher occupational achievement. This distinction has not been adequately recognized in the research literature on adolescent aspirations. When family status, sex, and school status are alternatively controlled, the original association between SELA (socioeconomic level of aspiration) discrepancy and perception of illegitimate expediency strongly persists. This suggests that adolescents living in a mobile society might become prone to deviate from legitimate norms in attaining cultural goals not only because their family status is relatively low but because they have a severe amount of SELA discrepancy.

I. THE PROBLEM AND PURPOSE

It has seldom been noticed that the aspiration of adolescents or their socioeconomic level of aspiration is not unidimensional. Socioeconomic level of aspiration (hereafter, SELA) is not unidimensional in two ways: an adolescent is ambitious for different accomplishments, such as improvement in material goods or income on the one hand and in an educational achievement on the other. In other words, different attributes of social class such as income and education are aspired to by adolescents, and as a consequence there can emerge an inconsistency between these attributes. Second, adolescents, especially those in the stage of late adolescence, may aspire to achievements on different dimensions. At least two dimensions may be differentiated: realistic aspect and ideal facet. Another way of saying this is that limited or condition-bound dimension of SELA and unlimited or condition-free dimension of SELA can be entertained by an adolescent. The discrepancy between these two different levels is treated as an independent variable in the present inquiry.

If one takes a bi-dimensional view of SELA, the chief problem is an adolescent's reaction to the *discrepancy* between different aspects of SELA. The present study suggests that a pronounced SELA discrepancy is likely to be a source of adolescents' frustration and to give rise to distinctive reactions which are not easily predictable simply from a knowledge of the socioeconomic status of the family.[1] The hypothesis to be tested in this research is that adolescents' SELA discrepancy is positively associated with their perception of illegitimate expedient norms.

II. THE SOCIOCULTURAL SOURCE OF SELA DISCREPANCY

Two concepts are needed in order to understand the problem of "how this discrepancy can occur in a mobile society."

[1] This theoretical thinking is based on two postulates: (1) Adolescents living in a highly mobile society with strong cultural emphasis on success and equal opportunity strive to maximize their satisfaction by excelling their father's (or family's) achievement; and (2) adolescents who become increasingly aware of the toughness of reality by perceiving either closed opportunities or the limitations of their

The notion of anticipatory socialization supplies a clue to the occurrence of lofty ideal aspirations among lower-class adolescents. The prevalence of a low level of realistic aspirations among lower-class youth, on the other hand, can be understood by the concept of limitations awareness. Therefore SELA discrepancy may be viewed as a function of dynamic interaction between anticipatory socialization and perception of limitations: when working-class adolescents (or adolescents of minority groups) are so actively engaged in anticipatory socialization that their weakened awareness of limited opportunity does not prevent them from desiring lofty careers, then there is the possibility that the level of ideal SELA is higher than that of realistic SELA.[2] In other words, anticipatory socialization evokes and stimulates unrealistic ideal aspirations and in consequence creates SELA discrepancy.[3]

III. VARIABLES AND PROCEDURE

For the index of SELA, three attributes were chosen: the subjects' aspired *occupation*, his aspired level of *education*, and his aspired *material achievement*.[4] This index is analyzed at the two different dimensions. On the realistic dimension, the subjects were asked to name their expected future occupation, level of schooling, and level of material comforts. On the ideal dimension, they were asked to name their wished-for occupation, education, and material comforts.[5] There are altogether seventeen response sets of SELA: four under education,

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own ability are likely to confront a dilemma in which their striving for mobility is incompatible with their realistic perception of obstacles.

[2] It might well be argued that anticipatory socialization results in both realistic SELA and ideal SELA. Except from different sources, in other words, one can be socialized in both.

[3] The hypothesis that SELA discrepancy is closely related to perception of limited opportunity and self-ability was tested and confirmed by the author (Wan S. Han, "Levels of Aspiration and Perception of Opportunity," unpublished thesis, Emory University Library, 1964, pp. 83–87).

[4] The previous measure of SELA is rather simple and crude. This simplicity is assumed to be due to the fact that the researchers passed unnoticed the bi-dimensional nature of aspiration. For even those who recognized this bi-dimensionality, their measures remain simple. For instance, Morland, in his study of blue-collar people's aspirations, used the following items: "How much more schooling would you like to have?" and "How much more schooling do you think you will actually be able to get?" The former is made to elicit ideal aspiration of education, while the latter, realistic aspiration of it. Two criticisms can be made of the items of this sort. (1) With such questions, it is felt to be difficult to evoke and stimulate adolescents' ability to discern two different dimensions of aspiration. (2) Each attribute of aspiration (occupation, education, and material comforts or income) is treated separately. In this sense, there has been no index of socioeconomic level of aspiration. The present study takes into account these criticisms in constructing a measure of SELA. In order to sensitize adolescents' discernibility, some sort of preparatory remarks are made before the items: "Suppose you are living in an ideal society where any young man with ambition can get ahead, under this ideal condition. . . ." The subjects were reminded to answer each item at the unconditional level by repeating the phrase, "under the same ideal condition." Preparatory remarks to evoke the respondents' conditional aspiration are: "Now, considering realistically your actual circumstances or limitations such as financial position of your family and also considering your own ability such as I.Q., or school grade, then under these real life situations. . . ." The respondents were also reminded to answer each item at the realistic level by repeating the phrase, "under the same real circumstance." The present inquiry does not treat each attribute of socioeconomic status separately. In constructing the SELA index, I used Warner's method of multiple-item indexes (J. K. Morland, "Kent Revisited: Blue Collar Aspirations and Achievements," in A. B. Shostak and W. Gomberg [eds.], *Blue Collar World: Studies of American Worker* [Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964], pp. 134–43; W. L. Warner *et al.*, *Social Class in America: A Manual for Procedure for the Measurement of Social Status* [Chicago: Science Research Associates, 1949], chaps. viii–xiv).

[5] It is a moot question whether the attributes of socioeconomic status should be regarded to be equally significant. When one is considered to be differentially significant, the problem of assigning weights is another moot question. Since these three components are considered not to be equally significant as indicators of social class position (even though it is not known how much more significant is that occupational item, for instance, than other items), they are given different weights to be used in computing the final score. Occupation, which is generally considered as the functionally significant

ranging from "no schooling beyond high school" to "graduate school or professional school"; seven response sets under occupation,[6] ranging from "manual worker" to "large business owner and official"; and six under material accomplishment, ranging from "two- or three-room house and used low-priced car" to "two or more large houses and several top-priced cars." Each response set is given one point, and thereby the sources of SELA range from 3 to 17.

The SELA discrepancy was obtained by subtracting the score of expected SELA from that of wished-for SELA. The following equation clearly shows the nature of

TABLE 1

SCORE OF SELA DISCREPANCY

Level	Score
High SELA discrepancy........	5 or greater
Middle SELA discrepancy......	3–4
Low SELA discrepancy.........	1–2
Non-discrepancy..............	0

this discrepancy: SELA discrepancy = score of wished-for SELA − score of expected SELA.

In order to understand the procedure of measuring the discrepancy score, an illustration of extreme cases is helpful. If an adolescent expects "no schooling beyond high school," "manual worker," and "two- or three-room house and used low-priced car," then his score of expected SELA is 3. Suppose that the same adolescent *wishes* to finish "graduate or professional school," to be a "large business owner or high official," and to achieve "two or more large houses and several top-priced cars," then his score of wished SELA is 17. His score of SELA discrepancy is equal to $17 - 3 = 14$. The score of 14 is the maximum score of SELA discrepancy. That is, the score of SELA discrepancy ranges from 0 (no discrepancy) to 14. The categorization of the discrepancy score into three levels is shown in Table 1.

Our dependent variable, perception of illegitimate expediency, bears a close parallel in its implication to Merton's theory of anomie, particularly his "innovation." Innovation, according to Merton, is a mode of individual adaptation occurring "through the use of institutionally prescribed but effective means of attaining at least the simulacrum of success-wealth and power.[7] In this sense, the dependent variable is the adolescent's attitude toward deviation from socially legitimate norms or means in achieving the cultural goal.

Five statements are used for measuring the dependent variable. In selecting and qualifying these statements, one's attitude toward deviation from socially acceptable norms is considered in the general and abstract context as well as in the specific and concrete context. Two statements are used to measure the dependent variable in the general level: (1) Good ends often justify bad means;[8] and (2) you are almost forced to do something which is not right in order to get ahead quickly in the world. The other three statements are employed for gauging the perception of illegitimate expedient norms with reference to specific context:

role in modern industrial society, is arbitrarily given a range of 7; material comfort, a range of 6; and education, a range of 4.

[6] Turner, after assessing the usefulness of previously used schemes of occupational classification, such as Edward's scale, the North-Hatt's rating, Warner's scheme, and Hollingshead's classification, sets up six criteria according to which nine major groups of occupation are classified. The first three groups are collapsed, since they are manual workers. Another reason to collapse the first three groups is that high school seniors hardly ever aspire to be manual workers, even in the conditional level (see Ralph Turner, *The Social Content of Ambition: A Study of High School Seniors in Los Angeles* [Chicago: Science Research Associates, 1964]), pp. 245–51.

[7] R. K. Merton, *Social Theory and Social Structure* (Glencoe Ill.: Free Press, 1961), p. 141.

[8] In the pretest, the statement was: "The end often justifies the means." I was frequently asked by the respondents to paraphrase it. This original statement is, thus, judged to be too ambiguous in its connotation to elicit a prompt response. In order to make the statement clear, a qualification was made: "good end" for "the end," and "bad means" for "the means," on the assumption that culturally valued goals of success are good ends while socially proscribed means are bad.

(1) Having "pull" is as important as ability and hard work in getting *prestigious government jobs*;[9] (2) some degree of "apple polishing" is required in order to get *job promotion*; and (3) one can be successful in his *business* without compromising moral principle.

Each statement represents a five-point scale, ranging from "strongly agree" to "strongly disagree." The weight of 5 is given to a response set which favors the most illegitimate means and the weight of 1 to a response set which least favors the illegitimate means. The scale scores range from 5 to 25. The two levels of the range are demarcated by the midpoint of the range from 5 to 25 as shown in Table 2.

Four hundred and sixty-five subjects were selected in 1966 from three high schools which are located in the metropolitan area of Atlanta. Because of the requirements of the Atlanta Public School System, it was necessary to select schools according to the census tracts which are ranked in terms of socioeconomic index. Each school so selected represents a concentration of the following three strata in their attendance areas: (1) working class, (2) lesser white collar, and (3) managerial and professionals. The relationship between schools and family occupational status is shown in Table 3.

TABLE 2

SCORE OF ILLEGITIMATE EXPEDIENCY*

Level	Score
High	15–25
Low	5–14

* The average of five discriminative power scores is 2.01, ranging from 1.68 to 2.81.

TABLE 3

SCHOOLS AND FAMILY OCCUPATIONAL STATUS

OCCUPATIONAL STATUS	SCHOOL			
	A*	B	C	N
High†	90 (60.4)‡	12 (7.8)	0 (00.0)	102 (25.1)
Middle	51 (33.6)	69 (45.1)	18 (17.5)	138 (33.9)
Low	9 (6.0)	72 (47.1)	85 (82.5)	166 (41.0)
Total	150 (100.0)	153 (100.0)	103 (100.0)	406 (100.0)

NOTE.—$\chi^2 = 220.17$; d.f. $= 4$; $p < .001$.

* A stands for high-stratum school, B for middle-stratum school, and C for low-stratum school.

† High refers to managerial (large business) and professionals, Middle to lesser white collar, and Low to manual workers.

‡ Number in parentheses indicates percentages.

The χ^2 value between schools and family educational status is 171.63. There is ample justification on the basis of this evidence to assume that the three schools represent three different social strata.

In each school, all seniors were given the questionnaire.[10] Fifty-seven seniors, however, were excluded because of their reli-

[9] In the pretest form, the statement was: "Having pull is more important than ability and hard work." The discriminative power score is very low (less than 1). The phrase "more important" is modified into "as important as." The discriminative power score is 1.86.

[10] The subjects of the present inquiry are restricted to high school seniors on the assumption that rather accurate and conscious perception of opportunity structures may not appear until mid- or late adolescence.

gious background. The subjects were restricted to white Protestant high school seniors living in Atlanta. Those who were excluded are either Catholics or Jews.[11]

IV. RESULTS

The present analysis for testing the influence of SELA discrepancy on perception of illegitimate expedient norms proceeds by the following two steps:

1. The original relationships between the independent and dependent variables were first assessed.
2. The relationships between SELA discrepancy (the independent variable) and perception of illegitimate expediency (the dependent variable) with the three test factors held constant were then analyzed. The test factors were family occupational status, school, and sex.

The original association between SELA discrepancy and perception of illegitimate expediency is provided by Table 4, which shows that the latter is positively and significantly related to the former as predicted in the hypothesis.

Almost two-thirds of the students who report a high level of SELA discrepancy also fall on the high end of the illegitimate expediency scale. Only 5 per cent of those who do not report any discrepancy fall on the high end of the illegitimacy scale. This strong relationship persists when the effects of three test factors are alternatively partialed out.[12]

The data bearing on the partial relationship of illegitimate expediency with SELA discrepancy are given in Table 5, where scale scores are given separately for sex groups. It is clear that partialing out the effects of sex does not reduce the original differences of SELA discrepancy in illegitimate expediency which were shown in Table 4. Table 6 shows that partialing out the effects of school status *specifies* somewhat the original association. That is, the partialing

TABLE 4

SELA DISCREPANCY AND PERCEPTION OF ILLEGITIMATE EXPEDIENCY

Illegitimate Expediency	SELA Discrepancy*				
	High	Middle	Low	Zero†	Total
High	97.4	84.9	65.7	32.7	66.9
Low	2.6	15.1	34.3	67.3	33.1
Total	100.0 (78)‡	100.0 (86)	100.0 (134)	100.0 (110)	100.0 (408)

Gamma = .73§

* The figures shown in the present study indicate percentage.

† "Zero" refers to zero score of SELA discrepancy, i.e., no discrepancy.

‡ The number in parentheses indicates the number of cases from which percentages are computed.

§ If we use Costner's interpretation of gamma, over two-thirds of the variance in illegitimate expediency can be explained by SELA discrepancy.

[11] The present study relies on the non-random nature of the sample of students. That is, three high schools were purposively selected according to the socioeconomic status of census tracts. This type of sampling is one of the factors which might reduce the dependability and generality of the findings.

[12] Three test factors—family status, sex, and school status—are not simultaneously controlled due to relatively small size of the sample. Thus, it is not known whether the joint effects of the three test factors would have been sufficient to eliminate the original relationships even though alternative control for each test factor does not substantially shift the original association.

results in a slight reduction of the original association for the upper-class students, while it increases the original relationship among middle-class seniors. Despite this specification, it is clear that there persists a positive and significant association between SELA discrepancy and illegitimate expediency.

The strong prediction power of the SELA-discrepancy variable in accounting for perception of illegitimate expediency remains unchanged when the effects of family status are partialed out. The data provided by Table 7 clearly show that, regardless of interclass differences in illegitimate expediency, almost two-thirds of the variance in illegitimate expediency can be explained by SELA discrepancy if we use Costner's interpretation of gamma.[18]

On the basis of the above evidence it can be concluded that adolescents' SELA discrepancy is a strong predictor of their acceptance of illegitimate expediency in attempting to achieve the cultural goal of success.

[18] H. L. Costner, "Criteria for Measures of Association," *American Sociological Review*, XXX (1965), 341–53. It is clearly revealed that there is also a significant relationship between SELA discrepancy and family occupational status. The value of gamma is .12, and the relationship is significant at the 1 per cent level.

TABLE 5

HIGH DEGREE OF ILLEGITIMATE EXPEDIENCY PERCEPTION AND SELA DISCREPANCY WITH SEX HELD CONTANT

Sex	SELA Discrepancy					Gamma
	High	Middle	Low	Zero	Total	
Male	96.1 (52)*	82.9 (35)	71.4 (56)	36.8 (68)	68.2 (211)	.74
Female	92.3 (26)	86.3 (51)	61.5 (78)	26.2 (42)	64.5 (197)	.72
Total	97.4	84.9	65.7	32.7	66.9	....

* The number in parentheses indicates the number of cases from which percentages are computed.

TABLE 6

HIGH DEGREE OF ILLEGITIMATE EXPEDIENCY PERCEPTION AND SELA DISCREPANCY WITH SCHOOL HELD CONSTANT

School	SELA Discrepancy					Gamma
	High	Middle	Low	Zero	Total	
A school	92.8 (14)	81.8 (33)	58.5 (65)	41.0 (39)	62.3 (151)	.56
B school	97.6 (41)	93.1 (29)	71.1 (38)	28.9 (45)	69.9 (153)	.84
C school	95.7 (23)	79.2 (24)	74.2 (31)	26.9 (26)	68.3 (104)	.72
Total	97.4 (78)	84.9 (86)	65.7 (134)	32.7 (110)	66.9 (408)	....

V. DISCUSSION

There are several implications of the confirmation of the hypothesis. First of all, it is shown that the variable of SELA can serve as an influential predictor of intraclass differences in adolescents' attitudinal deviation. In recent years there has been considerable research on the influence of socio-psychological factors upon differential degrees of adolescents' deviation either in behavior or attitudes.[14] Most of these studies have concentrated on socioeconomic status of family. By using this factor as an independent variable, previous studies have dealt primarily with the interclass difference in juvenile deviation. It has been frequently reported, for instance, that the lower the level of socioeconomic status of family, the higher the degree of deviation.[15] The previous investigations, in this sense, have left the matter of *intraclass* differences in deviation unexplained.

It is shown in the present study that a substantial amount of the variance in adolescents' deviation left unexplained by the variable of family status can be accounted for by the variable of SELA discrepancy. That is, adolescents' attitudinal deviation is more strongly explained by differences in SELA discrepancy than by differences in family status.

TABLE 7

HIGH DEGREE OF ILLEGITIMATE EXPEDIENCY PERCEPTION AND SELA DISCREPANCY WITH FAMILY OCCUPATIONAL STATUS CONTROLLED

Family Status	SELA Discrepancy: High	Middle	Low	Zero	Total	Gamma
High	92.3 (13)	78.3 (23)	63.6 (44)	36.4 (22)	64.7 (102)	.58
Middle	95.8 (24)	92.0 (25)	63.0 (46)	32.6 (43)	64.5 (138)	.76
Low	97.6 (41)	81.6 (38)	70.5 (44)	32.6 (43)	69.9 (166)	.75
Total	97.4 (78)	84.9 (86)	65.7 (134)	32.9 (108)	67.0 (408)	....

It is useful to point out the general utility of the concept of SELA discrepancy as a predictor in connection with the concept of status inconsistency. The problem of deal-

[14] J. P. Clark and E. P. Wenninger, "Socio-economic Class and Area as Correlates of Illegal Behavior among Juveniles," *American Sociological Review*, XXVII (1962), 826–34; A. J. Reiss and A. L. Rhodes, "The Distribution of Juvenile Delinquency in the Social Structure," *American Sociological Review*, XXVI (1961), 720-32; *A Socio-psychological Study of Adolescent Conformity and Deviation* (Washington, D.C.: U.S. Government Printing Office, 1959); "Delinquency and Social Class Structure," *American Sociological Review*, XXVI (1961), 720–33; A. K. Cohen, *Delinquent Boys: The Culture of the Gang* (Glencoe, Ill.: Free Press, 1955); W. B. Miller, "Lower Class Culture as a Generating Milieu of Gang Delinquency," *Journal of Social Issues*, Vol. XIV, No. 3 (1958); T. S. Monahan, "Family Status and Delinquent Child: A Reappraisal and Some New Findings," *Social Forces*, XXXV (1959), 251–58; A. B. Hollingshead, *Elmtown's Youth* (New York: John Wiley & Sons, 1949); T. Morris, *The Criminal Area: A Study in Social Ecology* (London: Routledge & Kegan Paul, 1957).

[15] It is well recognized that attitudinal deviation is not a sufficient condition for actual deviation, although these two are closely related. There may be many intervening variables. Recently Cohen aptly pointed out the lack of the intervening variables in anomie theory. Regarding the disconcordance between motivation and overt behavior, much research has been made. The main point in our study is that

ing with the effects of social mobility upon the attitudes and behavior of individuals and groups is almost exclusively restricted to adults who have their own station based on occupations. When adults are studied, the sources of their frustrations and depressions stemming from social mobility are considered in the framework of a status-inconsistency phenomenon. That is, social mobility may result in discrepancies among the different positions in each of the attributes, such as income, occupation, education, and ethnicity. This discrepancy called "status inconsistency" has been used as a reasonable predictor of adults' political attitudes, deviant behavior, and social change.[16]

However, when the effects of social mobility upon adolescents are studied, the concept of status inconsistency is obviously not applicable simply because adolescents are not position incumbents but aspirants. The variable of SELA discrepancy, therefore, is believed to serve as an important predictor of adolescents' attitudes and behavior directed toward deviance and social change just as the variable of status inconsistency has been utilized for adults' propensity to deviance and social change.

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SELA discrepancy can be a strong predictor in explaining both adolescents' attitudinal deviation and behavioral deviation if we can assume a continuity between attitude and behavior. It is readily admitted, however, that the utility of SELA discrepancy as a predictor of actual deviation among adolescents remains to be tested (A. K. Cohen, "The Sociology of the Deviant Act: Anomie Theory and Beyond," *American Sociological Review*, XXX [1965], 9; G. Blake and K. Davis, "Norms, Values, and Sanctions," in Robert E. L. Faris [ed.], *Handbook of Modern Sociology* [Chicago: Rand McNally & Co., 1964], pp. 456–84; M. L. DeFleur and F. R. Westie, "Verbal Attitudes and Overt Acts: An Experiment on the Salience of Attitude," *American Sociological Review*, XXIII [1958], 667–73).

[16] Gerhard E. Lenski, "Status Crystallization: A Non-vertical Dimension of Social Status," *American Sociological Review*, XIX (1954), 405–13; Lenski, "Social Participation and Status Crystallization," *American Sociological Review*, XXI (1956), 458–64; I. Goffman, "Status Consistency and Preference for Change in Power Distribution," *American Sociological Review*, XXII (1957), 275–81; E. Jackson, "Status Consistency and Symptoms of Stress," *American Sociological Review*, XXXI (1966), 375–82; G. B. Rush, "Status Consistency and Right-Wing Extremism," *American Sociological Review*, XXXII (1967), 86–92.

Fringe-City Life-Style Distance and Fringe Support of Political Integration[1]

Brett W. Hawkins

ABSTRACT

Aggregate data indexes of differences in life style between cities and fringes are correlated with fringe electoral support of political integration referenda. Contrary to the hypothesized relationship, life-style distance in the fringes' favor is positively correlated with fringe support. Several explanations are considered, and the one found to fit the data best suggests a desire for public services by residents of high-growth, familistic fringes.

The study of urban political integration is rich in case analysis and conceptualization[2] but impoverished in comparative research. However, this deficiency is bound to be remedied because the case-study literature has produced hypotheses for comparative research, while some of the existing conceptualizations suggest usable measures and methods.

This paper is an extensive comparative analysis of urban political integration referenda. By "extensive" we mean a broad-scale comparison of several referenda, as opposed to an intensive comparison of a few. The purpose is to seek broad generalization about factors associated with fringe electoral support of political integration proposals. It is not a study of the conditions under which urban communities are likely to offer such proposals to the electorate in the first place—that is, it is not a study of community differences in the initiation or non-initiation of integration proposals.

One of the conclusions of the case-study literature is that when communities (neighborhoods, polities, or areas) contain very different kinds of people they are unlikely to merge and form new communities. Often this conclusion is expressed in terms of city-fringe differences in life style. Such

[1] This research was financed by the Office of General Research and Institute of Government, University of Georgia. The author thanks Alisa S. Norris for collecting most of the data and Alvin S. Cohan, Mary Beth Holmes, and Anita Allen for assisting in the analysis. I am also grateful to Professor Robert L. Crain for helpful suggestions on an earlier draft.

[2] Relevant conceptualizations include Holden's international relations model; Ostrom, Tiebout, and Warren's economic market model. Toscano's communications model; and Williams' life style model. The latter provides the theoretical base of this paper, with the Shevky-Bell approach to the analysis of differentiation providing the basic measures. See Matthew Holden, "The Governance of the Metropolis as a Problem in Diplomacy," *Journal of Politics*, XXVI (August, 1964), 627–47); Vincent Ostrom, Charles Tiebout, and Robert O. Warren, "The Organization of Government in Metropolitan Areas: A Theoretical Inquiry," *American Political Science Review*, LV (December, 1961), 831–42; K. W. Deutsch, P. E. Jacob, H. Teune, J. V. Toscano, and W. L. C. Wheaton (eds.), *The Integration of Political Communities* (New York: J. B. Lippincott, 1964), pp. 98–119; Oliver P. Williams, "A Framework for Metropolitan Political Analysis," *Southwestern Social Science Quarterly*, XLVIII (December, 1967), 299–310; and Eshref Shevky and Wendell Bell, *Social Area Analysis* (Stanford, Calif.: Stanford University Press, 1955). Relevant case studies include Scott Greer, *Metropolitics: A Study of Political Culture* (New York: John Wiley & Sons, 1963); Edward R. Sofen, *The Miami Metropolitan Experiment* (Bloomington: Indiana University Press, 1963); Brett W. Hawkins, "Public Opinion and Metropolitan Reorganization in Nashville," *Journal of Politics*, XXVIII (May, 1966), 408–18; Richard A. Watson and John H. Romani, "Metropolitan Government for Metropolitan Cleveland: An Analysis of the Voting Record," *Midwest Journal of Political Science*, V (November, 1961), 365–98; and Basil G. Zimmer and Amos H. Hawley, "Local Government as Viewed by Fringe Residents," *Rural Sociology*, XXIII (December, 1958), 363–70.

differences are held to be an important factor in the poor success record of integration referenda. Similar conclusions also appear in the conceptual literature and in what little extensive comparative analysis has been reported.[3] The literature thus suggests that as the attributes of electorates who decide on political integration diverge, their potential for integration declines. When social attributes diverge, one category of people is likely to view political integration as social integration with undesirables. The correlation is negative, and it is equally negative whether the people in question are elites or electorates.

Our interest here is on the latter. It is held that the absence of social homogeneity between urban electorates indicates divergent kinds of groups, values, and life styles.[4] Electorates that differ in their social or ecological characteristics are likely to have different identifications, experiences, and valued ways of life. Futhermore, they are likely to choose different types of leaders, who then are subject to different influences, who serve different interests, and whose own attributes reflect different community attributes and values.

MEASURES AND METHODS

The argument that survey data are needed for the best measurement of diversity in life style is appealing if only because divergent ecologies may not mean underlying value divergences—much less those particular value divergences most relevant to political integration. (Survey data do not in themselves solve the latter problem, of course.) However, social scientists should not pass up analysis of available aggregate data on the grounds that they may not be the best data, especially in exploratory studies of the behavior of electorates. On the other hand, conclusions about life style and other values from the kind of data analyzed here must be considered tentative.

Social statistics based on the Shevky-Bell typology are regarded as among the best available objective indicators of differentiation in urban life style. Like Shevky and Bell, we feel that this method is useful in the systematic analysis of differentiation in American cities other than those in which it was developed—Los Angeles and San Francisco—and in the analysis of areas larger than census tracts.[5] The statistics and their conceptual meanings are: Social Rank: (1) percentage of high school graduates, (2) percentage of white-collar workers; Familism: (3) number of children under 5 years old per 100 women age 15–44 (fertility ratio), (4) percentage of females not in the labor force (female domesticity), (5) percentage of owner-occupied dwelling units, (6) percentage of 14–17-year-olds enrolled in school.[6] A large number of children in the family, women at home, owner-occupied homes, and teen-age children in school suggest a family-centered style of life. (The opposite of "familism" as a style of life is often called "urbanism" and suggests apartment living, few or no children in the family, women at work, and spending one's income on the entertainment opportunities offered by the city.

Our index of diversity in life style between cities and their fringes is made up of a fringe-spending life-style statistic over the same

[3] See, for example, Deutsch *et al.*, *op. cit.*; and Thomas R. Dye, "Urban Political Integration: Conditions Associated with Annexation in American Communities," *Midwest Journal of Political Science*, VIII (November, 1964), 430–46.

[4] See Charles S. Liebman, "Functional Differentiation and Political Characteristics of Suburbs," *American Journal of Sociology*, LXVI (May, 1961), 485–90.

[5] Shevky and Bell, *op. cit.* Pages 2 and 20 are illustrative.

[6] Except for the few intercity consolidations and localities in which cities are governmentally independent of counties, statistics on life style were collected for the county and the city separately, and then a subtraction was made to get the fringe statistic. All statistics on life style are from census data nearest to the year of the referendum. The last statistic is not based on the Shevky-Bell methodology but has been used elsewhere as an indicator of familism. See Thomas R. Dye, "City-Suburban Social Distance and Public Policy," *Social Forces*, XLIV (September, 1965), 100–106.

statistic for the city. For example, when percentage of high school graduates in the fringe is put over percentage of high school graduates in the city, the result is a fringe-city ratio. This is our index. The higher the index number (ratio), the greater the life-style distance in favor of the fringe.[7]

Data were collected for forty-two referenda on political integration. Only proposals contemplating the elimination of at least one unit of government, the urban county approach, and metropolitan districts were adjudged to imply a substantial degree of metropolitan or countywide integration.[8] Annexation, councils of government, and metropolitan planning commissions were omitted. The last three are usually adopted without referendum in any case. With the exception of a few small urban places for which comparable data are not available from published sources, these forty-two cases appear to cover the universe of referendum-based proposals of the requisite type since 1945.

An assumption underlying this research is that urban electorates voting on city-county consolidation, intercity consolidation, federation, the urban county, and metropolitan districts are experiencing comparable political phenomena despite differences in strategic content and the degree of integration contemplated in each instance. Support of all such devices is regarded as an integrative response. This assumption is unavoidable in extensive comparative analyses of urban political integration: extensive comparisons require more abstract categories of analysis.

In the analysis below, the dependent variable is the (percentage) yes vote of fringes. Simple, partial, multiple, and multiple-partial correlation coefficients are used in exploring the relationship of the yes vote to the life-style distance indices. Tests of significance are also reported to help the reader assess the sizability of observed relationships (Tables 1–5).

The hypothesis tested here posits a negative correlation between diverse life styles and the yes vote on political integration. Specifically, we hypothesize that life-style distance increasingly in favor of the fringe is negatively associated with fringe support (associated with fringe opposition). Should the hypothesized relationship appear, we will regard it as evidence to support the theory that when the social attributes of electorates diverge, their potential for integration declines.

FINDINGS

However, the results of the several analyses clearly conflict with the hypothesis. Although the coefficients are not all large, the persistent pattern of positive correlations, which survives the results of partialing, suggests that, as life-style distance increasingly favors the fringe, there is an increasing tendency for fringe electorates to vote *for* political integration, not against it. Theoretically relevant controls show that this deviant finding thoroughly pervades the data. In addition, it is most true where it might be expected to be least true—that is, in the merger-elimination referenda, which contemplate the greatest degree of integration, and in the cities over 100,000, where fringe-city differences in life style might be expected to be most pronounced.

[7] Here fringe percentages are nearly all higher than city percentages. Thus the index actually measures the degree of absolute distance in the fringe's favor.

[8] Areas having merger-elimination referenda were Birmingham, 1948 and 1949; Baton Rouge, 1949; Hampton, Virginia, 1952; Miami, 1953; Newport News, 1956 and 1958; Nashville, 1958; Albuquerque, 1959; Knoxville, 1959; Macon, 1960; Durham, 1961; Richmond, 1961; Muskegon, Michigan, 1961; Columbus, Georgia, 1952; Memphis, 1962; Nashville, 1962; St. Louis, 1962; Virginia Beach, Virginia, 1963; South Norfolk, Virginia, 1963; Chattanooga, 1964; Sacramento, 1964; Iowa City, 1965; Athens, Georgia, 1966; Brunswick, Georgia, 1966; and Jacksonville, 1967. Areas voting on some form of extensive county reorganization were Atlanta, 1950; Cleveland, 1950; Louisville, 1956; Miami, 1957; Toledo, 1959; Buffalo, 1959; Cleveland, 1959; Utica, 1961; Dayton, 1961; Syracuse, 1961; and Miami 1961. The Dayton, Athens, and Brunswick votes were to set up charter commissions. Areas voting on metropolitan districts were St. Louis, 1954 and 1955; Seattle, 1958; St. Louis, 1959; and Denver, 1961.

TABLE 1

LIFE-STYLE-DISTANCE MEASURES CORRELATED WITH THE FRINGE VOTE FOR POLITICAL INTEGRATION

(All Referenda, $N = 42$)

VARIABLE	COEFFICIENTS			
	Simple	Partial	Multiple	Multiple-Partial[a]
Social rank:				
High school graduates	.12	.21		.29
White-collar workers	.11	.16		
Familism:				
Fertility	.30	.42*	.48	
Domesticity	.20	−.03		.47*
Owner-occupied dwelling unit	.17	.08		
Children 14–17 in school	.09	−.03		

[a] The multiple-partial coefficient of correlation is a measure of the total explanatory power of several independent variables while controlling for other independent variables. Here we report a multiple-partial separately for the two social-rank variables and for the four familism variables.

* Significant at the .01 level.

TABLE 2

LIFE-STYLE-DISTANCE MEASURES CORRELATED WITH THE FRINGE VOTE FOR POLITICAL INTEGRATION

(by Type of Referendum)

VARIABLE	ELIMINATION-MERGER REFERENDA ($N = 26$) COEFFICIENTS				URBAN COUNTY METROPOLITAN DISTRICT REFERENDA ($N = 16$) COEFFICIENTS			
	Simple	Partial	Multiple	Multiple-Partial	Simple	Partial	Multiple	Multiple-Partial
Social rank:								
High school graduates	.22	.41		.44	−.13	−.04		.00
White-collar workers	.12	.09			−.15	−.02		
Familism:								
Fertility	.40*	.59*	.63		.24	.19	.29	
Domesticity	.21	−.19		.61*	.06	−.16		.25
Owner-occupied dwelling unit	.16	.15			.0009	−.004		
Children 14–17 in school	.04	.05			−.03	.03		

* Significant at the .01 level.

TABLE 3

LIFE-STYLE-DISTANCE MEASURES CORRELATED WITH THE FRINGE VOTE FOR POLITICAL INTEGRATION

(by Size of Central City)

VARIABLE	CITIES OVER 100,000[a] (N=29) COEFFICIENTS				CITIES UNDER 100,000[a] (N=13) COEFFICIENTS			
	Simple	Partial	Multiple	Multiple-Partial	Simple	Partial	Multiple	Multiple-Partial
Social rank:								
High school graduates....	.23	.06		.32	.01	−.07		.36
White-collar workers.....	.32	.33			−.007	.23		
Familism:								
Fertility...............	.11	.21	.57		.48	.61	.62	
Domesticity...........	.35	.23		.49**	.05	−.36		.62**
Owner-occupied dwelling unit................	.43*	.11			−.06	.05		
Children 14–17 in school..	.28	−.04			.03	.09		

[a] For evidence of a break in community social institutions at the 100,000 level, see Fenton Keyes, "The Correlation of Social Phenomena with Community Size," *Social Forces*, XXXVI (May, 1958), 311–15.

* Significant at the .05 level.

** Significant at the .01 level.

TABLE 4

LIFE-STYLE-DISTANCE MEASURES CORRELATED WITH THE FRINGE VOTE FOR POLITICAL INTEGRATION

(by Outcome of Referendum)

VARIABLE	SUCCESSFUL REFERENDA (N=18) COEFFICIENTS				UNSUCCESSFUL REFERENDA (N=24) COEFFICIENTS			
	Simple	Partial	Multiple	Multiple-Partial	Simple	Partial	Multiple	Multiple-Partial
Social rank:								
High school graduates...	.29	.45		.49	.49*	.43		.64**
White-collar workers.....	−.04	−.05			.41*	−.04		
Familism:								
Fertility...............	.56*	.74**	.83*		−.13	.14	.67	
Domesticity...........	.38	−.001		.81**	.01	.27		.51*
Owner-occupied dwelling unit................	.44	.56*			.12	−.49*		
Children 14–17 in school..	.02	−.06			.23	.31		

* Significant at the .05 level.

** Significant at the .01 level.

Proposals to eliminate a county or suburban government might be expected to produce the most electoral concern about social integration or, alternatively, the most intense governmental protectivism. And since life-style differences and other types of heterogeneity in urban subareas are thought to increase with increasing density, size, and scale,[9] one might expect the hypothesized relationship to be most true of the bigger cities.

What is there about smallness that might produce the results found here? As already noted, small urban areas are said to be less socially and ecologically diverse than larger and more dense areas. Up to some unknown size, one might reason, differences in life style are not so great as to be accompanied by separatist values among voters. Beyond that size, separatist values develop. However, if this were actually an explanation of our finding, one would expect the hy-

TABLE 5

LIFE-STYLE-DISTANCE MEASURES CORRELATED WITH THE FRINGE VOTE FOR POLITICAL INTEGRATION

(by Region)

Variable	South* (N=25) Coefficients				Outside the South (N=17) Coefficients			
	Simple	Partial	Multiple	Multiple-Partial	Simple	Partial	Multiple	Multiple-Partial
Social rank:								
High school graduates...	.04	.34	.61	.39	.31	.30	46	.29
White-collar workers.....	.04	.22			.22	−.27		
Familism:								
Fertility................	.46	.58		.61	.06	.20		.23
Domesticity............	.22	−.11			.16	−.06		
Owner-occupied dwelling unit..................	.06	.23			.35	−.19		
Children 14–17 in school..	−.03	.05			.27	.13		

* The "South" here refers to the eleven ex-Confederate states.

DISCUSSION

Why should life-style distance in the fringe's favor be positively related to fringe support of political integration? Perhaps the obvious place to begin looking for an explanation of this finding is in the particular universe analyzed here. The finding may be a function of the universe under analysis. For example, the cities covered in this study include few large cities, and most are in the South.

pothesized negative correlation of life-style distance to fringe support to be more pronounced in the larger urban areas than in the smaller ones. In fact, we find a more pronounced positive relationship in the larger urban areas. If smallness, or some attribute of smallness, were the explanation of our deviant finding, one would not expect the deviant relationship to be stronger in the larger urban areas.

The impact of "Southernness" on our finding is also food for speculation. One might reason that in Southern urban areas the absence of strong city-suburban political party differences makes it easier for

[9] See Shevky and Bell, *op. cit.*, especially pp. 14–17, and Philip M. Hauser and Leo F. Schnore, eds., *The Study of Urbanization* (New York: Wiley, 1965), p. 22.

elites to get together on the initiation of integration plans, and later on their promotion. One might also reason that the existence of a more middle-class active electorate in the South, compared with the North,[10] is likely to mean more support for a good-government, efficiency-and-economy idea like political integration. And, in fact, a a regional breakdown shows some differences between North and South. In the South the multiples and multiple-partials are higher, and the partials are higher and more positive. In other words, the positive relationship is somewhat stronger in the South. There is therefore a regional difference that suggests some effect on our findings of Southernness, perhaps along the lines suggested above. On the other hand, regional differences are not so large that one can confidently regard region as a good explanation of our deviant finding. Five of the twelve coefficients of simple and partial correlation are higher in a positive direction in the North. Further analysis is clearly indicated.

Another possible explanation is based on the idea, already introduced, that political integration is an item in the reformist, good-government program. Like manager governments and non-partisan elections, integrative governments are often described as expressions of upper-status or middle-class preferences for efficiency, economy, professionalism, and good-of-the-whole-community values. Perhaps a good explanation of our finding is that it reflects public-regarding values[11] among upper-status-fringe electorates.

If this were actually an explanation of our finding, one would expect the *absolute social rank* of fringes to be more highly and positively related to yes vote than the fringe-city ratios (along the social rank dimension). Fringe residents are presumably public-regarding because of their wealth or social rank, not because the central city is poor. In order to examine our data for evidence of this explanation, we first computed partial correlations of the ratios to support with absolute values of social rank controlled and of absolute values to support with the ratios controlled. Second, the pattern of multiple-partials between (1) absolute social rank and support and (2) the social rank ratios and support was examined. The results show no consistent pattern. For example, the partials show a slight tendency for the coefficients computed from absolute fringe values to the higher, but the mean difference between the coefficients computed from absolute values and those computed from the ratios is only about 0.06. The multiple-partials, on the other hand, show a slight tendency for the coefficients computed from the ratios to be higher, but the difference is only 0.12 for all referenda. (The mean difference within all the control categories is 0.16.) In three categories multiple-partials computed from absolute values are higher. From these results we conclude that there is no evidence in the data that public-regardingness is an explanation of our finding. However, the fact that absolute social rank correlates with the yes vote about as highly as life-style distance suggests that public regardingness may be a factor in fringe support.

Still another possible explanation is that fringe areas usually have much to gain from political integration. Advocates of integrative governments have long noted that important service needs accompany rapid

[10] See V. O. Key, *Public Opinion and American Democracy* (New York: Alfred A. Knopf, Inc., 1961), p. 105.

[11] "Public-regarding" values are characterized by a good-government, good-of-the-whole-community, efficiency-and-economy-in-the-provision-of-services attitude toward local policy proposals. See James Q. Wilson and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," *American Political Science Review*, LVIII (December, 1964), 876–87; Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," *American Political Science Review*, LX (June, 1966), 306–27; and Robert L. Lineberry and Edmund P. Fowler, "Reformism and Public Policies in American Cities," *American Political Science Review*, LXI (September, 1967), 701–16.

suburbanization and that new fringe developments are often lacking in sewers, street lights, garbage pickup, schools, and other public services. Perhaps our finding reflects the desire of familistic electorates in rapidly growing fringes for better public services or capital improvements in their communities.

If this were an explanation of our finding, one might expect (1) the growth rate of the fringes under analysis to be very high and (2) a sizable positive correlation between fringe growth rates and fringe support. And, in fact, the mean growth rate of the fringes under analysis (in the nearest previous decade to the vote) is an enormous 214.3 per cent. The national rate (in Standard Metropolitan Statistical Areas, outside central cities) was 48.6 per cent between 1950 and 1960, and 35.9 per cent between 1940 and 1950. Within control categories, growth rates range from 55.9 to 540.0 per cent; four are over 200 per cent. Also, fringe growth rate correlates +.32 with the yes vote on political integration. This coefficient is statistically significant at the .05 level. Within the various control categories, almost all the comparable coefficients are over +.30.

Other bits of evidence in the data that point in the direction of this explanation are the persistently high correlation of the fertility ratio to fringe support, and the generally higher association between the familism ratios and fringe support than between the social rank ratios and support. These relationships may mean that familistic, child-oriented fringe electorates see integrative governments as a way to get services that enhance and preserve their familistic way of life.[12] That central cities will help to foot the service bill under integrative governments should make the latter all the more desirable to fringe electorates.

Thus, the evidence in the data points to a desire for services—perhaps based on the need for capital improvements felt by the residents of higher growth areas and on familistic life-style preferences—as a partial explanation of our deviant finding.[13] Southernness also appears to be a factor, although a small one. However, inferences about desire for service and other values from aggregate data should be treated as tentative. Comparative survey data are lacking on voter perceptions of the various integrative devices, and so one cannot be confident that fringe electorates actually perceive integrative governments as service-providing and life-style-maintaining. But the evidence here suggests that they do. Perhaps more important for theory-building, the evidence in our data strongly indicates that fringe-city life-style differences are not the generalized obstacle to urban political integration that many students have felt them to be. In fact, the present broad-scale comparative analysis suggests that such differences more commonly spur a yes vote where high rates of suburbanization have taken place.

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[12] For a discussion of urban policies (functions) that tend to threaten or to maintain suburban life styles, see Williams, *op. cit.*

[13] The independent variables included in this study leave much of the variance in the yes vote unexplained. Like so many other electoral phenomena, support of integrative governments is multivariate and elusive.

Automation and Bureaucratic Structure[1]

Marshall W. Meyer

ABSTRACT

Most studies of automation in organizations show its effects on human relations, but they neglect changes in formal organizational structure. Data from a study of 254 city, county, and state departments of finance show that the data-processing sections are quite different from other parts of organizations: they have more levels of hierarchy, a wider span of control for first-line supervisors, fewer employees under the direction of higher supervisors, and fewer supervisory responsibilities for members who are nominally in supervisory positions. These findings suggest that the consultant's role and horizontal channels of communications are institutionalized in automated organizations. The implications of these findings for classical organizational and human relations theories are discussed.

Most theories of organizations can be conveniently reduced to either "Theory X" or "Theory Y." The former, "Theory X," stresses formal organization, authority, and administrative process; "Theory Y" emphasizes motivation, human relations, and informal processes in work situations. The divergent approaches to organizational theory force proponents of either side to put their arguments in perspective. Advocates of "Theory X" must continually acknowledge that human relations *do* make a difference; similarly, human relations specialists from time to time concede that formal properties or organizations have some impact on the kinds of behavior they study.

Empirical research on organizations has not been so neatly balanced as the theories. The great majority of researchers have dealt with "Theory Y"—problems of human relations in organizations. The work of Roethlisberger and Dickson, of Whyte, of Katz and Kahn,[2] and of many others is largely concerned with individual behavior in an organizational setting. The problems of organization—or formal organization as it is so often called—are usually relegated to sociologists or students of public administration rather than treated by organizational specialists. The sociologists have examined the relationship between "formal" and "informal" organization;[3] political scientists interested in administration have elaborated numerous theories but few findings to confirm or disconfirm them.[4] The fact that empirical knowledge of organizations consists for the most part of propositions about human relations is hardly surprising. There exist numerous psychological and sociopsychological propositions that have been tested in the laboratory and that seem readily applicable to behavior in organizations. By contrast, there exists no comparable body of knowledge about organization per se.

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[2] Fritz J. Roethlisberger and William J. Dickson, *Management and the Worker* (Cambridge, Mass.: Harvard University Press, 1939); William F. Whyte, *Human Relations in the Restaurant Industry* (New York: McGraw-Hill Book Co., 1948); Daniel Katz and Robert L. Kahn, *The Social Psychology of Organizations* (New York: John Wiley & Sons, 1964).

[3] Peter M. Blau, *The Dynamics of Bureaucracy* (2d ed., revised; Chicago: University of Chicago Press, 1963); Alvin W. Gouldner, *Patterns of Industrial Bureaucracy* (Glencoe, Ill.: Free Press, 1954).

[4] Herbert A. Simon, *Administrative Behavior* (New York: Macmillan & Co., 1957); Dwight Waldo, *The Administrative State* (New York: Ronald Press, 1948).

One develops understanding of formal organization by studying whole organizations (not work groups or informal cliques) and many of them, but to date there has been very little research of this sort.[5]

The study of the impact of automation in organizations perhaps best illustrates this disparity between theories and empirical research. Automation, *as an organizational phenomenon*, is extremely important because of the way it can change fundamental administrative processes. Yet there have been few studies of organizational changes associated with the use of computers. Instead, researchers have for the most part concentrated on the effects of automation on the behavior and feelings of the individual worker. And as is so often the case in the social sciences, only contradictory findings have emerged. It is found, for instance, that automation decreases workers' satisfaction with their jobs, almost to the point of quitting.[6] Ironically, the same study reports that in the long run, workers derive satisfaction and pride from the same factors that earlier caused discontent. It has been argued that automation reduces the need for supervision and allows wider participation in decision making, but some empirical results have shown just the opposite.[7] Finally, a number of commentators have predicted that automation will help upgrade jobs and cause the replacement of the working class by what Daniel Bell terms a white-collar *salariat*.[8] But again there is contradictory evidence. At least one study shows that automation reduces the level of required skill and increases the tedium of a worker's job.[9]

The studies of the effect of automation on human relations are not to be criticized because of their inconsistent findings; that is almost inevitable. They are to be criticized, instead, because they study the wrong thing. There is an implicit assumption in human relations research on automation that has never been substantiated. The assumption is roughly this: the impact of automation on the worker is direct; it is not mediated by formal organizational structure (e.g., the hierarchy of authority, division of labor, rules, regulations, etc.). This assumption is rejected here. The effects of automation on human relations should be assessed only after taking into account the changes in organizational processes which automation itself engenders. This paper will show that the use of a computer changes the administrative structure of organizations. Having shown this, it will then discuss implications for both classical organizational theory and human relations theory.

THE DATA-PROCESSING STAFF AND THE PROBLEM OF INTERDEPENDENCE

Automation—the use of computers—as a technical innovation probably has little impact on formal bureaucratic structure, but how the data-processing function is organized in a bureaucracy does have important effects on administrative processes. Two factors should be noted. First, most automated organizations have a separate data-processing staff. Of the organizations included in this study (which we shall describe presently), 86 per cent of those which

[5] Some comparative studies of organization include: Arthur L. Stinchcombe, "Bureaucratic and Craft Administration of Production," *Administrative Science Quarterly*, IV (1959), 168–87; Stanley H. Udy, "'Bureaucracy' and 'Rationality' in Weber's Theory," *American Sociological Review*, XXIV (1959), 791–95; and Peter M. Blau, Wolf V. Heydebrand, and Robert Stouffer, "The Structure of Small Bureaucracies," *American Sociological Review*, XXXI (1966), 179–86.

[6] Charles R. Walker, *Toward the Automatic Factory* (New Haven, Conn.: Yale University Press, 1957).

[7] Floyd L. Mann and Lawrence K. Williams, "Observations on the Dynamics of a Change to Electronic Data-processing Equipment," *Administrative Science Quarterly*, V (1960), 217–56; Thomas L. Whisler and George P. Schultz, "Automation and the Management Process," *Annals*, CCCXL (March, 1962), 81–89.

[8] Daniel Bell, "Work and Its Discontents," in *The End of Ideology* (New York: Free Press, 1960), pp. 222–62.

[9] James R. Bright, "Does Automation Raise Skill Requirements?" *Harvard Business Review*, XXXVI, No. 4 (July–August, 1958), 85–89.

use a computer have a data-processing staff. This staff is unique because its members are specialists in the operations of the computer and so have skills which other members of the organization do not understand. A second factor is that the data-processing activity is not itself a primary goal of organizations. Rather, it is intended only as an efficient means to organizational goals. Were computers inefficient, they would not be used. Perhaps this last point belabors the obvious, but it leads to the following formulation: the data-processing staff, composed of specialists whose work is not understood by others, performs a service for other units of an organization by quickly and cheaply doing tasks that would otherwise occupy many employees.

The formation of a data-processing staff, then, creates interdependence in an organization. The various line units—those whose activities contribute directly to organizational goals—must rely on the performance of the data-processing staff to accomplish their goals. A great deal of communication between line units and the data-processing staff is thus required so that their efforts can be co-ordinated. If such communication is absent or breaks down, then the data-processing staff cannot make its contribution and automation will seem to have failed. Communication between line units and the data-processing staff can be maintained in several ways. One could, if he wished, insist that all messages go vertically "through channels." Thus a message between members of line and data-processing units would be transmitted upward through the hierarchy until it reached their lowest common superior (that is, the person lowest in the hierarchy who has jurisdiction over both), and then sent downward to its recipient.[10] This method is likely to be a very expensive way of achieving co-ordination. The hierarchical channels would become so overburdened with messages that an inordinate proportion of an organization's effort would be devoted to maintaining communication between line units and the data-processing staff. Whatever efficiencies result from automation would be more than lost in this attempt to preserve hierarchical co-ordination in an organization characterized by interdependence among its subunits.

In practice, of course, most organizations tacitly permit or even encourage horizontal (i.e., non-hierarchical) communication among members. Even where it is illicit, horizontal communication frequently takes place in bureaucracies.[11] Where it is neither prohibited nor allowed for in a table of organization, horizontal communication sometimes results in better co-ordination of an organization's activities than might occur if supervisors intervened in the situation.[12] Most organizations simply allow horizontal communication to take place because it poses no real problems in day-to-day operations. Among peers—employees of roughly equal status—horizontal interchanges and co-operative relationships rarely cause trouble, especially if the precepts of human relations theory are followed. Among employees who hold very different statuses in an organization, deference (*not* authority, which presumably operates only in hierarchical situations) permits co-operation of sorts to emerge in non-hierarchical relationships.

The problem of interdependence and horizontal communication, however, is not solved by indirection when it is caused by the presence of a data-processing staff in an organization. The "clients" of a data-processing staff are most frequently heads of line units or their assistants; non-supervisory line personnel have little to do with data processing. The head of a line division is of high status in the organization, but the chances are that he understands little about what the computer (and the data-processing staff) can and cannot do for him. In this

[10] This practice of excessive "hierarchicalism" was quite common in pre–World War II French bureaucracy. See Walter R. Sharp, *The French Civil Service: Bureaucracy in Transition* (New York: Macmillan Co., 1931), pp. 40–42.

[11] See Blau, *op. cit.*, chap. vii.

[12] See Richard L. Simpson, "Vertical and Horizontal Communication in Formal Organizations," *Administrative Science Quarterly*, IV (1959), 188–96.

sense, he is of low expertness. If the situation goes unchecked, it is likely that he will have to deal with a member of the data-processing staff who occupies a fairly low status in the organization—a programmer or systems analyst who has no supervisory responsibility but who is of high expertness because of his knowledge of the computer. Co-operative relationships are very difficult to maintain under these circumstances. The line manager may, because of his low expertness, make unreasonable requests of the data-processing staff. What is worse, he may attempt to insist that these requests be fulfilled by asserting the authority of his office. The data-processing specialist may, in return, deny an unreasonable request (or sometimes a reasonable one if it is inconvenient) by asserting the authority of his expertness. He, not his high-status client, understands the intricacies of the computer. Disagreements, non-co-operation, and avoidance behavior thus characterize relationships between superior non-experts and subordinate experts; this is often called line-staff conflict.[13]

One could interpret the difficult relationship between line managers and data-processing specialists as a human relations problem, but in this paper it will be considered as a result of a type of bureaucratic structure that compels high-status non-experts and low-status experts to co-operate with one another. In other words, the tensions and strains so often associated with automation are in part caused by administrative arrangements that become inadequate as interdependence increases in an organization. It is hypothesized here that many organizations, when faced with the problem of interdependence between dispersed parts, adapt to this situation by altering their formal structure. In particular, the role of the consultant—a high-status member of the data-processing staff who is primarily responsible for co-ordinating the work of line units with that of computer specialists—is institutionalized as interdependence increases.

The consultant is a member of the data-processing staff who at least nominally holds a high-level supervisory position. He spends little of his time supervising, however, because he is primarily concerned with the task of co-ordination. From the perspective of classical organizational theory, the consultant's role is an anomalous one: he is a supervisor whose job is not to supervise. But as a matter of organizational practice, the consultant's role is a sensible innovation. Because the consultant is of high status, a line manager cannot dominate him by asserting the authority of his office. For the same reason, line managers have little reason to suspect the consultant of using his expertness to augment his power in the organization. Thus, in developing the consultant's role, some of the difficulties inherent in line-staff relationships may be avoided. The data-processing staff, then, is distinguished from other parts of an organization by its hierarchy of authority which includes one or more consultants—members who have supervisory authority but whose real task is to maintain co-ordination between line units and data-processing specialists.

Before turning to the data, perhaps it would be best to outline the causal model suggested above:

1. In automated organizations, a staff of data-processing specialists whose work is not understood by others runs the computer.
2. Data processing is not an organizational goal, but only a means to a goal; thus, there is much interdependence between the data-

[13] V. A. Thompson has discussed at length the problem of relationships between non-expert superiors and expert subordinates. See "Hierarchy, Specialization, and Organizational Conflict," *Administrative Service Quarterly*, V (1961), 511–12. See also Thompson's book, *Modern Organization* (New York: Alfred A. Knopf, Inc., 1951), where he labels the dysfunctional aspects of expert-non-expert relationships as "bureaupathic" behavior. Jack Seigman and Bernard Karsh, in "Some Organizational Correlates of White-Collar Automation," *Sociological Inquiry*, XXXII (1962), 108–16, describe a case of organizational conflict resulting from

3. Interdependence requires horizontal communications between an organization's units, but if no changes are made in traditional forms of organization, high-status members of line units must deal with low-status data-processing experts.
4. Co-operation between high-status non-experts and low-status experts is difficult to maintain and is a source of much tension. Thus, many organizations evolve the consultant's role so that high-status members of line divisions deal only with a high-status member of the data-processing unit.

DATA AND METHODS

The data for this paper are from a study of 254 city, county, and state departments of finance or comptrollers' offices. These departments have primary responsibility for financial administration in their jurisdictions. The study is described in detail elsewhere,[14] but several points bear special relevance to this report. An interview schedule for each department included a series of questions that indicate what a realistic organization chart might look like. Official organization charts were not used as the primary source of information, since they are too frequently inaccurate and often constructed with different considerations in mind. Interviewers asked the head of each division in a department how many levels of supervision, including himself and non-supervisory employees, exist in his division. The division heads were also asked how many employees work at each level and what proportion of their time employees at each level spend supervising—planning, directing, or reviewing the work of others. Employees at the lowest level of hierarchy, of course, spend no time supervising. Since the divisions within finance departments are small—their median size is twelve employees—this method provides fairly accurate information about the hierarchy of authority in divisions.

In considering data that describe the divisions in finance departments, this paper will compare the unit which has primary responsibility for the data-processing activity to the other units of an organization. To do this, a three-way classification of departments and divisions is used; it appears in Table 1A. Column 1 in the table presents data for the 121 departments in which there is no data-processing division. Among these departments, for example, there is an average of 2.84 levels of supervision (including the non-supervisory level) in each division, and the mean number of employees for whom each first-line supervisor is responsible is 6.62. The data in column 2 describe departments which have a data-processing division, but the division in charge of the computer has been excluded from the calculation of means. Thus, excluding the data-processing units, automated departments have an average of 2.97 levels of supervision in divisions, and the span of control of first-line supervisors is 7.22. The third column includes only data-processing divisions. These divisions have, to illustrate, a mean of 3.28 levels of supervision, and the average span of control over non-supervisory employees is 8.55.

Perhaps we should first consider how the presence of a data-processing unit affects an organization's use of highly qualified personnel. One might think that data-processing experts are more likely to have college degrees than other employees in finance departments. The bottom row of Table 1 shows that this is not the case, however. Divisions in charge of data processing have a lower proportion of positions for which a college degree is desired (19.5 per cent) than divisions of non-automated departments 25.9 per cent) or line divisions of automated departments (29.9 per cent). While a data-processing unit employs many workers who lack high educational qualifications, the use of a computer increases the need for highly qualified personnel in other parts of an organization. Some small part of the dif-

[14] The study of finance departments is further described in Peter M. Blau, "The Hierarchy of Authority in Organizations," *American Journal of Sociology*, LXXIII (1968), 553–67; and in Marshall W. Meyer, "The Two Authority Structures of Bureaucratic Organization," *Administrative Science Quarterly*, September, 1968.

ference between data-processing sections and other units in automated departments may result from the transfer of workers with low qualifications from other units to the section in charge of the computer. But most of the difference reflects a genuine need for more highly qualified personnel. The operation and maintenance of a computer do not require the generalized competence that a college education provides, but planning for the use of a computer and evaluation of the results which take place in line divisions do require a high level of competence.

The uneven distribution of college-trained personnel between the data-process-

TABLE 1A

MEANS OF STRUCTURAL VARIABLES BY AUTOMATION IN DEPARTMENTS AND DIVISIONS

Variable	Non-automated Departments, Average of All Divisions (1)	Automated Departments: Average of Line Divisions (2)	Automated Departments: Data-Processing Division (3)
1. Number of levels of supervision	2.84	2.97	3.28
2. Employees supervised by first-line supervisors	6.62	7.22	8.55
3. Employees supervised by other supervisors	1.58	1.57	1.27
4. Percentage of time first-line supervisors actually supervise	36.7	33.1	41.4
5. Percentage of time other supervisors actually supervise	41.7	43.5	38.5
6. Percentage of employees for whom a college degree is desired[a]	25.9	29.9	19.5
Number of departments	121	133	

[a] Difference between columns 1 and 2 significant at the .07 level.

TABLE 1B

TESTS OF SIGNIFICANCE

Variable	Difference	Standard Error of Difference	t	p
Between Columns 1 and 3				
1	− 0.434	0.137	−3.18	.002
2	− 1.93	0.779	−2.48	.014
3	0.303	0.019	2.18	.030
4	− 4.71	3.23	−1.46	n.s.
5	3.25	3.80	0.854	n.s.
6	6.49	2.32	2.79	.006
Between Columns 2 and 3				
1	− 0.307	0.101	−3.04	.003
2	− 1.34	0.618	−2.16	.032
3	0.296	0.111	2.68	.008
4	− 8.33	2.77	−3.01	.003
5	5.03	2.96	1.70	.092
6	−10.46	1.96	−5.344	.001

ing staff and the line divisions of an organization probably exacerbates the difficulties caused by interdependence. Relations between high-status line managers and low-status computer experts become all the more strained because of the disparity between the educational qualifications of these groups. The manager who possesses a college degree probably would find it quite trying to co-operate with and depend upon a data-processing specialist who, compared to the manager, is uneducated. Thus, development of the co-ordinator's role becomes all the more important in automated organizations.

ences between data-processing divisions and divisions and other subunits are shown by t-tests to be statistically significant below the .05 level. It should also be noted that these findings are not artifacts of the numbers of employees in divisions. Data-processing divisions are slightly larger than others, but the differences in size are not significant.

It might be best to summarize the present findings by constructing models of divisions which the data suggest. The models in Figure 1 exaggerate the differences we have found, but they show the distinctive features of a data-processing division when

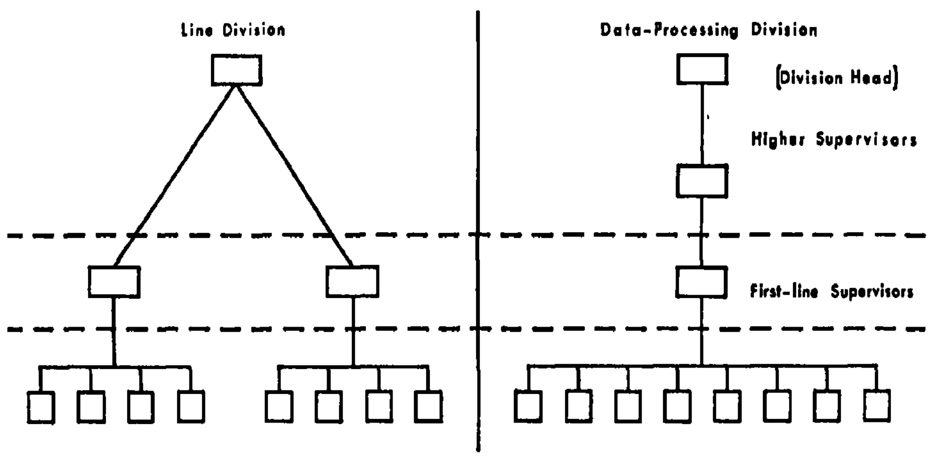


FIG. 1.—Two models of divisions

Table 1 also shows that the formal structure—the distribution of offices—in data-processing divisions differs from that in other subunits of finance departments. Data-processing divisions have more levels of hierarchy (3.28) than other divisions of automated departments (2.97) or divisions in non-automated departments (2.84). The average span of control of first-line supervisors is also higher in data-processing divisions (8.55) than in either other sections of automated finance agencies (7.22) or departments that have no data-processing unit (6.62). Finally, higher supervisors are responsible for fewer employees in computer sections (1.27) than in other subunits of automated departments (1.57) or in non-automated departments (1.58). These differ-

compared to other divisions. In the models, there are four levels in the data-processing unit and three in the other division. The span of control of the first-line supervisor is eight and that of higher supervisors is one in the data-processing division; the first-line span of control is four and the higher-level span of control is two in the hypothetical other division. The models show that the differences between data-processing and other divisions which we have observed can be explained as the result of change in the position of one or two supervisors. The position of a first-line supervisor is moved so that other first-line supervisors report to him and he, in turn, reports to the division head. This change simultaneously increases the number of

hierarchical levels, increases the average span of control of first-line supervisors, and decreases the span of control of higher supervisors. Data-processing sections, then, assume a more vertically differentiated structure than other subunits of a finance department. The number of supervisory levels, importantly, is increased by differentiating among supervisory personnel rather than by assigning supervisory positions to formerly non-supervisory personnel.

The unique structure of a data-processing section partly reflects the amount of planning and co-ordination that must take place within the unit so that it can service other parts of the department. Higher managers in the data-processing division spend less of their time (38.5 per cent of it) supervising than do higher managers in other divisions of automated departments (43.5 per cent) or their counterparts in non-automated agencies (41.7 per cent, though this difference is not significant). The work of a higher supervisor in a data-processing unit involves relatively little supervision, and, we suspect, much advising and consultation with members of other divisions to cope with the problems inherent in the use of a computer. A seeming paradox of automation is that it creates an extra level of supervisors whose job is not to supervise. But this paradox helps to resolve potential line-staff conflict. By having high-status managers who act as consultants to others over whom they do not have formal jurisdiction, one avoids situations in which low-status staff specialists attempt to give authoritative advice to high-status non-experts.

The special structure of data-processing units is also reflected in the activities of first-line supervisors. The managers who direct the work of non-supervisory personnel in the data-processing division spend more of their time supervising (41.4 per cent) than other first-line supervisors in automated departments (33.1 per cent) or first-line managers in non-automated departments (36.7 per cent, though again this difference is not significant). These differences can be attributed, to some extent, to the wide span of control that first-line supervisors have in data-processing divisions. These differences also create discontinuities in the authority structure of data-processing units. Such a discontinuity occurs when an intermediate level of managers has less supervisory responsibility than first-line managers. From Table 1, it is clear that in both non-automated departments and line divisions of automated ones, higher managers spend more of their time supervising than do first-line supervisors. The higher one ascends the hierarchy, the more supervisory responsibility he has. This is not the case among data-processing divisions, where, if anything, the opposite takes place; first-line managers spend more time supervising than do their superiors. Perhaps one latent function of such hierarchical discontinuities is to render lower-level data-processing personnel "unavailable" to top managers.[15] The proliferation of supervisory levels and discontinuities in the authority structure make it unlikely that high-status but non-specialist managers can make requests directly of low-status data-processing specialists, thus averting another source of line-staff conflict.

IMPLICATIONS FOR THEORIES OF ORGANIZATIONS

The present findings require some comment on both the classical and human relations theories of organizations. Most theories of organizational structure hold in common the notion that the use of hierarchical authority ought to be the principal means by which the activities of dispersed parts of organizations are co-ordinated. Weber speaks of "strict super- and subordination" that characterizes bureaucracies. Gulick and Urwick, attempting to salvage the notion that all relationships within

[15] We use the word "unavailable" to connote insulation of lower-level personnel from the direct demands of top managers. This meaning is similar to Kornhauser's use of the term "available" in a political context: the ability of elites to mobilize non-elites. See William Kornhauser, *The Politics of Mass Society* (Glencoe, Ill.: Free Press, 1959).

bureaucratic structures are hierarchical ones, devise the administrative fiction that the staff acts in the name of the head of the organization and that for this reason—not its expertness—the advice of staff members ought to be followed. March and Simon argue that the function of hierarchical structures is to translate the general goals of organizations into operational programs of behavior for subordinates; they make no allowance for interdependence and non-hierarchical forms of co-ordination in this process.[16] The findings in this paper indicate that theories of formal organization ought to take into account interdependence in bureaucratic structures and its implications for administrative practice. A simple proposition is advanced as a contribution to theories of formal organizational structure: as interdependence in an organization increases, non-hierarchical forms of co-operation and the co-ordinator's role tend to emerge.

From the perspective of human relations theory it is very important whether or not an organization retains a traditional vertical structure as interdependence increases. If a data-processing unit is superimposed on an older organizational form without other adjustments, then human relations are likely to suffer. Difficulties and delays in communication between line units and the data-processing staff are one source of frustration. Another problem is line-staff conflict which we discussed earlier: Who has final authority when differences arise between the computer experts and line managers? In traditional line organizations which allow only vertical channels of communications, these differences will all too frequently have to be resolved by the head of the organization, overburdening him with demands for decisions. Ultimately, this leads to high centralization of control, which in itself tends to contribute to an authoritarian atmosphere and poor human relations. If, by contrast, an organization can dispense with the concept of a rigid chain of command and allow horizontal interchange, then automation will have little impact on its members' ability to co-operate with one another.

More generally, a point made in the initial discussion should be restated here. One cannot assume that there is a simple relationship between the use of a computer in an organization and its impact on human relations among employees. Organizational structure—the arrangement of work by divisions or subunits and by levels of hierarchy, and the channels of communication that are available to employees—intervenes between automation and its effects on workers. Older organizations and those which maintain the most rigid hierarchical arrangements will probably experience the greatest difficulties with automation.[17] Thus organizational structure, not the use of a computer in and of itself, is the proximate cause of the human relations problems so often associated with automation.

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[16] Max Weber, "Bureaucracy," in H. Gerth and C. W. Mills (eds.), *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), pp. 196–244; Luther Gulick and L. Urwick, *Papers in the Science of Administration* (New York: Institute of Public Administration, 1937); James G. March and Herbert A. Simon, *Organizations* (New York: John Wiley & Sons, 1958), chap. vi.

[17] Arthur L. Stinchcombe argues that an organization's age is an important determinant of its structure. See "Social Structure and Organizations," in James G. March (ed.), *Handbook of Organizations* (Chicago: Rand McNally & Co., 1966), pp. 142–93.

Is Everyone Going Modern? A Critique and a Suggestion for Measuring Modernism[1]

John B. Stephenson

ABSTRACT

Modernization may be defined as a personal and cultural expression of one type of social change, namely, movement of persons along a dimension from what is defined by the cultural norms as traditional to what is considered modern by the same culture. An example is presented of the construction of a Guttman scale based on this usage, which does not assume a universal set of traits of "the modern" or "the traditional." The data are from an Appalachian population. Unidimensionality is suggested by the data, but the evidence is not conclusive. Modernism-traditionism may be measurable in a population only if modernization is comprehended by the members as a change process occurring among them. Limitations on comparability of different populations by the use of such scales are discussed.

THE PROBLEM

A. CURRENT INTEREST IN SOCIAL CHANGE

The attention of social scientists has turned back again to the subject of change. Within the past fifteen years, neglect of the study of social dynamics has changed to active interest, as indicated by the increased number of articles in professional journals on the nature and consequences of change at various levels and in a variety of institutional areas—politics, economics, religion, for example—and of change in culture as well as social organization. In this growing body of literature different types of changes are given somewhat confusing and like-sounding labels, such as democratization, development, industrialization, urbanization, secularization, and modernization.[2] Agreement is not perfect, but it probably would be generally accepted that, whereas the terms "development" and "industrialization" are used to describe mainly economic growth, and urbanization is used to describe shifts in population dispersion and concentration, the concept of modernization has to do with a transformation of culture and of personality insofar as it is influenced by culture, rather than of some aspect of social organization or of human ecology.[3] This paper is concerned with modernization in the cultural and personality sense, and more particularly with *measurements* of modernism.

B. MEASURES OF MODERNISM

It should be made clear that modern*ization* is not the same as modern*ism*. The idea of modern*ization* is that there is an alteration or a movement of something from a

[1] I am grateful to the University of Kentucky for financially assisting the project out of which this paper grew. I would also like to express my gratitude to David Lewis Smith, interviewing assistant, and Mrs. Edna Urie, research assistant. Critical readings of the paper by Leonard Jordan, Thomas R. Ford, and George A. Hillery were also very helpful.

[2] For recent general discussions of these types of change, see, for example, Daniel Lerner, *The Passing of Traditional Society* (Glencoe, Ill.: Free Press, 1958); S. N. Eisenstadt, *Modernization: Protest and Change* (New York: Prentice-Hall, Inc., 1966); George M. Foster, *Traditional Cultures and the Impact of Technological Change* (New York: Harper & Row, 1962); Bert F. Hoselitz and Wilbert Moore (eds.), *Industrialization and Society* (Paris: UNESCO-Mouton, 1963); Ralph J. Braibanti and Joseph Spengler (eds.), *Tradition, Values, and Socio-Economic Development* (Durham, N.C.: Duke University Press, 1961); Myron Wiener (ed.), *Modernization: The Dynamics of Growth* (New York: Basic Books, 1966).

[3] The term "secularization" and others of its order are seen as more specific classes of change within the larger category of cultural change.

more traditional state to a more modern state from one point in time to another. The set of doctrines, values, or beliefs which makes up the "traditional" state is tradition*ism*, while the comparable state at the other end of the continuum—the set of "modern" beliefs and values—is modern*ism*. It is necessary to make this distinction because in discussing certain measures which purport to be indicators of "modernization," we shall find that it is not modernization itself that is measured, but the extent of modernism, or the extent to which individuals hold what are claimed to be modernist values. *Changes* in the proportion of people holding modern values, or changes in the extent to which individuals have "gone modern," constitute modernization. Thus, measuring modernism should be viewed as a means of assessing modernization. The latter, unlike the former, involves a temporal dimension.

The need for reliable and accurate indicators of modernism is thus obvious. Several such measures may be found in the literature, two of which will be reviewed briefly here.

In a recent article on culture change and stress in Peru, Kellert, Williams, Whyte, and Alberti look at the effects of "modernization" on symptoms of stress among a large sample of rural Peruvians.[4] We are here interested only in the three measures of modernism used by these investigators. In the first instance, "modernization" (Kellert's term for our "modernism") is measured by the degree to which individuals take sides with groups in the village representing "old custom" or "modern ideas."[5] A shortcoming of this indicator is that it does not specify the content of the old and the new ways. More serious problems in validity arise, however, with the other two indicators. One of these measures "modernization" by the amount of change in optimism or pessimism regarding the economic progress of the village.[6] The unstated assumption seems to be that the modern individual has the power of positive thinking and believes that everything is always getting better. The third indicator asks respondents whether they think the village is progressing slowly, not progressing, or going backward (no dimension of progress is specified).[7] Frankly, I need some assurance that the population under study adequately shared with the investigators the meaning of such a *yanqui* term as "progress." There is also a question regarding the relationship between perceived progress of local villages and modernism: Is this modernism to the researchers or to the respondents, or to both, or to neither?[8]

Another article, which outlines one of the most ambitious attempts yet to measure modernity, is one in which Smith and Inkeles report the cross-national application of 119-plus items which they conceptualize as relevant to individual modernity.[9] Their definition of "modern" and "modernity," with which this writer would basically agree, must be understood before describing the scales themselves.[10]

> . . . "modern" generally means a national state characterized by a complex of traits including urbanization, high levels of education, industrialization, extensive mechanization, high rates of social mobility, and the like. When

[4] Stephen Kellert, Lawrence K. Williams, William F. Whyte, and Giorgio Alberti, "Culture Change and Stress in Rural Peru: A Preliminary Report," *Milbank Memorial Fund Quarterly*, XLV, No. 4 (October, 1967), 391–415.

[5] *Ibid.*, pp. 407–8.

[6] *Ibid.*, p. 408.

[7] *Ibid.*, p. 409.

[8] Such criticisms are perhaps unduly harsh in view of the primary research interest of Kellert and his colleagues, who are more interested in the correlates of high rates of psychosomatic illness than in modernization. Still, the relevance of the particular indicators of modernism in the study has not been as fully demonstrated as one might desire.

[9] David Horton Smith and Alex Inkeles, "The O-M Scale: A Comparative Socio-Psychological Measure of Individual Modernity," *Sociometry*, XXIX, No. 4 (December, 1966), 353–77. Samples are drawn from six countries: Chile, Argentina, Pakistan, India, Nigeria, and Israel.

[10] Actually, the authors present a long-form scale and several shorter forms consisting of subsets of items.

applied to individuals, it refers to a set of attitudes, values and ways of feeling and acting, presumably of the sort either generated by or required for effective participation in a modern society. In this report we deal only with *individual* modernity.[11]

They proceed to describe the development of the scale:

After reviewing the literature and defining our own theoretical position we identified some thirty topics, themes, areas, or issues which seemed relevant to a definition of modernity.... In this paper, unfortunately, we lack space either to define the content of any of these attitude and behavior realms or to indicate the reasons why we considered them relevant.[12]

This is indeed unfortunate, for, even though the authors promise that explanations will be given in a forthcoming book, one must read the remainder of the article having faith that the areas selected either have some logical connection with the definition of modernity offered or that these areas were found to be meaningfully associated with modernism in the minds of the people to whom the scale was applied.

Application of the scale and the resulting findings lead to the conclusion that "there is an underlying dimension of psychological modernity pervading our set of 119 attitude items."[13] Reliability appears quite high. The question of validity gnaws at me throughout, however. Perhaps it is not the validity of the scale in the minds of the investigators which is open to question so much as the validity of the scale for the various populations and subpopulations studied. Smith and Inkeles are indeed convinced that they have measured what they set out to measure: "We do not see how one could do better within the limits we imposed."[14] What is open to question is whether they set out to measure the right thing.[15]

Such studies as Smith and Inkeles' appear to share another assumption which this investigator feels is open to question: namely, that all modern cultures are basically similar in content and that all persons who can be said to be "going modern" share the same traits, regardless of what culture they are part of or out of what past they have moved. What is defined as modern for one population is assumed to be indicative of modernity for any other. It would appear that the search for change-universals (so that one can emerge from his studies with the conclusion that "Everything is changing in the direction of *X*") has biased measures in favor of this discovery. Whether all peoples in the world are indeed moving from one particular set of values toward another particular set should be left open for empirical proof or disconfirmation.[16]

[11] *Ibid.*, p. 353.

[12] *Ibid.*, pp. 354–55.

[13] *Ibid.*, p. 362. A footnote to this sentence, however, appears to contradict it: "Actually, we have not established that there is one and only one dimension underlying this item set, nor that all types of items enter equally, nor that the main underlying dimension is 'modernity.' But we have established *at least one* pervasive underlying dimension, and on prima facie grounds we can argue from the content of the items that this dimension is psychological modernity as we have conceptualized it. No other explanation, such as response set, will suffice."

[14] *Ibid.*, p. 371.

[15] There is no quarrel with the authors' conceptual definition of modernity; what is not clear is how they got from this abstract definition to the specific indicators of modernity that comprise their scale. Was it through intuition, or logic, or some empirically grounded linkage? Unfortunately, we will have to wait for the book to find out. But one suspects that members of populations later assessed by means of the scale may not have been permitted to suggest what traditionism and modernity meant to them, a shortcoming (and we cannot really be sure it is one in this case) which can have the unfortunate consequence of leaving the scientist with a well-constructed measuring stick which measures we know not what.

[16] A closely related problem associated with measures of modernism which assume universality is that they do not allow for the possibility that traditional-to-modern changes are totally absent from a given population. One may find that he is able to classify personality types into "traditional" and "modern," but finding differentiation in a society does not offer anything but circumstantial evidence for change. The fact of change is assumed,

We might pause to reconsider this position in the light of Lerner's argument, based on research evidence, that "the Western model of modernization exhibits certain components and sequences whose relevance is global."[17] He observes that "the same basic model reappears in virtually all modernizing societies on all continents of the world, regardless of variations in race, color, creed."[18] Lerner's conclusion seems to provide ample justification for the assumption that traits of the modern and the traditional must be shared worldwide. Yet he notes that not every society has accepted the message of modernity in the same way. Of the Middle East he says, "Wanted are modern institutions but not modern ideologies, modern power but not modern purposes, modern wealth but not modern wisdom, modern commodities but not modern cant."[19] While pointing out basic similarities, even Lerner recognizes that the nature of modernism varies according to the specific context.[20]

Hoselitz makes a similar point regarding variations in traditionism and modernism when he says that "culture change is not unilinear, but multilinear." He points out that "different varieties of belief and attitude systems, all of which may be lumped together under the general designation 'traditional,' do exist. Hence the processes of change in these contrasting situations begin from quite different starting points, would show considerable variation, and . . . the outcomes of these processes of change may vary considerably."[21]

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not demonstrated; all we are offered is a static classification of cultural and personality types made by a professional observer.

[17] Lerner, *op. cit.*, p. 46.

[18] *Ibid.*

[19] *Ibid.*, p. 47.

[20] *Ibid.*, p. 47. Also see Eisenstadt, *op. cit.*, p. 1.

[21] Bert F. Hoselitz, "Economic Development and Change in Social Values and Thought Patterns," in George K. Zollschan and Walter Hirsch (eds.), *Explorations in Social Change* (Boston: Houghton Mifflin Co., 1964), p. 679.

C. AN ALTERNATIVE DEFINITION OF MODERNIZATION

A definition of modernization is needed which avoids the assumption that it is a universal process of unilinear change, and which avoids the assumption that the particular value contents of traditionism and modernism are everywhere the same, so that measures which follow from the definitions will not also be based on the same unfortunate assumptions. In this connection, the following definitions are suggested: *Modernization is the movement of persons or groups along a cultural dimension from what is defined by the cultural norms as traditional toward what is defined by the same culture as modern. Those values defined in the local culture as traditional comprise what may be called traditionalism; those defined as modern constitute modernism.* If no differentiation is made in the culture between traditionism and modernism, then movement along the dimension cannot be recognized by the participants, and study of modernization in this cultural setting is meaningless. If modernism and traditionism are seen by the population as different cultural entities, then measurement of change between the two becomes feasible through the measurement of changes in the extent of adherence to the particular value content of traditionism and modernism for that population.

AN ILLUSTRATION: THE SHILOH STUDY

A case illustration may help demonstrate how such a definition can be put to use in actual measurement. It will be reasoned that although comparability across populations is lost, assertions about changes taking place in the population are given some empirical credibility, and assumptions about the universality of modernism and modernization need not be made. The illustration will revolve around the construction of an actual Guttman scale of modernism-traditionism for a particular population.

A. THE SETTING

The population under study is a small community (more precisely, a cluster of

neighborhoods served in common by a school, churches, and stores) in the southern Appalachian mountains. Approximately 200–250 households are located there. The community had been relatively (but never completely) isolated until the 1940's and 1950's, when roads, schools, and mass media effectively raised the "pine curtain" between it and the rest of the country. New occupations in industry have become available to members of the community, while at the same time older forms of work in agriculture, mining, and timber have grown less economically viable. As in most other parts of the Appalachians, the population of Shiloh is almost entirely white and native-born.[22]

B. THE INSTRUMENT

1. *Field survey.*—A four-month field study was conducted in Shiloh in the summer of 1965. Extensive notes were made of conversations, interviews, and observations. Among the varied statements made by the inhabitants of the community were many which indicated in local terms what it meant to be traditional and what it meant to be modern. Some informants referred, for example, to certain "oldtimey ways," or commented that to believe so-and-so was "old-fashioned." Others made reference to things that were "newfangled," "new," or "outside" (a term used synonomously with "modern"—for instance, it was once noted that a certain individual had become "minded to outside ways," that is, he had become oriented to values from beyond the mountains). Thus it was possible later to catalogue over fifty statements taken directly from field notes in which the community itself defined traditionism and modernism.

Information from the field study also made it possible to conclude that it was meaningful to study moderni*zation* as a process occurring in Shiloh, since this kind of change was observed by many respondents. For example, one man noted that whereas some people stayed like their parents, others seemed to learn new ideas. Moreover, some informants marked with regret the fact that many young people were leaving the old ways; others ridiculed people who were old-fashioned and wouldn't, like themselves, become more up to date. Such statements are testimony to the existence of movement from traditionism to modernism in the community; it is a kind of change meaningful to the population under study.

2. *Use of judges.*—In the winter of 1967 the fifty-odd statements referred to above were subjected to judgment by a panel of seven persons who were asked to sort them on the basis of their "traditional" or "modern" content. All the judges could be considered "expert" in the sense that they had had first-hand experience with Appalachian populations and were familiar with the notions of modernism and traditionism. Three of the judges were professors of sociology who can safely be said to be recognized authorities on the Appalachian region. Two judges were Ph.D. candidates in sociology who have lived and worked in southern Appalachia and who can claim personal as well as academic knowledge of the people of that area. The two remaining judges were not sociologists but were individuals who had been exposed at length to the concepts of modernism and traditionism through me. They were called on for judgment primarily because of their lifelong residence among mountain people and their untrained natural capacity for analysis of those people.

The judges were required to sort statements into three piles: one which indicated a clearly "modern" attitude, one which clearly showed a "traditional" attitude, and one which showed neither and was therefore ambiguous. While systematic inquiries were not made into the criteria of classification held by the judges, comments occasionally were recorded which offer some insight into these standards. For example, one judge observed that the task of classifying statements was clouded by mental reference to

[22] A more complete description of the community and the changes it has undergone since 1940 may be found in my forthcoming (1968) book, *Shiloh: A Mountain Community* (Lexington, Ky.: University of Kentucky Press), especially chap. i.

particular individuals who had made such statements. Another noted that it was difficult to separate the traditional-modern dimension from a dimension of social class. One judge raised the possibility that there were several types of modernism rather than just one, a fact which raises doubts about the unidimensionality of the items in his mind. One of the non-sociologist judges admitted that he had been influenced by me in his conception of modernism and traditionism, but said also that his decisions were guided by "particular stereotypes" built from his own experience. Again, another of the sociologists said he was "confused because many items are multidimensional (indeed, modernism is multidimensional)." He also encountered frustration in attempting to keep modernism-traditionism distinct from social-class differences. By inference from these observations, it appears that among the criteria used for classification by these judges were reference to particular known individuals, value differences related to social class,[23] and conceptions obtained from me. (It appears, furthermore, that there was some discomfort among the judges when they were required to assume unidimensionality of modernism among an Appalachian population.)

Prior to the judging, each statement was coded to indicate which of seven "value areas" it represented: time, achievement, work, education, person-versus-object orientation, religion, sex role orientation. After the judging was complete, the two statements on which there was highest agreement were chosen from each value area to be placed in the final instrument.[24] With the addition of another last-minute insertion, the total battery of value statements included was fifteen[25] (see Table 1).

3. *The larger instrument.*—The interview schedule of which the fifteen-item modernism-traditionism battery was a part was constructed for the primary purpose of collecting data in order to test hypotheses about modernism, marginality, and mental health. Interviews lasted between thirty and forty-five minutes on the average and were conducted for the most part in the homes of respondents.

4. *The sample.*—The sample consists of 130 respondents who are members of "whole" families with children living at home. The sample is almost coterminous with the number of people in this category who live in Shiloh, the remainder of the population being made up of older residents and couples who have retired here from "outside." The rate of refusals, most of them by females, was around 20 per cent. Where men and their wives were both interviewed, they were questioned simultaneously in different rooms by an interviewing assistant and myself. In only a few instances were the couples interviewed sequentially by the same interviewer.

The sample includes seventy-one males and fifty-nine females. Occupations of heads of households ranged from professional, managerial, and white collar (20 respondents), through full-time blue collar, such as factory work (40 respondents), and seasonal blue collar, semiskilled and unskilled work, such as farming, mining, and timber (59 respondents), to no significant gainful employment or dependence on agency support (11 respondents). Sixty respondents could be classified as upwardly mobile, in that their jobs were higher on the above

[23] The extent to which modernism is related to social class has never been made clear. Some have said that it *is* social class, while others have maintained that the two are separate dimensions. While I tend to hold the latter view, I know of no evidence which has direct bearing on the issue. This will be the subject of later study.

[24] Placing an item into the "ambiguous" pile was considered an error and was scored "1." Placing of what was intended to be a modern or a traditional statement into the opposite pile was also considered an error and was scored "2" on the assumption that it is an even less clear indicator than had it been placed in the "ambiguous" pile. Total error scores for each statement range theoretically from 0 to 14. Two of the items used had error scores of 0, ten had error scores of 1, and five had error scores of 2.

[25] A fifteenth statement was inserted in the instrument at the last minute without adequate justification. Interestingly, it was one of those discarded during construction of the scale.

fourfold occupational scale than the jobs of their fathers. Fifty-three could be labeled non-mobile by the same criterion, and twelve were downwardly mobile.[20] These figures reflect a change in the occupational structure affecting the people of Shiloh as the community becomes "industrialized."

C. CONSTRUCTION OF THE SCALE

A Guttman scale based on six of the original fifteen traditional-modern items was constructed with the use of the Gradgram instrument.

1. *Elimination of items.*—The fifteen items were arranged in order from highest to lowest total score (traditional responses were scored "0"; modern responses, "3"; middle categories, "1" and "2," respectively). Answer categories for each item were then collapsed so that each item was dichotomized to produce more non-error than error in all categories, in accordance with usual scalogram procedure. This rule could not be met on three of the items (numbers 10, 1, and 2 in Table 1), and they were therefore discarded at this point.

Respondents were rearranged so as to minimize error with the new set of twelve items. It was clear that three more items would have to be discarded because they contributed so heavily to the over-all number of errors (numbers 15, 12, and 8 in Table 1). The remaining nine items and the respondents were rearranged again in line with scalogram procedure, with a resulting coefficient of reproducibility of .884. It was obvious from looking at the proportion of errors in each item that number 4 was contributing more than its share. With its elimination, the coefficient of reproducibility rose to .901.

One of the several criteria applied when assessing Guttman scales is that at least 5 per cent of the respondents must fall in each scale type. This is the same as requiring "spread" among the scale cut points.

TABLE 1

VALUE STATEMENTS INITIALLY INCLUDED IN INSTRUMENT

Item No.	Statement*	"Modern" Response
1.........	A person should improve his living conditions even if he has to go into debt to do it.	Agreement
2.........	A person should stand up for his kin even when the law says they are in the wrong.	Disagreement
3.........	The old, small neighborhood school was better than the new consolidated schools.	Disagreement
4.........	There is a conflict between religion and science.	Disagreement
5.........	You can't make progress without change.	Agreement
6.........	I would rather be a person who tries to make do with what he has; being dissatisfied all the time just leads to problems.	Disagreement
7.........	I think the old ways are mostly best for me.	Disagreement
8.........	A person really shouldn't have to work any more than he has to to get by.	Disagreement
9.........	Consolidating the schools has improved educational opportunities for people like me and my children in almost every way.	Agreement
10.........	A person's job is so important that sometimes he has to turn his back on his family and friends.	Agreement
11.........	The old Bible (the King James version) is the only true word of God.	Disagreement
12.........	If a woman votes, she should probably vote the same way as her husband.	Disagreement
13.........	Planning a career is as important a responsibility as raising a family.	Agreement
14.........	Women should be allowed to "wear the britches" more (or have more say) than they have in the past.	Agreement
15.........	By and large, I am satisfied with my way of life.	Disagreement

* The answer categories used for all items were: Agree strongly, agree somewhat, disagree somewhat, disagree strongly.

[20] Five individuals did not give sufficient information for mobility to be measured.

This requirement could be met better and the scale enhanced with the removal of item number 13, which, when accomplished, left seven items and a coefficient of reproducibility of .919.

It was noted that two of the seven items tapped virtually the same value area, education. If one of these two were removed from the scale, not only would cutting points between scale types be more evenly distributed, but the six remaining items would each tap a different value area and no two would tap the same one. (The only one of the original seven value areas included in the full battery which was *not* represented in the final scale was "work.") Therefore, item number 9 was eliminated from the scale.

2. *Data on final six-item scale.*—After items and respondents were rearranged for the final time, the coefficient of reproducibility was .916. The proportion of respondents in each scale type is about the same, as shown in Table 2, which indicates that cutting points are well distributed. Two of the cutting points are near the center of the scale, a feature regarded as desirable by some scalogram analysts.

TABLE 2

THE SIX-ITEM MODERNISM-TRADITIONISM SCALE

	Item Number (+="Modern" Response)						Distribution of Scale Types			
	1	2	3	4	5	6	Non-Scale Types	Perfect Scale Types	Total	% of Respondents
1*	+	+	+	+	+	+	4	12	16	13.4
2	−	+	+	+	+	+	5	10	15	12.6
3	−	−	+	+	+	+	8	7	15	12.6
4	−	−	−	+	+	+	4	17	21	17.7
5	−	−	−	−	+	+	5	14	19	16.0
6	−	−	−	−	−	+	9	7	16	13.4
7	−	−	−	−	−	−	12	5	17	14.3
Total	......	......	......	......	......	......	47	72	119	100.0

	Item Contents	Value Areas
1	"Women should be allowed to 'wear the britches' more (or have more say) than they have in the past."	Sex roles
2	"The old Bible (the King James version) is the only true word of God."	Religion
3	"I would rather be a person who tries to make do with what he has; being dissatisfied all the time just leads to problems."	Achievement/satisfaction
4	"I think the old ways are mostly best for me."	Orientation to past
5	"The old, small neighborhood school was better than the new consolidated schools."	Education
6	"You can't make progress without change."	General value of change

* Scale runs from 1 (most modern) to 7 (most traditional).

Out of the 119 usable respondents scaled, 72 (60.5 per cent) were pure scale types, while 47 (39.5 per cent) were error types. The scale type with the largest proportion of error types was 7, which is the "most traditional" category. That there is patterned error here is suggested by the presence of six cases among the twelve error types in which the error was in the first column. Since the first item has to do with changing

sex roles, it is a reasonable guess that these "errors" are made by females who are otherwise tradition-oriented. A large number of error types were found also in scale type 6. Only one of the errors is accounted for by item 1, but four are cases where there is an error in item 4, concerning orientation to the past. The only other instance of a large number of error types was in scale 3. Of the eight error types, four are ones in which an error occurs in the first item (changing sex roles), and four in which an error occurs in item 4 (orientation to the past). Thus, most of the scale impurities can be attributed to these two items. Patterned error in one of them (changing sex roles) suggests that the same dimension may not be appropriate for scaling both men and women.

The *error ratio* was computed by first computing the proportion of errors expected by chance[27] and then dividing that number into the proportion of actual errors. An acceptable error ratio is suggested by Riley to be anything less than .50. The error ratio for the present scale was .14.

The coefficient of scalability was computed at .621. Menzel suggests that the level of acceptance for this coefficient "may be somewhere between .60 and .65."[28]

Thus it appears that, by all the customary criteria, this set of six items and 119 respondents forms an acceptable Guttman scale. What does it mean?

3. *Interpretation of scale.*—It is said that although Guttman intended scalogram analysis to be a method whereby the unidimensionality of items could be demonstrated, scalability does not necessarily mean unidimensionality. There must be some theoretical or intuitive basis for asserting that items are selected from the same universe of content, in addition to meeting the more formal scalogram criteria. This raises an interesting question about the present scale, the six items of which are shown in Table 2. Are these from the same universe or content? Is this a unidimensional scale? Or is it simply an accidental artifact of the sampling of complex, unrelated cultural patterns? If there were no other evidence bearing on the matter, one would be tempted to dismiss this "scale of traditionism-modernism" as a mere artifact. There is, however, the knowledge that this particular set of items was chosen initially on the basis of its representativeness of a single universe or discourse. There is the further fact that these items were judged by seven knowledgeable persons to be the most accurate reflections of that dimension among the total pool of items. There was good reason, in short, to suspect that at least some of these value statements would belong on the same dimension, the dimension of traditionism-modernism.[29]

Even having a priori justification in addition to the confidence born of having met scalogram criteria of unidimensionality does

[27] The coefficient of chance reproducibility itself was .407. It was computed by following the procedure described in Matilda White Riley, *Sociological Research: I. A Case Approach* (New York: Harcourt, Brace & World, 1963), pp. 476–77.

[28] Herbert Menzel, "A New Coefficient of Scalogram Analysis," *Public Opinion Quarterly*, XVII, No. 2 (Summer, 1953), 279.

[29] One would logically expect respondents of the more traditional scale type to be older, less educated, living farther from main roads, and in more traditional occupations which are seasonal or occasional and outdoor in nature. Fortunately, information on some of these variables was available. Looking only at extreme scale types 1 and 2 (most modern) and 6 and 7 (most traditional), it was found that 50.0 per cent of the 1's and 2's were over 39 years of age, while 40.6 per cent of 6's and 7's were over 39 years old. With regard to location of residence, 59.4 per cent of the 1's and 2's lived "on or near main highway," 31.3 per cent "off main highway but not isolated," and 9.4 per cent "relatively isolated or at end of road." Comparable percentages for 6's and 7's were 18.8, 53.1, and 28.1, respectively.

Occupations were classified into four categories: I.—White collar or managerial. II.—Full-time blue collar (such as factory work). III.—Part-time or seasonal blue collar and unskilled (such as mining, timbering, small-time commercial farming). IV.—Occasional or no employment, or agency support. When occupational categories I and II (more modern) and III and IV (more traditional) were combined, it was found that 71.0 per cent of scale types 1 and 2 were "more modern" in occupation, while 66.7 per cent of scale types 6 and 7 were "more traditional."

Thus, the scale appears validated with regard to location of residence and occupational type. No

not permit the conclusion that these value statements are linked consciously in the minds of the people of Shiloh in the fashion suggested by the scale. But whether conscious or not, the scale of items does suggest that there is a set of value "steps" which a person in Shiloh takes when leaving traditionism and entering modernism. First, he must agree that change in general is valuable, that "You cannot make progress without change." As a second step, he will probably note that older forms of education, while more comfortable, are not as effective as newer forms; he will not agree that "The old, small neighborhood school was better than the new consolidated schools." Next, he will give up his orientation to the past and will not agree that "the old ways are mostly best for me." Achievement orientation will probably change next, and the willingness to defer gratification, as expressed by disagreement with the statement, "I would rather be a person who tries to make do with what he has; being dissatisfied all the time just leads to problems." The next-to-last value area to change appears to be religion, at least as measured by disagreement with the statement that "The old Bible (the King James version) is the only true word of God." And last, one has become fully modern (by this set of items) when he accepts a more emancipated role for females: "Women should be allowed to 'wear the britches' more (or have more say) than they have in the past."

CONCLUSIONS

The traditionism-modernism scale from Shiloh illustrates the kind of measurement procedure which must be followed if we are not to assume what we set out to prove. Data from an earlier field survey indicated that modernization, as we have suggested that it be defined, is occurring in the community, and that study also provided a source from which to draw a pool of value statements comprising a single "universe of content." The use of judges gave even more assurance that the items used were valid indicators of traditionism and modernism. The fact that six of the fifteen original items form a Guttman scale makes more credible the contention that we are measuring something real "out there," and that that something is what we thought it was. We have not merely *imposed* a measure of modernism on the population under study, but have allowed the population to tell us whether modernism exists and what form it takes.[30]

To anticipate criticism, the heaviest argument against following this kind of procedure may also turn out to be an argument in favor of it. The greatest weakness of the present scale, it will probably be maintained, is that it obtains only for the population under study. Following the same procedure with any other population would probably yield a different set of value statements and hence a different scale. Therefore, with this type of measure of modernism, it becomes impossible to compare stages of modernization across cultural groups, something which would appear highly desirable. But is it realistic to expect that modernization *could* be comparable from one culture or society to another, or is it not at the very least an empirical question? For if we assume that modernism measures are comparable and valid cross-culturally, are we not forced to assume (1) that traditionism consists of the same set of values and beliefs wherever it is found,[31] (2) that the same path is followed from traditionism to mod-

explanation can be offered for the lack of differentiation in ages (or even a slight reversal of expectations). Since the scale appears valid by other criteria, however, one might conclude that individuals in Shiloh do not become more traditional as they grow older.

[30] At the risk of being unduly repetitive, it will be pointed out that I make a claim, not to have studied moderni*zation*, but to have devised a way of meaningfully measuring moderni*sm* which can later be used to gauge the extent of change over time. The present study (like most such research) is not a study of change because it omits the temporal dimension.

[31] If this is true, then the most "traditionism" (or "modernism") can mean is "the belief in tradi-

ernism, via the same value "steps," and (3) that modernism consists of the same set of values and beliefs wherever it is found? How many would be willing to make those assumptions?

Therefore, it may not be proper, even if it appears possible, to compare the traditional-modern standing of one group with that of another. The technique illustrated here will permit such comparison when it is justified.

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tion (or modernity), whatever that may be." We have seen that modernism means more than that to at least a few students of the subject. It seems clear that either the terms must be defined so generally as to be useless or the specific content must be left to vary from culture to culture.

Patterns of Name Ordering among Authors of Scientific Papers: A Study of Social Symbolism and Its Ambiguity[1]

Harriet A. Zuckerman

ABSTRACT

With increasing scientific collaboration, visibility of individual role-performance has diminished. Ordering of authors' names is an adaptive device which symbolizes their relative contributions to research. Interviews with Nobel laureates and comparisons of their name-order practices to those of other scientists suggest that this symbol is ambiguous and makes evaluation of individual role-performance difficult. A probability model of expected distributions of name orders is used in measuring preferences for particular sequences, and these preferences vary with the authors' eminence. On the assumption that authors' names are listed in order of the value of their contributions, laureates should be first-authors more often than other scientists; in fact, they are not. Instead, they exercise their *noblesse oblige* by giving credit to less eminent co-workers increasingly as their eminence grows. They do so more often after the prize, and eminent laureates-to-be forego first-authorship more often than those as yet unrecognized. This *noblesse oblige*, however, has its limits; laureates' contributions to prize-winning research are more visible than contributions to their other research.

Every social institution provides for the evaluation of role-performance within it. Assessment of the quality, quantity, and style of role-performance is required for social systems to operate with some degree of effectiveness. Evaluation is both a component of social control which curbs excessive departures from institutional norms and a component of the reward system which responds to variations in role-performance by promoting, demoting, retaining, and removing participants from particular social systems. Evaluation, however, requires that role-performance should be visible in some degree. All this holds in the institution of science just as it does in the institutions of religion, politics, the arts, and the economy.[2]

In science, evaluation is based on the extent and quality of scientific contributions as appraised by significant others in the field. This is nothing new. What is new is the profound change in the social organization of scientific work which, in many cases, reduces the visibility of role-performance of individual investigators. In the past, there were, as now, discrepant judgments about the quality of a scientist's work, but rarely was there any ambiguity about who had done what. A scientist may have had technical assistants working with him, but he was unambiguously *the* investigator, not merely the "principal investigator." When he put his findings into print, he was, with few exceptions, the sole author. There could be no doubt whose scientific performance was being assessed.

As the social organization of scientific work has become more complex—more often collaborative and sometimes intricately organized—the visibility of *individual* role-performance is reduced. The

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[2] For a systematic discussion of the relations between visibility of role-performance, evaluation, and social control, see R. K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1957), pp. 320 ff.

changed organization is reflected in the rapid increase of multi-authored papers. As Derek Price, Bernard Berelson, and I have variously reported elsewhere,[3] about 80 per cent of papers in chemistry are now multi-authored (a fourfold increase since the turn of the century), 60 per cent in physics and 40 per cent in the biological sciences, in contrast to the 1 per cent of papers in such humanistic subjects as philosophy, language, and literature. Such papers, of course, introduce ambiguity about the respective contributions of the joint authors.

This ambiguity has interfered with the operation of the reward system in science which is now, as it always has been, better geared to allocate credit to individual scientists than to groups of them.[4] Collaborative research is, in varying degrees, a functional requirement of contemporary scientific investigation. At the same time, rewards in the form of promotions, prizes, and resources for further work typically go to individuals. This malintegration of the reward system and the social organization of scientific research is reflected in the difficulties of finding ways to allocate relative responsibility and credit for particular investigations among collaborators. When difficulties persist in a changing social system, adaptive mechanisms, effectual and ineffectual, designed to cope with them typically develop. And so it is here. As the number of multi-authored papers increased and as the number of co-authors (what I call the author-set) increased, new practices were introduced to lessen the ambiguity about the distinctive roles of co-authors in doing research and in preparing published papers. Various patterns of name ordering of co-authors were evolved for this purpose.[5]

This is just the sort of device that produces ambivalence. Intrinsically, it seems trivial or humiliating to be concerned with the order in which the names of authors appear; for scientists to be as interested in the order of their billing as actors or business partners. But symbolically, name ordering is an adaptive device that facilitates the allocation of responsibility and credit among co-workers in otherwise ambiguous situations induced by the new structures of scientific research. It is designed to have the reward system operate with a degree of equity and adequacy. The ambivalence toward name ordering is only one more instance of scientists' ambivalence toward recognition for their work and the means used to achieve it.[6]

Beyond this, two aspects of the ordering of authors' names need only be mentioned here. First, name orders convey ambiguous information about the relative contributions of co-workers since, as we shall see,

[3] Derek J. deS. Price, *Little Science, Big Science* (New York: Columbia University Press, 1963), pp. 88 ff.; Bernard Berelson, *Graduate Education in the United States* (New York: McGraw-Hill Book Co., 1960), p. 55; Harriet Zuckerman, "Nobel Laureates in the United States: A Sociological Study of Scientific Collaboration" (unpublished Ph.D. dissertation, Columbia University, 1965), chap. iii. Others have reported similar findings for limited samples and time periods. See Zuckerman, *op. cit.*, for a list of these.

[4] Scientists are not unaware of the incongruity of the organization of contemporary science and the allocation of credit to individual scientists. Since it is difficult indeed to assess the particular contributions of collaborators, one physicist facetiously suggested that research groups in his field be organized into teams with the not improbable titles: "The Harvard-M.I.T. Yankees" and "The 'Reds' of Dubna." The standing of each in its own league could then be computed and rewards properly allocated (R. E. Weston *et al.*, "Modest Proposal," *Physics Today*, XV [June, 1962], 79–80).

[5] It may be of interest that this study was under way at the very time that Norman Kaplan was observing with regret that "there has been no known systematic study of the norms pertaining to . . . the arrangement of names in multiple-authored papers" ("Sociology of Science," p. 857 in R. E. L. Faris, [ed.], *Handbook of Modern Sociology* [Chicago: Rand McNally & Co., 1964], pp. 852–81).

[6] See R. K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review*, XXII (December, 1957), 635–59, for his analysis of the tension between institutional norms requiring humility and the emphasis placed on making a distinctive contribution to science.

customary practices differ among disciplines and within them, and since name orders, like other symbols, are sometimes adapted to serve purposes other than their manifest ones. As a result of this ambiguity, circles of scientists ascribe their own meanings to sequences of authors' names, meanings that often diverge from those intended by the authors themselves. For example, textual references to scientist "X and his collaborators" when scientist X is listed third or fourth in a long list of names are indicators that scientist readers sometimes ignore name orders in seeking to allocate credit and responsibility among co-workers. In the process, it is not unusual for scientists to request and to receive information about "who did what" through informal channels of communication.[7]

Little is known about actual patterns of name ordering employed by scientists of varying degrees of eminence and in various fields. To get some leverage on these questions, patterns of name ordering found on the scientific papers of forty-one of the fifty-five Nobel laureates living in the United States are compared to those on a random sample of articles listed in the abstracts of physics, chemistry, and the biological sciences from 1920 to 1964—the period corresponding to the dates of the laureates' publications.[8] Although the author of a particular article appearing in the abstracts may be as eminent as a laureate, on the average, randomly selected authors are, of course, far less distinguished and represent the average scientists publishing in these fields.

Using these data, I can systematically compare name-ordering practices among scientists of varying eminence in different fields. Later on, I shall consider the practices of men at different stages of the scientific career.

PATTERNS OF NAME ORDERING

Three principal patterns of name ordering are used in multi-authored papers. The first may be described as the "equality pattern" since it is designed to put all coauthors on a plane and, so far as possible, not to distinguish their respective contributions. This type consists of alphabetized and reverse alphabetical sequences according to the initial letters of authors' last names. A classically literal case of alphabetizing was contrived not long ago by the trio of physicists who signed their paper Alpher, Bethe, and Gamow.[9]

The second consists of patterns which enhance the visibility of one or another member of the set of authors. These also take two principal forms: the first-author-out-of-sequence followed by an alphabetized group (thus: ZABC), and the last-author-out-of-sequence preceded by an alphabetized list (thus: XYZA). In both forms, the out-of-sequence author appears as a figure against a ground. He stands in contrast to the others who appear as mere elements of an alphabetical pattern.[10]

[7] Although laureates agree that discussion of differential contribution is permissible when generous recognition is given to other, the norm of privacy is considered legitimate. A physicist laureate put it this way: "Littlewood said that one of the primary rules governing collaboration is that one must not reveal to the external world who did what. I think he is correct . . . (*a*) because one's recollections are not always accurate and (*b*) there is no reason for doing so. I think that if everybody abided by this rule, in general, collaborations would be happier." He refers to the somewhat less explicit remark by Littlewood in the mathematician's review of joint work by Ramanujan, Hardy, and Littlewood (J. E. Littlewood, *A Mathematician's Miscellany* (London: Methuen & Co., 1953), p. 89.

[8] Articles listed in *Science Abstracts* (physics), *Chemical Abstracts*, and *Biological Abstracts* were randomly sampled for two years of each decade from 1920 to 1959 and for two years in the period 1960–64. Since multi-authored papers were comparatively rare in the first two decades, these were oversampled. Absolute numbers of papers do not represent actual distributions of multi-authored papers.

[9] R. Alpher, H. Bethe, and G. Gamow, "The Origin of Chemical Elements," *Physical Review*, LXXIII (1948), 803.

[10] There are, of course, variations on this theme. A group consisting of two senior investigators and several junior colleagues may list the names of the senior men first or last and then alphabetize names of the juniors. To simplify coding, these were treated as alphabetically random sequences.

The third major type of ordering usually gives prime visibility to the first-author and smaller increments of visibility to each succeeding author. This is the alphabetically random pattern. As we shall see, names are arranged according to definite criteria other than the arbitrary one of alphabetical order.

Each pattern requires separate consideration.

DIFFERENTIAL USE OF NAME-ORDER PATTERNS

ALPHABETICAL SEQUENCES

At first appearance, Table 1 seems to indicate that laureates are a little less apt to use the alphabetical equality pattern than their less distinguished counterparts: 39 per cent of papers by prize winners and 46 per cent of the others carry alphabetic orders. What we actually find are greater differences in the use of the pattern among different disciplines. Alphabetizing is more frequently used in physics than in chemistry or the biological sciences,[11] and it is much more often used by physicist laureates than by rank-and-file physicists. In chemistry and biology, by contrast, laureates are slightly less apt to signal equality of contributions by using alphabetical listings. As I shall show, Nobelists in these fields attempt to confer credit upon their co-workers in other ways.[12]

Obviously, alphabetical sequences do not always symbolize equality of contribution. In some cases, the alphabetical order will coincide by chance with the order of differential contributions. A chemist lau-

TABLE 1

PERCENTAGE OF PAPERS WITH ALPHABETIZED NAME ORDERS BY LAUREATES AND A SAMPLE OF AUTHORS LISTED IN ABSTRACTS ACCORDING TO FIELD AND SIZE OF AUTHOR-SET

	SIZE OF AUTHOR-SET					
	2	3	4	5	6+	All
Physics:						
Laureates.......	71 (398)	55 (147)	63 (58)	54 (46)	83 (46)	67 (695)
Abstracts........	64(1,410)	38 (643)	35(142)	39 (38)	26 (34)	57 (2,267)
Chemistry:						
Laureates.......	52 (638)	17 (337)	12(119)	6 (49)	11 (35)	35 (1,178)
Abstracts........	55(3,519)	25(1,162)	14(305)	14 (81)	12 (58)	45 (5,125)
Biological sciences:						
Laureates.......	47 (750)	19 (380)	4(186)	1(100)	1 (78)	29 (1,494)
Abstracts........	55(2,802)	22(1,061)	10(347)	2(112)	6 (63)	41 (4,385)
All:						
Laureates.....	54(1,786)	24 (864)	16(363)	15(195)	27(159)	39 (3,367)
Abstracts......	57(7,731)	27(2,866)	16(794)	12(231)	13(155)	46(11,777)

[11] It turns out that the specialties of chemistry follow the patterns of their respective neighbor sciences: laureates working primarily in physical chemistry, like the physicists, are more likely to alphabetize authors' names than are biochemists who choose patterns much like the biologists. Forty-eight per cent of physical chemists' papers are alphabetized compared to 28 per cent of papers by biochemists.

[12] Although differences between fields are significant, we are not yet in a position to explain them satisfactorily. Data on the types of collaboration in which laureates have been involved suggest that the rank structure of research groups may account for them. Physicist laureates, for example, are more apt to have worked with peers than are chemists and bioscientists whose groups tend to be comprised of one senior scientist and a number of juniors. Data on the distribution of types of collaborative groups in the fields being considered are needed to test this hypothesis (see Zuckerman, *op cit.* [n. 3 above], p. 313).

reate reported this familiar type of case from his own experience. "This paper I was co-author of did not have this authorship question in it . . . because . . . the sequence of authors in terms of contribution was identical with the alphabetical order and this put me first which is unusual [considering that] my name begins with T."

Scientists are abundantly aware of this type of ambiguity. In many cases, the manifest "equality" pattern supplies misinformation. One of the several laureates who collaborated with Enrico Fermi emphasized the frequency of this symbolically inept sequence when working with the great physicist: "Anytime I did something . . . with Fermi [and the group], although we always co-authored these things . . . in alphabetical order . . . [it] has to be understood that Fermi did the lion's share of the work."

A second artifact that makes for ambiguity in the meaning of alphabetical orders is their increasingly preferred use as the number of co-authors increases, since the task of ranking the contributions of many authors, say, the laureates, is then especially difficult, unpleasant, and apt to induce conflict. A physicist put it this way: "When a lot of people are involved . . . the alphabetical order . . . certainly simplifies matters. And I just don't like this weighing of each person's contribution. It is almost impossible to do." In these cases too, the reader of scientific papers is confronted with alphabetical orders which do not convey the unequal contributions of co-authors.[13]

Table 1 cannot tell us whether there is a preference for alphabetical name orders. This is evident in the case of papers by two authors. In these papers, if no preference for name ordering were being exercised, names on about half of the papers would be alphabetized and the other half would be reverse alphabetical. With three-author papers, in the absence of choice, about one-sixth would have alphabetized orders, another sixth, reverse alphabetical, and so on as summarized in Table 2. A measure of preference for alphabetized orders—or any one of the other name-order patterns—is therefore provided by the ratios of observed frequencies of papers to the number of papers that would be expected if no preferences were being exercised.

In Table 3, we see that preference for alphabetized name orders becomes noticeable among laureates and the sample of authors listed in the abstracts when papers have four or more co-authors. The tendency to choose alphabetical orders as the size of the author-set increases is much the same among both groups. With four-author papers, both choose to alphabetize about four times as often as would be expected if this were not a matter of deliberate decision. The ratios rise rapidly for five-author papers to nineteen times as many papers by the laureates and sixteen times among the others. Finally, when papers have six or more authors, alphabetizing occurs 270 times as often as would be expected (if it were random rather than preferential) for the laureates and 133 times for other scientists. Ratios based on small expected frequencies tend to be inflated, of course, but it is clear that alphabetizing is preferred as the size of the author-set increases beyond four.[14]

There is also an increase, though less marked, in preference ratios for patterns which heighten the visibility of first- and last-authors. These will be examined later. The alphabetically random sequence of names—presumably reflecting deliberate se-

[13] At the same time, the intent of authors becomes more visible as the number of authors increases. A two-author paper may indeed be alphabetized without this being unambiguously identified as the principle governing the two-item sequence. With three authors, alphabetizing becomes perceptible, and this increases with the number of authors up to a point after which the addition of items does not add extra information. This, of course, is simply a special instance of the Weber-Fechner principle of perception of patterns.

[14] Both laureates and the sample of authors listed in abstracts choose the alphabetical order significantly more often than would be expected. $\chi^2 = 50.6$ for laureates, $\chi^2 = 54.6$ for sample, 1 d.f., $p < .001$ for both groups.

quences gauged in terms of relative contribution—occur somewhat less often in successively larger author-sets than would be expected if no preferences were being exercised.[15]

[15] These are nonetheless the most numerous in absolute terms.

So much for the comparison of actual authorship practices with patterns which would occur if authors were not deliberately choosing some sequences rather than others. We still must deal with the question of whether laureates' selections of name-order patterns are different from those of

TABLE 2

PROBABILITIES THAT PARTICULAR PATTERNS OF NAME ORDERING WILL APPEAR ON MULTI-AUTHORED PAPERS IN THE ABSENCE OF EXERCISED PREFERENCES

ORDERING PATTERNS	NUMBER OF AUTHORS				
	2	3	4	5	6
Alphabetical	.500	.167	.042	.008	.001
Reverse alphabetical	.500	.167	.042	.008	.001
Alphabetical except for first name	....	.333	.125	.033	.007
Alphabetical except for last name	....	.333	.125	.033	.007
Alphabetically random	....	....	.667	.917	.983

TABLE 3

RATIOS OF OBSERVED TO EXPECTED FREQUENCIES OF PAPERS WITH DIFFERENT NAME-ORDER PATTERNS BY LAUREATES AND A SAMPLE OF AUTHORS OF PAPERS LISTED IN ABSTRACTS

NAME-ORDER PATTERNS	NUMBER OF AUTHORS				
	2	3	4	5	6+*
Alphabetical:					
Laureates	1.1	1.5	3.9	19.3	270.4
Abstracts	1.1	1.6	3.9	15.6	133.3
Reverse alphabetical:					
Laureates	0.9	0.9	0.7	4.0	‡
Abstracts	0.9	0.9	0.9	0.6	‡
X+Alphabetical:†					
Laureates	....	0.9	1.0	2.3	8.1
Abstracts	....	0.9	1.1	1.7	8.2
Alphabetical+X:†					
Laureates	....	0.8	0.9	1.2	3.6
Abstracts	....	0.8	1.2	0.8	‡
Alphabetically random:					
Laureates	....	....	0.8	0.8	0.7
Abstracts	....	....	0.8	0.8	0.8
Number of papers:					
Laureates	(1,786)	(864)	(363)	(195)	(159)
Abstracts	(7,731)	(2,866)	(794)	(231)	(155)

* The incremental probabilities governing the expected frequencies of papers with seven or more authors are so small that we used the probabilities of a six-author paper as the base for computing the number of expected papers.

† These refer, respectively, to the pattern of first-author-out-of-sequence followed by an alphabetical sequence and the last-author-out-of-sequence preceded by an alphabetical sequence.

‡ No cases appeared in these cells.

rank-and-file scientists. Taking all multi-authored papers by both groups and classifying them according to patterns of name ordering, we find that these distributions differ significantly from one another.[16] Table 3 shows that the differences between eminent scientists and their less celebrated colleagues are concentrated in papers with at least five authors,[17] and in particular in the choice of alphabetical orders.[18] Although available data do not permit us to test the validity of any explanation of differences, it is, as we shall see, consistent with other evidence we have, to assume that these differences can be accounted for in two ways. First of all, the laureates' names are so familiar to the scientific audience that they sacrifice relatively little by having their names alphabetized along with those of several other scientists. Wherever their names come in these sequences, they are visible.[19] In this respect, and as we shall see in others as well, adopting conventions which do not signal their pre-eminence is nearly cost-free. And second, the chances are that laureates, even when they are very young or very old, contributed enough to particular researches to be considered by their co-workers as equals. In any case, we find that laureates are no more apt to have their names in sequences which emphasize the contributions of particular authors than are rank-and-file scientists—with the exception of the laureates' greater use of the last-author-out-of-sequence pattern on papers with many authors. In this sense, the elite has much the same preferences with regard to name ordering as other scientists on the majority of their published papers. But as we shall see, laureates differ from their colleagues in the positions they occupy in name orders rather than in their preferences for particular sequences as such.

Special situations affecting the ambiguity of alphabetical name orders.—When alphabetized name orders are adopted as a matter of custom,[20] scientists whose names begin with letters late in the alphabet and those who collaborate with distinguished men are consistently penalized.[21] One physicist laureate spoke out of his own experience about the consequences of persistent alphabetizing for men whose names come at the end of the alphabet: "Since publication is such an important source of judgment of the individual's work . . . if a given person is always put [late in lists] . . . there is a tendency for people to think that he is just a subsidiary all the time."

The laureates imply that the allocation of credit to well-known scientists for particular contributions—at the expense of subordinates—is especially problematic in the

[16] $\chi^2 = 163.7$, 4 d.f., $p < .01$. The χ^2 value is inflated by the absolute size of the samples being considered.

[17] Differences between the two groups on selection of name-order patterns on papers with fewer than five authors are not significant. χ^2 for four-author papers = 2.54, 4 d.f., $p > .10$. For papers with five authors, $\chi^2 = 8.65$, 4 d.f., $p < .10$; and for papers with six or more authors, $\chi^2 = 14.63$, 4 d.f., $p < .01$.

[18] Examination of χ^2 computations indicates that the bulk of the difference between the two groups of scientists on papers with five or more authors is concentrated in the differences between them in their tendency to alphabetize; more than 60 per cent of the χ^2 value is due to this difference.

[19] We shall have more to say on the matter of name ordering and visibility of scientists. It is sufficient here to note that Stephen Cole and Jonathan Cole find no consistent relationship between the visibility of physicists' work and their positions in name-order sequences and that the laureates' comments are compatible with the Coles' findings ("Visibility and the Structural Bases of Awareness in Science," *American Sociological Review*, XXXIII [June, 1968], 387–413). A laureate in medicine and physiology described it this way: "You usually notice the name that you're familiar with; even if it's last, it will be the one that sticks."

[20] In some circles, this convention has become normative; one investigator remarked: "Gentlemen publish with names in alphabetical order" (G. R. Wendt, *Science*, CXLV [July 10, 1964], 110–12).

[21] This has become more acute as automated information retrieval systems are designed to keep track of the names of the first two or three authors and to anonymize the others in the category of *et al.*—"that club of scientific unmentionables whom the world is too busy to notice" (Editorial, *Journal of the American Medical Association* [May 11, 1963], p. 497).

case of alphabetical sequences. This is one version of the Matthew Effect described by Merton as "the accruing of greater increments of recognition for particular scientific contributions to men of considerable repute and the withholding of such recognition from men who have not yet made their mark."[23] When the name of an eminent man happens to come first in an alphabetical sequence, the scientific community, which tends to credit him with the work in any case, is all the more likely to do so. One physicist who has been listed first for this alphabetical reason confessed that it "was just embarrassing."

To prevent injustices that would be perpetrated by consistently alphabetizing authors' names on a series of joint papers, scientists have seized upon the expedient of rotating names through lists. A geneticist reported: "With F, it was not at all clear [how we should order our names] because we did everything together . . . So we tended to alternate . . . I had a general feeling of 'Gosh, we're equal; let's just alternate and give the [correct] impression that it's an equal partnership.' "

When only a pair of collaborators is involved, rotation of names on a series of papers—to equalize credit—is easy to arrange. As the number of co-workers increases, the number of papers they must publish to give every man first-authorship must, of course, equal the number of authors—and if every author is to be given an equal chance to occupy every position in the list, the number of papers required would obviously be *n!*. To put the matter simply, six co-authors would have to publish a mere 720 papers to rotate every author through the sequence. Such precisely mathematized allocations of credit would scarcely be visible to the scientific community and would reveal little more than an obsessive concern with the problem.

Rotation of names is not the only expedient tried and discarded in the search for an equitable ordering pattern. One laureate expressed his dissatisfaction with all the alternatives in these words: "It has been tried to rotate the alphabetical order; it has been tried to put [only] the institution [as author] and none of these has worked out . . . When you say, 'We will go alphabetical,' and you get a student whose name begins with 'A'; after a while, you get fed up . . . No perfectly standard and uniform practice has been established nor can it be."

We find that alphabetical ordering patterns designed to symbolize equality of contribution have their own share of ambiguity. How do things stand for other name-order patterns?

TABLE 4

PERCENTAGE OF PAPERS BY LAUREATES AND A MATCHED SAMPLE OF SCIENTISTS ON WHICH THEY ARE FIRST-AUTHORS, BY AGE AT PUBLICATION*

Age	Laureates	Matched Sample
20-29	49 (103)	38 (34)
30-39	40 (304)	38(130)
40-49	26 (578)	56 (80)
50-59	20 (374)	37(131)
60+	18 (222)	25 (64)
All	27(1,581)	39(439)

* Papers here refer to those having three or more authors.

NON-ALPHABETIC ORDERS

Lists of authors appearing out of alphabetical order are presumably intended to indicate unequal contributions to the research. In practice, such ordering is determined both by this criterion and by the relative ranks of authors.

First-authorship.—With these two criteria for name ordering, we would expect the laureates to be first-authors more often than those whose contributions to science have been less substantial. When the laureates are compared not to a sample of authors in the abstracts but to a sample matched to them for age, field of specialization, organizational affiliation, and initial letter of last name, as in Table 4, we find that this is not

[23] R. K. Merton, "The Matthew Effect in Science," *Science*, CLIX (January 5, 1968), 56–63.

the case.[23] The laureates are listed first on 27 per cent of their multi-authored[24] papers and the matched sample on 39 per cent of their papers. This is consistent with the Coles' finding that the correlation between the visibility of physicists and their propensity toward first-authorship is insignificant.[25] Laureates are, of course, highly visible but not given to placing their names first on scientific papers. The difference between laureates and the rank and file holds in all three fields of science. But though the laureates are less often first-authors on all their collaborative papers, it might still be that they earmark their contributions by electing to be first in the name order that especially heightens the visibility of first-author. This is, as we noted earlier, the "first-author-out-of-sequence" pattern (ZABC). Yet here too the laureates are first-authors less often than the matched sample; they occupy first place on 34 per cent of these papers as against their less eminent colleagues who are first 41 per cent of the time.

Why do the laureates take the first position less often than those in the matched sample? Laureates' contributions to collaborative investigations are scarcely apt to be less significant than those of the matched sample, and we know that they are highly ranked. Their name-order practices and the interview data, taken together, suggest that they often abide by the standard of *noblesse oblige:* their rank imposes special obligations. Precisely because the Nobel prize—or the other symbols of recognition received before it—confers high rank upon them, they are expected not to exercise their authority and power to the limit. And so they, more often than the less distinguished sample, are moved to give co-workers an enlarged share of the credit. A biochemist explained it this way: "If I publish my name first, then everyone thinks the others are just technicians . . . if my name is last, people will credit me anyway for the whole thing, so I want them to have a bit more glory."

This remark and others like it signal that the laureates are well aware that they are not penalized by their distinctive name-order practices. The Matthew Effect, which enhances the standing of eminent men at the expense of their less distinguished co-workers, assures them of recognition wherever they are placed on author lists. To this extent, their generosity is cost-free.

[23] Bibliographies were received from fifty-five of the 123 scientists drawn from *American Men of Science* from whom they were requested, permitting comparison of forty matched pairs of scientists. These pairs were matched in the following respects: (1) Age: Within five years of the laureate's birth date. When more than one bibliography was returned for a particular match, the one closest to the laureate in age was chosen. (2) Field of Specialization as listed in *American Men of Science*. Although the numbers of men in each specialty are the same for the laureates and the matched sample, the proportion of papers are not, since men were not matched for productivity. (3) Type of Organizational Affiliation: Laureates were classified according to whether they had worked in independent research laboratories, industrial laboratories, or universities. The scientists in the sample were matched accordingly. Pelz, among others, suggests that organizational context affects patterns of publication (see Donald C. Pelz, "Creative Tensions in the Research and Development Climate," *Science*, CLVII [July, 1967], 160–65; and Donald C. Pelz and Frank M. Andrews, *Scientists in Organizations* [New York: John Wiley & Sons, 1966]). (4) Initial Letter of Last Name: In order to avoid variations in positions in author lists attributable to place in the alphabetical sequence, men in the matched sample were matched to the laureates in this respect. Taking various criteria of eminence, such as receipt of scientific awards, prestige of departments, or holding editorships or elected positions in scientific organizations, the laureates are, as a group, substantially more eminent than men in the matched sample. However, these men whose names were drawn from *American Men of Science* are probably more distinguished than most scientists, since that directory lists only about a fourth of all scientific workers. Finally, there is no reason to suppose that men who returned their bibliographies differed from those who did not, with respect to name-order practices.

[24] Multi-authored papers refer to those having at least three authors, unless otherwise specified. As noted earlier, the laureates' publication practices on two-author papers do not differ from those of the sample of authors listed in the abstracts nor do they differ from those of the matched sample.

[25] Cole and Cole, *op. cit.* (n. 19 above). The Coles' computations also include single-authored papers on the assumption that visibility is enhanced both by first-authorship and single-authorship.

This interpretation, in terms of *noblesse oblige*, is at least consistent with a more detailed review of the data in Table 4 where laureates' publication patterns at various ages are compared to those of the matched sample. In their twenties, the laureates-to-be were first-authors on half of their collaborative papers. Both motive and opportunity structure joined to produce this result. First of all, they were intent upon establishing their distinctive contributions to joint enterprises. Recalling those days, a biochemist mused: "When you are young and not really very far advanced, you are much more excited about [being first-author] than you are later on. It's not very important now."

And second, when the laureates were young, they worked with eminent scientists. In fact, twenty-six of the forty-one laureates who were interviewed worked under men who were, themselves, Nobel prizewinners. And of the total of fifty-five Nobel laureates at work in the United States, thirty-four worked under older laureates. Their mentors often made decisions to put young men first just as the present laureates now do.[26] As one laureate remembered: "When I worked with T [an older prizewinner] it usually turned out that I was listed first." His experience is not unique. When the laureates-to-be co-authored papers with their laureate masters, it turns out that the junior men were first-authors 60 per cent of the time and the seniors only 16 per cent of the time.[27] Simultaneously, the young laureates-to-be were recipients of the generosity of their mentors and socialized into the pattern of *noblesse oblige*. We can identify anticipatory socialization in the case of a chemist who engaged in name-order practices associated with eminence and seniority early in his career:

INTERVIEWER: From the earliest times, when you published with students, I notice your name appeared last.

LAUREATE: Oh, that was just an affectation . . . I suppose. But there are two customs . . . either you put the students' names first or else it's alphabetical.

This combination of having made significant contributions—none of the laureates was a "late bloomer"—and generosity on the part of their seniors makes for a high proportion of first-author listings among the laureates' papers published when they were young.[28] Different values of the same two variables are tentatively taken to account for the lower percentage of first-authors among the matched sample. Their contributions to collaborative research were probably less significant and their own mentors probably less generous than those of the laureates.

As both the laureates and the matched sample take control over name-order decisions—and the interviews indicate that these decisions are generally made by senior investigators—the laureates place themselves first less often and the matched sample more often. By their forties, most of the laureates are distinguished men, and half of them have already received the Nobel prize. They are, by this time, aware that their visibility and eminence are not di-

[26] Laureates not trained under prior prizewinners nor under other equally eminent men were apparently not treated so well. One of them observed: "Oh, I've been a second author all my life. . . . First you're second author because you're young and then you're second author because you're the senior man in the group." His report is true enough for him—he is first-author on only 12 per cent of his papers.

[27] Neither laureates-to-be nor their laureate masters were first-authors on the remaining 24 per cent of papers. This situation is a marked contrast to papers co-authored by scientist peers, both of whom later became laureates. On these papers, one future laureate is just as apt as the other to be first-author in papers having at least three authors. In two-author papers—those with one laureate mentor and one young scientist and later prizewinner—the junior man is first-author 68 per cent of the time, more often than would be expected if no preferences were being exercised.

[28] Laureates describing their own attitudes toward talented young scientists suggest that generosity is easier to bestow upon these men (who were not unlike the laureates at the same ages) than upon men who have neither the ability to solve problems nor to formulate questions for research.

minished by their name-order practices.

Finally, as the scientists become older, the issue of name ordering, in particular, and competition for recognition, in general, declines in importance for both groups.[29] Furthermore, the contributions of older men to particular investigations are probably less significant. Again, in the interviews, older laureates say with regret that research is tiring and that, increasingly, they now serve in a supervisory capacity.[30]

Up to now, I have ascribed the inverse relationship between first-authorship of papers and age at publication among laureates to their growing eminence and visibility. Some additional data support this interpretation. First of all, rates of first-authorship should decrease quite considerably following the Nobel prize which, after all, confers much prestige upon its recipients. Table 5 shows that the laureates' rates of first-authored papers, already low, decline some 9 per cent following the award; the publication practices of the matched sample do not change at all in the same period.[31]

This change, following the Nobel prize, is not as great as we would have expected, given the very substantial prestige bestowed by the award. My analysis, however, disregards the fact that not all Nobel laureates were equally distinguished *before* receiving the prize. Some were already un-

TABLE 5

PERCENTAGE OF MULTI-AUTHORED PAPERS ON WHICH LAUREATES AND A MATCHED SAMPLE OF SCIENTISTS ARE FIRST-AUTHORS ACCORDING TO TIME OF PUBLICATION

	DATE OF PUBLICATION RELATIVE TO THE AWARD*		
	6 or More Years before the Award	0–5 Years before Award	1 or More Years after the Award
Laureates.....	39(688)	22(477)	13(416)
Matched sample.....	43(153)	35 (82)	36(204)

* Time of publication of papers of the men in the matched sample was determined by using the date of the prize of the laureate in each matched pair. A paper by a member of the matched sample published in 1952, for example, was classified as being in the category 0–5 years before the award if his laureate twin received the prize between 1952 and 1956.

[29] These changes in name-order practices with aging and the laureates' reports of corresponding changes in their involvement with it suggest that first-authorship may have different consequences for the visibility of scientists at different stages of the scientific career. Although the Coles find no association between name-order practices and visibility for a sample of physicists whose ages span the working years, it may be that the two variables are more strongly related at early phases of the life-work cycle than at later ones, when visibility has already been achieved.

[30] Even though laureates are first-authors proportionately less often as they age, one might suppose that they would continue to emphasize their contributions by being first on the two name orders which enhance the visibility of the first-author—the first-author-out-of-sequence and the alphabetically random patterns. The laureates decline to emphasize their contributions in the double sense of foregoing first-authorship and having an increasing share of their papers over time carry this sequence, thereby further enhancing the visibility of work of their collaborators.

PERCENTAGE OF PAPERS HAVING NAME ORDERS WHICH ENHANCE THE VISIBILITY OF FIRST-AUTHORS ON WHICH LAUREATES AND A MATCHED SAMPLE OF SCIENTISTS ARE FIRST-AUTHORS, ACCORDING TO AGE AT PUBLICATION

Age at Publication	Laureates	Matched Sample
20–29............	50	33
40–49............	26	63
60+............	18	11

[31] Since these data and those reported in Table 4 are drawn from the same information, it is scarcely surprising that the same pattern of declining first-authorship is shown in each. When laureates are classified according to age at the time of the prize, we find that men who received the award before they were fifty were first-authors more often in the period just before the award than were older laureates. In the five years before the prize, 28 per cent of the young winners' papers were first-authored by them and 16 per cent of those by older men. After the award, rates of first-authorship are just the same, 14 per cent for younger winners and 13 per cent for older ones. Differences between laureates and men in the matched sample hold in each field for every time period.

crowned laureates; they were members of their own national academies and foreign ones, recipients of major scientific awards, and holders of multiple honorary degrees. For these men, the Nobel prize only caps distinguished research careers. Others, however, were neither academicians, recipients of important awards, nor holders of honorary degrees before they were named laureates.[32] The Nobel prize confers very considerable increments of prestige upon these men. I want, then, to compare the authorship practices of men who achieved substantial eminence before receiving the prize to those of men who had not, in order to see whether the exercise of *noblesse oblige* is indeed more characteristic of eminent men.

As Figure 1 indicates, men who were eminent before receiving the prize began to transfer first-authorship to their co-workers before their less eminent colleagues did so.[33]

[32] Twenty-seven were classified as eminent prior to the award of the prize. All were members of the National Academy of Sciences or of foreign academies and had, on the average, twice as many major prizes and medals as the non-academicians. Moreover, they received eighty honorary degrees among them compared to the total of one awarded to the others. My earlier examination of changes in productivity following the Nobel award for scientists already eminent and for those who were not is based only on data for laureates named in 1960 or before. These computations on name-order practices include all forty-one laureates who received the prize before 1964 (see p. 399, H. A. Zuckerman, "Nobel Laureates in Science: Patterns of Productivity, Collaboration and Authorship," *American Sociological Review*, XXXII [June, 1967], 391–403).

[33] Differences between the two groups cannot be attributed to differences in time elapsed between publication of prizewinning research and the receipt of the prize. Scientists, already eminent at the

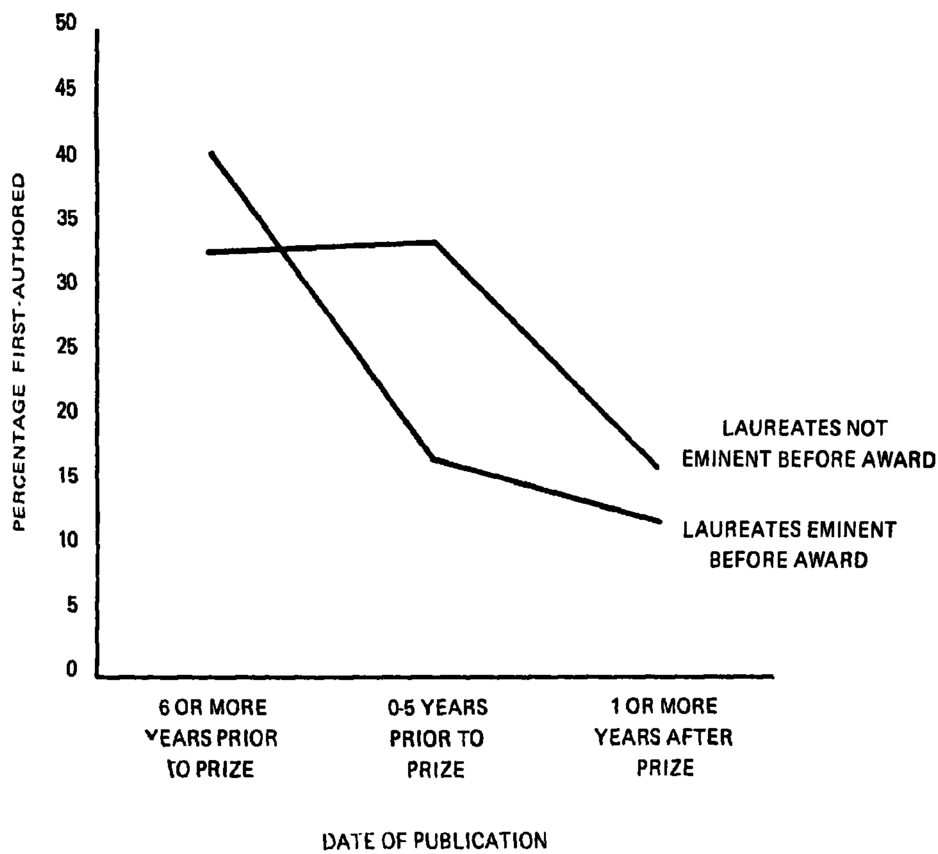


FIG. 1.—Percentage of multi-authored papers by laureates on which they are first-authors, according to date of publication and eminence at time of the award.

In the period just before the award, laureates who were already distinguished scientists took first place in author lists only 17 per cent of the time, but men who had not yet made their mark did so twice as often. Differences between the two groups of laureates, moreover, cannot be attributed to variations in name-order practices with aging. Taking those who received the prize before they were fifty, for example, we find that the previously eminent were first-authors on only 15 per cent of their papers published just before the award, but the less eminent laureates-to-be were listed first 48 per cent of the time. After the award, first-authorship is far less frequent—10 per cent of the already eminent and 19 per cent of the newly eminent laureates' papers carried their names first.[24] In this respect, future laureates—young and old—who had not yet become members of the scientific elite are much like the rank-and-file scientists in the matched sample. These laureates-to-be presumably felt obliged to earmark their contributions to collaborative efforts to an extent that their eminent colleagues did not. After the award, the practices of the previously eminent and the newly eminent—regardless of age—converge: they are first-authors 12 per cent and 16 per cent of the time, respectively. The Nobel prize apparently goes far toward equalizing earlier differences in prestige among laureates:[25] it also minimizes the tendency among prizewinners to highlight their own contributions to joint research through symbols of authorship.

These data are consistent with the suggestion that *noblesse oblige* is exercised more frequently as the eminence of individual scientists increases. It does not imply, however, that laureates are entirely selfless or wholly unconcerned with recognition; they "stake their claims" without hesitation to work to which they believe they made signal contributions. Two laureates emphasized this consideration when describing name-ordering patterns on papers reporting research for which each received the prize: "I think both Joe and Jim [his two co-workers] regarded me as the originator of the idea and the main promoter of the experiments. I have never felt with either of them, the slightest awkwardness as to whose idea it was or anything like that . . . And this was why I was first . . . But I would also like to say that without any one of us, it would not have been carried through." Just as directly, the other laureate reported: "Why was my name first? . . . I suppose one should publish in alphabetical order . . . . [but] it was really my job that I had been working on and then he came in. So I don't think it was unfair to him. He did supply a key piece of the thing [but] his time on it was small compared to mine anyway."

These laureates suggest that they adopt some symbolic device for earmarking significant contributions as their own. We would expect these distinguished scientists to use one or another of the high visibility positions in name orders—either the first-author- or the last-author-out-of-sequence or first place in the alphabetically random pattern more often on papers which eventually brought them the prize than on others they deemed less significant.[26] And

time of the award, received it nine years on the average after publication, and the others waited more than seven years for the prize to be given.

[24] Analysis of eminence and authorship practices of men who received the prize before the age of fifty and after it suggests that eminence is more important than age in shaping authorship decisions. There are a few papers by the very few men who were more than fifty at the time of the prize and not yet eminent. The reward system in science appears to be sufficiently effective to minimize the number of men who have done significant work but have not achieved eminence by the age of fifty.

[25] This is not to say that all laureates are equally esteemed after the award. Scientists are wont to speak of "strong" prizes and "weak" ones—that is, prizes which were awarded to men of unquestionable stature and accomplishment and prizes given to scientists whose selection by the Nobel Foundation was not a foregone conclusion.

[26] In retrospect, the laureates indicated that they were well aware of the importance of the work which eventually brought them the prize. As one of them put it: "[I knew] it was great stuff. Absolutely . . . I mean a great piece of physics . . . It would have

it turns out that this is the case. Taking only those papers reporting prizewinning research, we find that laureates occupy high visibility positions in name-order sequences 46 per cent of the time compared to 30 per cent on all other papers they have published with these same name-order patterns.[37] Nor do they stop here. They publish papers reporting award-winning research under their names alone—still another means of enhancing the visibility of their own contributions.[38] Taken together, approximately 60 per cent of all papers that reported prizewinning research appeared under the sole authorship of the laureate-to-be or with his name in a position of high visibility. The laureates' reluctance to forego all symbols of their prime achievement suggests that *noblesse oblige* has its limits.

Last-authorship.—It might be assumed that the normative criteria for last place in author lists are reciprocals to those for first place; that authors who contribute least and rank lowest should come last. Yet the laureates are last-authors more often than the members of the matched sample; in 53 per cent of their papers as against 33 per cent for the others.[39]

The practice of these laureates again expresses the norm of *noblesse oblige.* A laureate in medicine said: "My name *always* comes last . . . I did receive quite a bit of recognition for the work I did on X compound [not the work for which he won the prize]. Just so long as my name was there, it didn't make any difference where it appeared. Don't you think that's true? So why not put it last?" And another even described it as a matter of etiquette: "You'll find in recent years that my name is always last. This isn't because I think I have contributed least. But I think that, as senior person in the lab, it's more appropriate that it be there."

As we noted earlier, one name order, the last-author-out-of-sequence, actually heightens the visibility of the last-author. It appears equally as often on multi-authored papers by laureates and the matched sample, about one-fifth of the time for each group. Once this pattern has been chosen, however, the laureates *are* more apt than other, less eminent scientists to use it as a device for signaling their distinctive contributions. As Table 6 shows, laureates are last-authors almost twice as often on this high visibility pattern than are men in the matched sample, 61 per cent of the time as against 34 per cent.[40] Here again, *noblesse*

graced any university in the universe, it was that good." A fair number of them were ambivalent about the particular work cited for the Nobel prize—that is, they did not think that it was their best work—but none of them suggested that the work honored was insignificant. These findings are only apparently inconsistent with the Coles' report that position in name orders and citations are unrelated. They limited their investigation to first-authorship and single-authorship and did not take into account either name-order pattern or last-authorship.

[37] Laureates symbolize their distinctive contributions to prizewinning work more often than on other papers. First, they are slightly less apt to use alphabetical orders in these papers than on other publications—19 per cent as against 26 per cent. More important, they place themselves in highly visible positions on those papers with name orders which enhance the visibility of a particular author.

PERCENTAGE OF PRIZEWINNING PAPERS AND OF OTHER PUBLICATIONS ON WHICH LAUREATES OCCUPY POSITIONS OF HIGH VISIBILITY

Name-Order Pattern	Prizewinning Papers	Other Papers
First-author out-of-sequence. . .	54 (120)	23 (233)
Last-author out-of-sequence. . . .	69 (107)	55 (185)
Alphabetically random.	23 (156)	18 (288)
All.	46 (383)	30 (706)

[38] As I have noted above, single-authored papers are rarely the product of one investigator at work in isolation. Single-authored papers, especially those summarizing joint research or reporting specific parts of it, are generally not submitted for publication without consulting with co-workers. It is fair to assume that collaborators of the laureates-to-be accepted these single-authored publications as one of their legitimate prerogatives (see Zuckerman, "Nobel Laureates in the United States . . ." [n. 3 above], pp. 288 ff.).

[39] The difference between the two groups is concentrated in chemistry and the biological sciences for reasons that remain obscure.

[40] Although we know that the laureates are, more often than not, the most eminent members of their

oblige apparently is exercised to a point and no further. Moreover, the laureates adopt this pattern of visibility increasingly often as they get older, thereby earmarking their contributions without demanding first-authorship. In the biological sciences and in chemistry, for example, the laureates tell us, last-authorship has prestige, even when name orders do not call attention to it. A medical researcher remarked: "In our field, the last name of a group is the 'father' of the effort. . . . This has become a pattern in medicine so that the last name does have a certain position." Our data bear this out;

TABLE 6

PERCENTAGE OF PAPERS ON WHICH LAUREATES AND A MATCHED SAMPLE OF SCIENTISTS ARE LAST-AUTHOR, WHICH ARE SO ORDERED AS TO HEIGHTEN THE VISIBILITY OF THE LAST-AUTHOR, ACCORDING TO FIELD*

Field	Laureates	Matched Sample
Physics..............	52 (21)	41(27)
Chemistry...........	63(125)	10(10)
Biological sciences....	59(143)	35(55)
All...............	61(289)	34(92)

* The last-author in this pattern is alphabetically out of phase and is preceded by an alphabetical sequence (XYZA).

laureates in the biological sciences and in chemistry are more often "fathers" of investigations; they are last on 62 and 59 per cent of the papers, respectively, with names listed in the alphabetically random pattern, compared to laureates in physics who were last authors only 28 per cent of the time on papers of the same type.

I have shown that patterns of name ordering have been devised as adaptive mechanisms to heighten the visibility of role-performance of individual investigators working in collaboration. The various patterns which, at first, seem to have unequivocal meanings for scientists do convey varying degrees of ambiguity and, to this extent, fail to serve their intended functions. However, it is also the case that such ambiguity in the meaning of name orders reduces the stress of collaboration. As in other departments of social life, making things explicit often introduces strain into social relations.

Laureates, having been faced with decisions on name ordering and their consequences, are well aware of the tensions which accompany emphasis on strict weighting of contributions. They agree that some measure of ambiguity in this delicate matter of allocating credit and responsibility helps to maintain peace in collaborative situations. A physicist's remarks are representative of general feelings:

> I think [a standard system for distinguishing the contributions of co-authors] would tend to produce a great deal of interference. . . . If a system were set up to try to dispense credit with complete justice, I think we'd end up with almost everyone . . . disappointed, in general, with what they've gotten—feeling that other people have perhaps gotten more than their share. . . . So I would be opposed to trying to cut this very fine. It seems to me that the decision as to whether somebody should be senior author or should appear simply alphabetically is perhaps as fine as it should be reasonably cut.

It appears that, in the long run, the functional requirements of the evaluation system and of collaborative groups are incompatible and so call for different and sometimes conflicting procedures for making role-performance visible.

One matter remains for discussion. Until now, we have assumed that position in name-order sequences enhances the visibility of individual role-performance and thereby makes it possible to allocate credit among co-workers. Whether this is so or not, the fact is that scientists believe that it does. As with other definitions of the situation, consequences are apt to reflect such beliefs. It seems to be the case that eminence and

research groups, we do not know whether this is the case for men in the matched sample. It may be that the most distinguished co-workers of the sample members are last-authors on papers of this type as often as the laureates are. The reported differences in last-authorship are then suggestive, not decisive.

name-order positions jointly affect visibility in such a way that occupying visible positions in name-order sequences is important for the young and unknown but less so for scientists whose names are familiar to their colleagues. If so, then there is some isomorphism between the consequences of name ordering and differential concern with the matter. Finally, if name-order positions do not affect visibility in any consistent fashion, they have latent functions for co-workers. Individual collaborators often want symbolic recognition of their unequal contributions, if only to "keep the record straight" and to maintain equity within the group. This may explain why discussions of name ordering are anything but affectively neutral, even when co-workers believe that the published papers are routine.

I have noted that certain patterns of name ordering are variously adopted by eminent men more often than by others, that preferences for name orders differ among fields and among men at different stages of the scientific career.[41]

Since this device serves its purpose only partially, we should expect continuing attempts to modify existing name-order practices—as well as continuing dissatisfaction with them—until other arrangements are devised which integrate more effectively the functional requirements of research groups and of the evaluation system in science.[42]

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[41] Cross-cultural comparisons will show whether these patterns have uniform meanings for the world community of scientists and, in particular, whether the distribution of patterns differs between socialist and other societies.

[42] The very great concern voiced by physicists about the use of "institutional authorship"—that is, the listing of laboratories rather than individual investigators—suggests that more visibility, rather than less, is desired by scientists. For the comments of one distinguished physicist on this matter, see S. A. Goudsmit, *Physical Review Letters*, VIII (March 15, 1962), 229–30.

RESEARCH NOTES

The Distribution of the Ethnic Groups in Chicago, 1960

ABSTRACT

The original ethnic and native groups in Chicago have tended to move away from the central core of the city during the period from 1940 to 1960. The earliest immigrant group, such as the Irish, moved farther than the second group, such as the Italian, who moved farther out than the latest group, such as the Puerto Rican. Also, the whole city can be divided into two Negro districts (west and near south) and eight other ethnic districts—central, near north (German), far north (Russian), near northwest (Polish-German-Italian), far northwest (Polish-German), near southwest (mainly Polish), far southwest (Irish). The far south is heterogeneous.

The purpose of this paper is to identify and explain the distribution of the major ethnic groups in Chicago since 1960. To a large extent, it is a follow-up of two previous studies dealing with the population succession in Chicago—one by Paul F. Cressey and the other by Richard G. Ford.[1] Cressey's paper covers the years 1898–1930; Ford's paper concerns 1940. Both papers include Negroes and seven groups of foreign-born whites—Polish, German (including Austrian), Italian, Russian, Irish, Swedish, and Czechoslovakian. This paper includes all these groups in addition to Mexican and Puerto Rican.

Following Ford's procedure, I have divided the city of Chicago into eighteen mile-wide concentric circle zones from the intersection of State and Madison Streets. The numbers of the ethnic groups in the different census tracts are recorded from the U.S. Census for 1960. Any census tract with 50 per cent or more of its area lying within a mile zone is listed as belonging to that zone. For purposes of comparison, I have grouped the tenth to eighteenth mile zones together, as Cressey and Ford did.

During 1940–60 the total population decreased from 15 per cent to 11 per cent of the total city residences for the first three mile zones, and increased from 7 per cent to 22 per cent for the tenth mile zone and beyond (Tables 1 and 2). This indicates an outward movement of people from the Loop to the city's outskirts. The same trend is seen for all ethnic groups, except that there are no statistics showing the distribution of Mexicans and Puerto Ricans for the period of 1898–1940 in Cressey's or Ford's study. In 1940 up to 20 per cent of all the leading ethnic groups except Mexican and Puerto Rican were found in the inner zone (the first three mile zones). This reduced to 13 per cent or less in 1960. In 1940 from 2–11 per cent of all the leading ethnic groups except Mexicans, Puerto Ricans, and Negroes were found in the outer zone (the ten-mile zone and beyond). This increased to 13–47 per cent in 1960. The figures for Negroes were 1 per cent in 1940 and 9 per cent in 1960 in the outer zone. Generally speaking, the Irish moved farther than other foreign-born whites, who moved farther than the Negroes.

The 1940–60 trend was a continuation of the outward movement since 1898 except that the earlier movements were mainly from the inner zone to the middle zone located within 4–9 miles of the intersection of State and Madison Streets, rather than to the outer zone. As a result, the middle zone (proportionately) increased population before 1940. It declined afterward except that

[1] Paul F. Cressey, "Population Succession in Chicago: 1898–1930," *American Journal of Sociology*, XLIV (July, 1938), 59–69; Richard G. Ford, "Population Succession in Chicago," *American Journal of Sociology*, LVI (September, 1950), 156–60.

its Czechoslovakian population increased during 1940–60.

In 1960 the distribution of the leading ethnic groups in Chicago was partially due to a time factor in that those of the earliest group, such as the Irish and Swedish, were found farther from the Loop than the newcomers—the Mexicans and Puerto Ricans, and the Negroes. For example, in 1960 half of the city's Puerto Ricans were found in the inner zone and only about 1 per cent in the outer zone. The corresponding figures for the Irish were almost zero or less than 1 per cent, and 47 per cent, respectively.

Other factors related to the population succession were the shape of the city, the reference point, and the direction of movement for each ethnic group. The city is longer from north to south than from east to west. Also, the intersection of Madison and State Streets is closer to the northern than to the southern city limits. As a result, within the city itself, the southward movement can go farther than the northern or western movement. A case to illustrate the point is that both Irish and German belong to the earliest immigrant group, but in 1960 there was a higher percentage of Germans in the inner or middle zone and more Irish in the outer zone. Actually, the Germans were dispersed as well as the Irish, but the latter moved toward the southern or the southwestern outskirts while the former were concentrated in the northern half of the city.

As a matter of fact, the whole city of Chicago contains seventy-six communities; each can be classified according to its ethnic composition in 1960 or afterward, as indicated in Table 1. Two Negro districts and eight other ethnic districts can also be

TABLE 1

THE DISTRIBUTION OF THE ETHNIC GROUPS IN CHICAGO, 1960*

(Percentage)

Mile Zone	Population										
	Total	German	Irish	Swedish	Polish	Italian	Russian	Czechoslovakian	Mexican	Puerto Rican	Negro
1	1	0	0	0	0	0	1	0	2	2	0
2	3	2	0	1	1	5	1	0	16	20	5
3	7	4	0	0	7	8	4	0	23	28	8
4	9	11	1	0	14	11	9	0	11	22	13
5	12	11	4	7	10	10	9	22	11	11	20
6	13	12	7	6	12	11	6	30	10	5	14
7	12	13	14	40	9	12	9	35	4	3	13
8	12	11	17	18	11	9	19	0	2	6	13
9	9	11	10	2	10	8	22	0	3	1	5
10	7	9	22	8	9	8	13	0	1	0	3
11	6	6	15	5	8	5	4	13	6	0	2
12	3	4	4	0	4	3	2	0	7	1	1
13	2	3	1	6	2	3	1	0	2	1	1
14	2	2	4	7	1	3	0	0	1	0	1
15	1	1	1	0	2	3	0	0	1	0	0
16	1	0	0	0	0	1	0	0	0	0	1
17	0	0	0	0	0	0	0	0	0	0	0
18	0	0	0	0	0	0	0	0	0	0	0
Total	100	100	100	100	100	100	100	100	100	100	100

* The city of Chicago is divided into eighteen mile zones from the intersection of Madison and State Streets. The population figure in each mile zone is based on the U.S. Census. All census tracts with 50 per cent or more of their areas lying within a mile zone are listed as belonging to that zone.

TABLE 2

PERCENTAGE OF DISTRIBUTION OF ETHNIC GROUPS IN CHICAGO*

MILE ZONE	POPULATION			
	1898	1920	1940	1960
Total:				
1–3	25	23	15	11
4–9	70	70	78	67
10 or more	5	7	7	22
Total	100	100	100	100
German:				
1–3	44	19	12	6
4–9	51	74	80	69
10 or more	5	7	8	25
Total	100	100	100	100
Irish:				
1–3	46	19	10	0
4–9	50	76	81	53
10 or more	4	5	9	47
Total	100	100	100	100
Swedish:				
1–3	38	10	8	1
4–9	52	78	81	73
10 or more	10	12	11	26
Total	100	100	100	100
Polish:				
1–3	60	38	20	8
4–9	29	48	69	66
10 or more	11	14	11	26
Total	100	100	100	100
Italian:				
1–3	85	25	15	13
4–9	12	73	83	61
10 or more	3	2	2	26
Total	100	100	100	100
Russian:				
1–3	72	30	8	6
4–9	25	66	88	74
10 or more	3	4	4	20
Total	100	100	100	100
Czechoslovakian:				
1–3	65	27	17	0
4–9	33	70	80	87
10 or more	2	3	3	13
Total	100	100	100	100
Mexican:				
1–3	......	......	......	40
4–9	......	......	......	42
10 or more	......	......	......	18
Total	......	......	......	100
Puerto Rican:				
1–3	......	......	......	50
4–9	......	......	......	49
10 or more	......	......	......	1
Total	......	......	......	100
Negro:				
1–3	66	24	15	13
4–9	33	75	84	78
10 or more	1	1	1	9
Total	100	100	100	100

* The figures for 1898, 1920, and 1940 are based on the diagrams in Richard G. Ford, "Population Succession in Chicago," *American Journal of Sociology*, LVI (September, 1950), 156–60, except that there are no available dates for Puerto Ricans and Mexicans during these years.

identified (Table 2). The two Negro areas are the west (including the near west side, North Lawndale, East Garfield Park, and West Garfield Park) and the near south (including the near south side, Douglas, Oakland, Kenwood, Washington Park, Chatham, Armour Square, Fuller Park, Grand Boulevard, Englewood, Greater Grand Crossing, and Woodlawn), where the majorities have been Negroes since 1966. The other eight ethnic areas are the central, near north, far north, near northwest, far northwest, near southwest, far southwest, and far south. In 1960 the leading foreign ethnic group was German in the central and near north; Russian in the far north; Polish-German in the far northwest; Polish-German-Italian in the near northwest; the Polish in the near southwest (except German in Chicago Lawn); and the Irish in the

far southwest. The far south was heterogeneous where Hungarians led in Burnside; Russians, in South Shore and South Deering; Polish, in South Chicago, East Side, Hegewisch, Calumet Heights, and West Pullman; Italians, in Roseland and Pullman; and Irish in Avalon Park; here Riverdale has been a Negro community. While the two Negro areas may remain unchanged ethnically, the other eight ethnic areas are subject to modifications because of recent developments.

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A Note of Extreme Caution on the Use of Guttman Scales

ABSTRACT

Since the chance occurrence of the various scale types is a function of the joint probability of the occurrence of their constituent responses, it is possible that Scalograms meeting currently acceptable levels of Scalability and Reproducibility can occur by chance alone. Even Chilton's method, which determines whether the observed Reproducibility is significantly better than chance, does not guarantee that a scale is homogeneous; significant Reproducibility can occur even when three of six items are unrelated. Although very strict adherence to all of Ford's criteria might eliminate most chance-derived scales, the rule-of-thumb origins of these criteria limit their usefulness as rational proof that Guttman Scales are homogeneous.

Guttman Scale (or Scalogram) analysis is one of the most widely used methods for assuring that all items in an index are drawn from the same universe of attributes. The present paper starts with a brief summary of the logic, apparently not universally known to the field, which demonstrates that Guttman Scales meeting generally accepted tests of unidimensionality can occur by chance alone. It then examines a method that has been proposed for statistically evaluating whether a given scale is merely the result of chance factors.[1] This method is then used to demonstrate that Scalogram analysis, as generally used at present, provides no definitive proof that all the items in a given scale are measures of the same dimension.

From its inception, proponents of Scalogram analysis have been concerned about the role chance might play in producing apparently acceptable Scales.[2] More recently, it has become evident through the work of Schuessler,[3] Sagi,[4] and Chilton[5] that apparently meaningful Guttman Scales with coefficients of Reproducibility of 0.90 or more and coefficients of Scalability[6] of 0.60 or more can occur by chance alone. Such possibilities exist because the a priori likelihood of the occurrence of a particular scale type is equal to the joint probability of the occurrence of each of the constituent responses. Since the probability of the occurrence of a particular response to a question is its marginal frequency, the probability of the occurrence of all possible scale types can be calculated directly from knowledge of the marginal frequencies alone. This is done by multiplying the proportion of individuals making the appropriate response to each of the questions in the set. For example, the proportion of people who would fall by chance into the "yes, yes, no, no" scale type, defined by a response pattern involving saying "yes" to an item with which 80 per cent of the sample agree, "yes" to an item with which 60 per cent agree, "no" to an item with which 40 per cent agree, and "no" to an item with which 20 per cent agree, is 0.23 ($0.80 \times 0.60 \times 0.60 \times 0.80 = 0.23$). For any given set of marginal frequencies, the Reproducibility and Scalability attributable to chance can be obtained by calculating the chance probability of occurrence of each possible scale

[1] Roland J. Chilton, "Computer Generated Data and the Statistical Significance of Scalogram," *Sociometry*, XXIX (June, 1966), 175–81.

[2] See, for example, Louis Guttman's discussion in Samuel Stouffer, Louis Guttman, Edward A. Suchman, Paul F. Lazarsfeld, Shirley Star, and John A. Clausen (eds.), *Measurement and Prediction* (Princeton, N.J.: Princeton University Press, 1950), p. 82.

[3] Karl F. Schuessler, "A Note on Statistical Significance of Scalogram," *Sociometry*, XXIV (September, 1961), 312–18.

[4] Phillip O. Sagi, "A Statistical Test for a Coefficient of Reproducibility," *Psychometrica*, XXIV (March, 1959), 29–42.

[5] Chilton, *op. cit.* (n. 1 above), p. 175.

[6] Herbert Menzel, "A New Coefficient for Scalogram Analysis," *Public Opinion Quarterly* (Summer, 1953), pp. 268–80.

type, totaling the probabilities of the "correct" and "incorrect" patterns and entering the resultant totals into the appropriate formulas.

As can be seen from Table 1, given the theoretically conservative situation where 80 per cent and 20 per cent are the extreme cutting points, and the other cutting points are evenly distributed (e.g., 0.80, 0.65, 0.50, 0.35, and 0.20 for a five-item scale), chance Reproducibility would be well above 0.90 for three-item scales, would hover around 0.90 for four-item scales, and would drop only to 0.85 when six items are used. In fact, the use of ten items reduces chance Reproducibility only to 0.80. Furthermore, the conventionally accepted standard of 0.60 Scalability is met by chance alone by three-, four-, and five-item scales, and even the six-item scale has a chance Scalability of 0.59.[7]

Chilton has suggested a method "which transforms chance Reproducibility into a useful value and provides those working with Scalogram analysis with a simple and direct way to decide if initial results are worth pursuing."[8] This is done by dividing the difference between the expected and observed Reproducibilities by the standard error of the expected proportion of non-error responses, where the standard error is defined as

$$\sigma p = \sqrt{\frac{(\text{chance Reproducibility})(1 - \text{chance Reproducibility})}{\text{number of cases}}}.$$

The method is based on the assumption "that randomly determined Reproducibilities for an infinite number of sets of data are normally distributed around chance Reproducibility."[9] Calculations based on 45,000 sets of machine-generated data suggest that the assumption is true. Chilton foresees that "the requirement of a specified number of items may be eliminated when a statistical test is used, and the requirement that there be no substantial number of non-scale types may also be eliminated."[10]

[7] These figures would seem to support Guttman's early warning that "if the items are dichotomous ... it is probably desirable that at least 10 items be used" (Stouffer, *op. cit.* [n. 2 above], p. 71).

[8] Chilton, *op. cit.* (n. 1 above), p. 180.

[9] *Ibid.*, p. 178.

[10] *Ibid.*, p. 180. Chilton's method assumes that only one set of cutting points is evaluated. The greater the number of potential cutting points, the

TABLE 1

CHARACTERISTICS OF VARIOUS SIZED GUTTMAN SCALES, CALCULATED ON A PROBABILITY BASIS, GIVEN MARGINAL DISTRIBUTIONS WITH 80 PER CENT AND 20 PER CENT AS THE EXTREMES OF A SET OF EVEN AND SYMMETRICAL CUTTING POINTS

	NUMBER OF ITEMS			
	3	4	5	6
Cutting points=(% cumulative "yes" response)	(80/50/20)	(80/60/40/20)	(80/65/50/35/20)	(80/68/56/44/33/20)
Probable no. errors per individual	0.200	0.4064	0.635	0.877
Chance Reproducibility	0.934	0.898	0.873	0.854
Chance Scalability:				
Item	0.778	0.661	0.604	0.612
Individual	0.762	0.691	0.649	0.594
% in largest error category	8.00	10.24	7.28	5.73
Highest proportion (category error)/(category frequency)	0.30	0.448	0.661	0.663
Highest % total errors per question	8.00	11.36	14.04	15.84

A major difficulty, however, remains: Even if the Reproducibility of a given scale is significantly better than chance, this does not insure that all its component items are drawn from the same universe of attributes. The problem is best brought into focus by examining the probable chance error per individual in the situation where there is a symmetrical distribution and 80 per cent and 20 per cent are the extreme cutting points. Under such conditions, an individual will by chance make only 0.41 errors in a four-item scale and 1.95 errors in a ten-item scale. Thus, chance alone provides scales with relatively few errors. The explanatory effect of the probabilities, calculable on the basis of the marginal distributions alone, is so powerful that it leaves little unexplained variance. Not very much information is gained when we learn that a person has made significantly fewer than 1.95 errors in ten items. After the potential effects of the marginal distribution are taken into account, there may not be enough variance left to insure that *all* of the items are drawn from the same universe of attributes. Results significantly better than chance may come about merely because a small subset of the items is appropriately interrelated.

greater the likelihood of capitalizing on chance so as to achieve acceptable levels of Reproducibility. Since high Reproducibilities can be obtained, irrespective of content, by the selection of items with appropriate cutting points, the number of potentially examinable cutting points directly affects the likelihood of an item's being spuriously related to other items. Thus, random responses to each of a set of four initially dichotomous items would result in Reproducibility of about 0.80, but if each of the items had had five potential cutting points from which a point of dichotomization could have been chosen, Reproducibilities in the 0.90's would have been reached. Given the extreme case, where the response to the items has been totally random, a kind of maximum efficiency is reached when the number of cutting points per item equals the potential number of correct scale types (i.e., 4 cutting points per item in a 3-item scale, 5 in a 4-item scale, etc.), because every item can then provide a cutting point which can be efficiently used. Thus, the final probability of a significant difference from chance is a function of both the particular marginal distribution used and the relative uniqueness of that distribution as a description of the data at hand.

One way to explore this problem is through the use of artificial data. Sets of such data can be constructed which differ both in the numbers of pairs of items whose interrelationships meet the criteria of Scalogram analysis and in the number of items for which the responses are randomly supplied. If a Scalogram is to be effective there should be relatively few cases where an individual says "no" to one item and "yes" to another, less frequently endorsed, item. For a scale to be completely homogeneous, so that *all* of its items come from the same universe of attributes, this type of relationship has to exist between each possible pair of items. To the extent that a scale contains pairs of items between which such a relationship does not exist, it falls short of fulfilling the concept of unidimensionality which Scalogram analysis seeks. The effectiveness with which Scalogram analysis insures that such a relationship exists among *all* items within a scale can be assessed by evaluating those scales that would occur on a probability basis if some of the pairs of items were so related and others were not. Such scales can be constructed by hypothetically restricting the possible interrelationships between varying members of pairs of items so as to exclude that set of error patterns which occurs when an individual accepts the less frequently endorsed item in a given pair.

Table 2 shows the results of imposing such restrictions on varying numbers of items in scales of different sizes having the marginal distributions of those in Table 1. Scale types that could not occur under these restrictions were eliminated, the probable occurrence of the remaining types recalculated,[11] and the resultant level of Repro-

[11] I want to thank Isidore Chein for supplying the formula for recalculating the probability of occurrence of the remaining scale types. The formula is:

$$\text{New probability of remaining response patterns} = \frac{\text{original probability}}{1 - \text{sum of the original probability of the removed response patterns}}.$$

TABLE 2

Characteristics of Various Sized Guttman Scales, Calculated on a Probability Basis, Given Marginal Distributions with 80 Per Cent and 20 Per Cent as the Extremes of a Set of Symmetrical Cutting Points; with Various Numbers of Error-Producing Relationships among Items Eliminated

	Number of Items							
	4	4	5	5	5	6	6	6
Errors eliminated	Q1no-Q8yes Q4no-Q8yes	Q2no-Q4yes Q4no-Q8yes	Q2no-Q8yes Q4no-Q8yes	Q1no-Q 4yes Q4no-Q16yes	Q1no-Q2yes Q2no-Q4yes Q4no-Q8yes	Q2no-Q4yes Q4no-Q8yes	Q1no-Q 4yes Q4no-Q32yes	Q1no-Q2yes Q2no-Q4yes Q4no-Q8yes
	Number of Items Related							
	3	3	3	3	4	3	3	4
Reproducibility	0.950	0.963	0.934	0.920	0.967	0.903	0.894	0.933
Significance of difference from chance (300 cases):								
$z=$	2.98	3.57	3.17	2.44	4.87	2.44	1.99	3.90
$p\leq$	.01	.001	.002	.02	.001	.02	.05	.001
Scalability:								
Item	0.843	0.878	0.794	0.700	0.896	0.698	0.667	0.792
Individual	0.837	0.884	0.787	0.719	0.891	0.718	0.694	0.747
% in largest error type	6.78	8.00	5.18	9.10	6.23	6.96	7.16	4.36
Highest proportion (category error)/(category frequency)	0.395	0.533	0.663	0.347	0.677	0.765	0.420	0.682
Highest % of total errors per question	11.3	10.7	13.3	15.41	16.64	17.95	15.12	18.31

ducibility compared with the chance Reproducibility that would occur if no such restrictions were imposed. Given 300 respondents and the marginal distributions of Table 1, relationships between *only three* items in four-, five-, and six-item scales can result in Scalabilities greater than 0.60 and Reproducibilities both greater than 0.90 and significantly greater than chance. Thus, it is possible that even when Reproducibility is greater than chance, one item in a four-item scale, two items in a five-, and three items in a six-item scale may bear no relationship to a putative common universe of attributes other than that their marginal frequencies happen or are constructed to fit convenient points in a cumulative distribution. Scalogram analysis, even when evaluated by the criteria of 0.60 Scalability, 0.90 Reproducibility, and significant Reproducibility according to Chilton's method, provides no proof that *all* of the constituent items are necessarily drawn from a homogeneous universe of attributes.

One set of criteria for acceptable Guttman Scales that does seem to reject the nonhomogeneous scales depicted in Table 2 are those presented by Ford.[12] His criteria are: (1) A specific error pattern may contain no more than 5 per cent of the sample population. (2) The frequency of error resulting from a given response (i.e., rejection of a given item, or acceptance of a given item) should be less than half the frequency of that response. (3) Reproducibility must be 0.90 or more. (4) No question may provide over 15 per cent of the total amount of error. Every scale in Table 2 violates at least one of these criteria.

These criteria, however, present several difficulties. First, they are not stringent enough. The data presented in Table 2 represent the theoretically derived Reproducibilities and modal distributions of individuals into scale types, given various sets of marginal distributions and limited interrelationships among items. If repeated samples were taken with such distributions and interrelationships, both the Reproducibilities and numbers of individuals falling into the various scale types would vary around the theoretical central tendencies presented in Table 2. Since several of these scales barely miss satisfying all of Ford's criteria, it seems highly probable that the variation to be expected about the central tendencies depicted in Table 2 would result in some instances where scales with similar properties would meet all of Ford's criteria.

Second, Ford's criteria do not take account of the obvious differences among scales containing varying numbers of items. Given a random response pattern, the chance of an error scale type containing more than 5 per cent of the sample or of a single question accounting for more than 15 per cent of the total error is a direct function of the number of items in the scale.

Both of the above problems are functions of the most general difficulty with Ford's criteria—they are rules of thumb rather than deductions from the relationship between Scalogram analysis and probability theory. Consequently, there is no rationally based proof that all of the items in a scale that meet Ford's criteria are in fact drawn from the same universe of attributes. Even if more stringent versions of Ford's criteria are used, until such criteria are put on a theoretically meaningful and statistically assessable basis we have no real knowledge of the extent to which Scalogram analysis can be used to isolate a set of items, *all* of which come from a homogeneous domain of attributes.

Several approaches to statistically assessing the homogeneity of Guttman Scales can be suggested. One, proposed by John D. Campbell, would be to compare the observed interrelationship between each pair of items in a scale (e.g., Table 3, A), both with those that would occur by chance (Table 3, B) and with those that would oc-

[12] Robert N. Ford, "A Rapid Scoring Procedure for Scaling Attitude Questions," in Matilda W. Riley, John W. Riley, Jr., Jackson Toby, in association with Marcia L. Toby, Richard Cohn, Harry C. Bredemeier, Mary Moore, and Paul Fine, *Sociological Studies in Scale Analysis* (New Brunswick, N.J.: Rutgers University Press, 1954).

cur if a perfect scale existed (Table 3, C). With n number of items, there would be $n(n-1)/2$ pairs to examine. If the observed interrelationship between each pair was significantly better than chance and not significantly worse than that which would be expected if a perfect scale existed, then an argument could be made that the scale being tested is both better than chance and not significantly worse than a perfect scale.

TABLE 3

CAMPBELL ASSESSMENT OF GUTTMAN SCALE HOMOGENEITY

	Item Accepted Less		
	Yes	No	Total
	A. "Observed" Relationship		
Item accepted more:			
Yes	30	30	60
No	10	30	40
Total	40	60	100
	B. Chance Relationship		
Item accepted more:			
Yes	24	36	60
No	16	24	40
Total	40	60	100
	C. Perfect Relationship		
Item accepted more:			
Yes	40	20	60
No	0	40	40
Total	40	60	100

A second approach would be to develop a computer program that would calculate the response patterns that would result from systematically varying, for scales containing differing numbers of items, the degree to which, among varying pairs of items, accepting a less frequently endorsed item increases the probability of accepting a more frequently endorsed one. The resultant distributions of response patterns could then be examined to establish which criteria would best insure homogeneity.

A third possibility would be to consider Scalogram analysis as a test for heterogeneity rather than homogeneity. Although currently accepted procedures for Scalogram analysis do not insure that all of the items in an "acceptable" scale fulfil the underlying conception of unidimensionality, scales that do *not* meet current criteria are almost certainly heterogeneous.

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The Effect of Income on the Suicide Rate: A Paradox Resolved

Two sorts of research techniques produce quite conflicting evidence about the direction of the effect of income on the suicide rate. Time-series regression analysis over the period of the business cycle suggests that suicide falls as income (employment) rises.[1] Cross-sections, on the other hand, suggest that more people with higher incomes commit suicide than people with lower incomes.[2] The purpose of this paper is to reconcile the two sorts of evidence. The answer arrived at is that the paradox arises because the empirical analyses have been guilty of "specification error." More specifically, the various studies have omitted lagged variables from the analyses. A model that contains lagged variables is then shown to be compatible with all the existing evidence.[3]

Assume that suicide is a function of income and education:[4]

$$\begin{aligned} S_{i,t} = {} & a_{i,0}Y_{i,t} + a_{i,1}Y_{i,t-1} \\ & + a_{i,2}Y_{i,t-2} \dots a_{i,k}Y_{i,t-k} \\ & \pm b_{i,0}E_{i,t} \pm b_{i,1}E_{i,t-1} \\ & \pm b_{i,2}E_{i,t-2} \dots \pm b_{i,k}E_{i,t-k} + u_{i,t}, \end{aligned} \quad (1)$$

where

$S_{i,t}$ = probability of suicide of person i in year t,

$Y_{i,t}$ = income of person i in year t,

$E_{i,t}$ = whether or not person i was in school in year t,

$u_{i,t}$ = disturbance term whose specification we shall ignore.

Equation (1) is written to indicate that the a and b coefficients pertain to the individual i. But as is common in work of this sort, from here on we shall work only with somewhat vague pooled community analogies to the individual coefficients, and hence i subscripts will not be given for the coefficients.

Consider further that education is a function of income—perhaps income in the prior year. (A better statement would make education a function of income in all previous years, and/or a function of prior education, but let that go.) That is,

$$E_{i,t} = c_1 Y_{i,t-1} + v_{i,t}. \quad (2)$$

Now if one substitutes equation (2) into equation (1) one gets:

$$\begin{aligned} S_{i,t} = {} & a_0 Y_{i,t} + a_1 Y_{i,t-1} \\ & + a_2 Y_{i,t-2} \dots a_k Y_{i,t-k} \\ & \pm b_0 c_1 Y_{i,t-1} \pm b_1 c_2 Y_{i,t-2} \dots \\ & \pm b_k c_{k+1} Y_{i,t-k} + u_{i,t}. \end{aligned} \quad (3)$$

Equation (3) represents what we postulate as the "true" model. But the *specified* model for a cross-section analysis is:

$$S_{i,t} = a_0' Y_{i,t} + w_{i,t}. \quad (4)$$

The observed coefficient $\hat{a}_0'$ is the outcome of the usual least-squares regression of the cross-section model (4). It is the empirical estimate of a', and it is an undefined composite of the individual coefficients that we are now assuming are equal, or at least independent of income. This $\hat{a}'$ differs from

[1] Andrew F. Henry and James F. Short, Jr., *Suicide and Homicide* (Glencoe, Ill.: Free Press, 1954), Appendix IV.

[2] *Ibid.*, p. 137.

[3] Elsewhere a completely analogous analysis is given for the paradoxical income-fertility relationship, with the same clarifying results (see Julian L. Simon, "The Effect of Income on Fertility" [1968] [mimeographed]).

[4] Please note that I am *not* asserting that other factors do not also influence the suicide rate. And, indeed, the addition of such variables as religion would certainly increase the amount of variance explained by this or any other model, perhaps greatly. The following models are not offered as a best explanation but only to show how the paradox of the discrepant evidence disappears when even a simple model is more fully specified as to time.

the *real* mean coefficient, a_0, in important ways, which is the nub of the problem. Specifically, it is shown elsewhere[5] that:

$$\begin{aligned} a_0' \simeq\ & a_0 + r_{Y_tY_{t-1}}a_1 \\ & + r_{Y_tY_{t-2}}a_2 \ldots r_{Y_tY_{t-k}}a_k \\ & \pm r_{Y_tY_{t-1}}b_0c_1 \pm r_{Y_tY_{t-2}}b_1c_2 \ldots \\ & \pm r_{Y_tY_{t-k}}b_kc_k . \end{aligned} \tag{5}$$

Therefore, in a cross-sectional study of the effect of current income, Y_t, upon suicide, the observed coefficient, $\hat{a}_0'$, differs from the "real" coefficient, a_0, by the entire right-hand side of (5) less a_0. This difference is the specification bias resulting from the fact that the cross-section does not include lagged income variables in the analysis.

If the "real" coefficients of income through education, b_kc_{k+1}, are positive and the coefficients of direct income, a_k, are negative, then whether the observed coefficients in the cross-section, $\hat{a}_0'$, show up as positive or negative depends upon the relative weights of the education and income variables as well as upon the correlation coefficients $r_{Y_tY_{t-k}}$.

The $r_{Y_tY_{t-k}}$ will be very high for almost any cross-section, however, so the relative weights of the coefficients are all-important. And it is not implausible that the education weights are relatively high, which would be compatible with an observed negative relationship.

Now assume that the underlying model is the same, but a time-series analysis is conducted of the nature

$$S_t = a''_0\, Y_t + z_{i,t} . \tag{6}$$

This is the simplest regression plot of suicide on income in the same year, as exemplified by the correlations reported in Henry and Short, and others. Over the business cycle (the period over which these time-series regressions are usually done) $r_{Y_tY_{t-k}}$ is quite *low;* that is, the year-to-year income fluctuations are large relative to the secular change. This means that specification bias (following the same analysis as above) must be relatively small, and the effect of the lagged education variables (as well as the lagged direct income variables) washes out. Therefore, if the true coefficient a_0 is negative, the observed time-series coefficient $\hat{a}''_0$ will also be negative, which is indeed what is observed.

It should be noted that the resolution of the contradiction between cross-section and time-series evidence on the suicide-income relationship is *not* that different factors operate over time as compared to across observations at the same point in time. Rather, *statistical illusion* is responsible for the contradiction, because some lagged factors that wash out in the time series make their presence felt in the cross-section. The reality is the same; it is just the ways the pictures are taken that produce different images.

It seems reasonable, then, to conclude that the evidence confirms a model like (1) in which the partial effect of direct income on suicide is negative and the partial effect of education and other variables for which it is a proxy (which indirectly reflects the effect of income) is positive.

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[5] Julian L. Simon and Dennis J. Aigner, "The Length-of-Run Nature of Cross-sectional and Time-Series Parameter Estimate" (Social Science Research Institute, University of Wisconsin, 1968).

LETTERS TO THE EDITOR

Comments on Henslin's "Craps and Magic"

If one must treat American cabbies as aborigines (James M. Henslin, "Craps and Magic," *American Journal of Sociology*, LXXIII [November, 1967], 316–30), one ought at least to accord them the basic courtesies due any native group. For example, one ought to take the trouble to learn their language before presuming to interpret their verbal behavior.

Fortunately, Henslin has presented some of the verbal behavior in the original cabbie-ese, along with descriptions of the context. Let me illustrate the weakness of his interpretation of the behavior by translating a few of these classic remarks into English (of course, the vigorous flavor and subtle nuances of the original are necessarily lost in the translation). On page 325, "Why those motha . . . [etc.]" should be rendered: "My, my that was *most* unfortunate." The cabbie's advice to the author quoted on page 319 means "Craps (like sex, perhaps) is a fundamentally routine and humdrum sport unless carried off with a little style." Throughout the article, Henslin's understanding of colorful and robust workman's English is so literal as to make the reader conclude that the author must possess the world's foremost collection of skyhooks and left-handed wrenches.

Patterns of behavior are equally misinterpretable, especially by persons unfamiliar with the culture in question. The bet against an uncounted handful of change reported on page 322, for example, was not an exercise in magical technology, it was a *tour de force*, an aesthetic gesture common to American gambling culture in which the bettor offers his opponent a better-than-even break, forcing the opponent to "chicken out," to win badly, or to lose a sure thing. That this has, *in addition*, a psychological function is illustrated by the use of the same strategy in foil fencing. The "invitation," consisting of a lowered guard and an unprotected chest, appears to the spectator to be an easy point but is actually an invitation to make a predictable and therefore mistaken attack under the emotional pressure of looking foolish.

Cabbie crap games afford, as a matter of fact, an excellent context for the study of magic. They are equally rewarding, however, to the student of leisure, sociability, status behavior, or ritual. To confuse these (and other) pursuits is to miss the point as badly as Frazier did in regarding certain magical acts as *merely* unsophisticated technology. To swallow, hook, line and sinker, the technical manipulation of dice through magic is to achieve a naïveté in fieldwork seldom equaled in sociological research.

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The Author Replies

I shall attempt to avoid replying to the highly irrelevant and vituperative personal aspects of his attack (e.g., "skyhooks and left-handed wrenches") which have no place in a scientific journal and instead deal with whatever substantive charges his letter might contain.

Unfortunately, Curtis takes a most naïve position when he assumes that the phrases of the language of one's own social class are necessarily commensurate with or equivalent to those of a differing social class except for stylistic differences. To say, as he does, that "Why those motha fuckas! They'll turn around an' fuck ya in the ass every time!" should be rendered as "My, my that was *most* unfortunate" is laughable non-

sense. Differing world views that are reflected by language must also be taken into account—world views of causation, for example, that are manifested not only on the verbal level but that are also matched with concomitant behaviors. And this, of course, is what the article is all about.

Now, as to specifics: The advice given me when I began shooting does without doubt involve "style," but to end one's analysis with that simplistic observation is to miss the whole point—that "workin' at" one's shooting and "talkin' to the dice" do indeed lead to control over the dice and to desired outcomes, in the view of these crapshooters.

Curtis misses the point quite badly when he views the "bet for change" as being "the same strategy [as] in foil fencing" or as an "aesthetic gesture." The purpose of this act is not to get one's opponent to chicken out, to make him look badly, etc. *This act is not even directed against the opponent* but is used as a last resort, when one doesn't seem to be able to make points, as a straightforward attempt within their system of rationality to regain one's own control over the dice. It is *directed toward the dice* and not toward the other bettor, for it is *the dice* that are viewed as being the opponent that must be overcome by some strategy. One would, however, misinterpret this, as Curtis does, "if one weren't adequately familiar with the culture in question."

Since it has been quite some time since I shot back-alley craps, I decided not to trust solely my judgment in these matters. Accordingly, I gave a copy of the article to Mr. Henry Osborne, who, in my opinion, is "sufficiently familiar with the culture in question," since he is a Negro who has played craps in the ghetto from his childhood. He is also, I might add, currently finishing his Bachelor's in sociology. I asked him to read the article and to then "tell me what he thought" of it and to criticize its weaknesses as he saw them. He did *not* in any way know about Curtis' letter, nor was he directed to any specific part of the article, nor to any particular ideas it contains. Yet the following is his comment:

> The article didn't go far enough in analyzing the dice as causal agents. The dice are personified beings in themselves. They are either for you or against you. There is really a three-way interaction system: you, the other players, and the dice.

I think this is sufficient as a reply to Curtis, and I would like now to take this opportunity to make two addenda to the article, for both of which I am again grateful to Osborne:

1. Concerning "touching behavior": If a shooter is doing well, other players like to touch him, but if he's "not making it," other players don't like to touch him. This behavior is another example of "the influence of personal relationships" (p. 325) and is similar to the "Good Book" example cited, where power is believed to be transferable from one person or thing to another person.

2. Concerning a variant of the control strategy of "catching the dice" (p. 324): With some groups one is not allowed to catch the dice if there is a "split fade," where the shooter's bet is covered by more than one bettor. In such cases a common strategy is to make sure that one fades the entire bet in order that one can catch the dice and thus lessen the shooter's control.

Finally, if readers would bring to my attention other examples of such magical behavior, whether in craps or in other forms of gambling, or in other games, or in sports, or in any other aspect of life, I would appreciate it.

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BOOK REVIEWS

Social Indicators. Edited by RAYMOND A. BAUER. Cambridge, Mass.: M.I.T. Press, 1966. Pp. xxii+357. $3.45.

This symposium, prepared by the American Academy of Arts and Sciences under the sponsorship of the National Aeronautics and Space Administration, opens a broad front of what may skeptically be called the "macro-analytic revolution." Its revolutionary potential lies in the attempt to be both macroscopic *and* quantitative: "The focus is nothing less than the entire set of social indicators used in our society . . . that enable us to assess where we stand and are going with respect to our values and goals, and to evaluate specific programs and determine their impact" [quoted from back cover]. Readers of this *Journal* will not be surprised that William F. Ogburn compiled forty-one standardized charts of "social trends" as early as 1936 and pioneered a somewhat analogous study, *The Social Effects of Aviation* (Boston, 1946). Besides considering the "second-order" effects from crime to stock raising, he predicted a surge in civil aviation during the first postwar decade such as we are yet to witness. But, as Bertram M. Gross states, "The present volume . . . is the first occasion on which the entire field has been surveyed and a comprehensive set of proposals based upon careful analysis, has been developed" (p. xv).

The symposium opens with a prefatory note by Gross on the history of social indicators, followed by the editor's introductory chapter on "Detection and Anticipation of Impact: The Nature of the Task," a major article by Albert D. Biderman on "Social Indicators and Goals," what amounts to (and has since become) a monograph by Gross on "The State of the Nation: Social Systems Accounting," methodological notes by Biderman on "Anticipatory Studies and Stand-by Research Capabilities," and concludes with some appropriate but unexciting remarks by Robert A. Rosenthal and Robert S. Weiss on "Problems of Organizational Feedback Processes."

"Social indicators" Biderman defines as "quantitative data that serve as indexes to socially important conditions of the society" (p. 69). His is a sociology of knowledge approach or, as he prefers, a "sociology of societal data" (p. 73). He construes statistical data about a society as institutional products subject to constraints of patterned evasion, ignorance, and error which may convert statistical indicators into statistical vindicators (p. 78). Scientific uses are no more immune: Following Mannheim's distinction, Biderman asserts that within the statistical profession there occurs a displacement of substantial by functional rationality, in neglecting the pertinence of data to social values. The question of validity—what indicators indicate—is acutely raised in criticism of the FBI's *Uniform Crime Reports.* Although they offer no basis for determining trends in criminal behavior, "year-to-year increases in crime rates may be more indicative of social progress than of social decay" (p. 115). His main concern is the disparity between formulating national goals and matching them with statistical indicators to gauge progress toward goal attainment. Of eighty-two explicit goal statements in the 1960 report of the President's Commission on National Goals, only 59 per cent correspond to *any* indicator data from the *Statistical Abstract of the United States* and *Historical Statistics of the United States* under "the loosest criterion of relevance" (p. 90). One possible reason for this discrepancy is that, "To be useful, each indicator must relate to a concept about the society that is a part of an explicit or implicit theory of the society" (p. 84).

Gross makes explicit one such theory. Where Biderman prefers the empirical approach to societal model building, Gross takes the prescriptive approach to derive a rich but unrefined "structure-performance" model of *any* society. Its essentials may be summarized in his two analytic propositions: 'The structure of any social system consists of (1) people and (2) nonhuman resources (3) grouped together into subsystems that (4) interrelate among themselves and (5) with the external environment, and are subject to (6) certain values and (7) a central guidance system that may help provide the capacity for future performance" (p. 183); and "The performance of any social system consists of activities (1) to satisfy the interests

of various 'interesteds' by (2) producing various kinds, qualities, and quantities of output, (3) investing in the system's capacity for future output, (4) using inputs efficiently, (5) acquiring inputs, and doing all the above in a manner that conforms with (6) various codes of behavior and (7) varying conceptions of technical and administrative (or guidance) rationality" (p. 184). Rather than giving a full empirical interpretation to his model, Gross proceeds to discuss some broad problems of index construction which mostly resolve to a specificity-generality dilemma. While the indicator model should be comprehensive in respect to crucial system variables, it cannot aspire to completeness in detail. General system models permit selective application to special and even unique models (p. 263). General and partial models are mutually supportive, not mutually exclusive. Gross's parallel development of organizational and societal models provides some insight into the system-subsystem relationships implied. An abstraction-specificity ladder unites realism and relevance in the model's index structure. An unresolved dilemma is the accustomed one of quantity and quality. While the social indicators' "movement" has emphasized the former without denying the latter, societal development toward "postindustrialism" (of which this trend is itself indicative) must stress the *quality* of social life.

Gross claims that payoffs have already accrued in new research stimulated by these studies, and that the viewpoints reflected have gained access to the highest policy-making levels. One "influential" influenced is Senator Walter F. Mondale, who introduced a bill calling for a Council of Social Advisors as a counterpart to the President's Council of Economic Advisors. In an address on "Social Indicators as a Method for Forecasting and Planning," he complains of "channel vision"—the unperceived relatedness of social things—as an occupational disease of government: "I don't recall any serious discussions on the Space Committee in which we tried to compare the value returned of the nation's space effort as against, say, education, or against some of the other ongoing programs. And yet clearly an American Government, with limited resources, ought to consider that a fundamental part of the job." This address, given the year *after* publication of *Social Indicators*, testifies to the problems of organizational feedback and confirms Biderman's anticipation (p. 45) of innovative lag in this system of social technology. While the quest for social realism and relevance continues, social scientists might ponder deeply the question, "What would *you* tell the President?"

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Stochastic Models for Social Processes. By David J. Bartholomew. New York: John Wiley & Sons, 1967. Pp. xii+275.

This analysis of social processes from a stochastic viewpoint is welcome. No previous book covers the ground in the detail provided by Bartholomew. His models deal with topics as diverse as social mobility, changes within organizations resulting from specified promotion policies, and the spread of news and rumors. Evidently inspired by James S. Coleman's *Introduction to Mathematical Sociology*, he has taken one portion of it for elaboration and extension.

Bartholomew describes every one of a score of models in enough detail for the beginner in stochastic processes to be able to follow the mathematical argument (a beginner, that is, who has had a course in calculus and some contact with matrix algebra). Most of the relevant stochastic models extant are included, and the writer makes contributions in the reconciliation of other scholars' results with one another and in adding some of his own fresh ideas.

One who has had to pursue this literature through the scattered sources in which it has up to now existed is relieved to find succinct and clear statements on such a topic as occupational mobility. The mover-stayer model and others are presented concisely and clearly. Results are exemplified with data due to Glass and Hall and Natalie Rogoff. Data as such are not the writer's main interest—instead of developing any of his own, he used Rogoff as reworked by Kemeny and Snell. Often the numbers he uses are not real data at all, but hypothetical. These are instructive for making points seem more concrete than their expression in symbols, but of course they have nothing to do with the application of the theory. We can only hope that the sorts of models here made easily accessible to sociologists will lead to more widespread application; theory has often improved the collecting of data that permit it to be applied.

The model for a graded social system asso-

ciated with the name of Gani is given special prominence. We may think of the grades as positions in an organization, with well-defined occupations arranged in layers, or as university years, or as numbers of university credits. A rule is provided for promotion from one grade to the grade above; provision is made for departure from the organization at any grade. Entrants may come in at the lowest grade, or else in a specified frequency distribution with positive fractions at each grade. If demotions may be excluded, the transition matrix takes on a triangular form which makes the explicit solution especially simple. The application to assessing the effects of a promotion policy may be important. For instance, the result of small and reasonable fractions promoted at each grade can generate a surprisingly large number of incumbents in the highest grade.

An issue is raised by the simplicity of the models. We know that the graded hierarchy, with entrance at the bottom and random promotions through the system, was more appropriate to the early industrial factory with workers, foremen, and managers, or to the early counting house with clerks and supervisors, than it is to the plant or bank of the future. A shift occurs with advancing technology from line to staff. The homogeneous workers under a single command give place to systems engineers, maintenance specialists, trouble-shooters of all kinds. In the much more complex production process of the future we will find it hard to say who reports to whom, and who is above whom. The remarkably productive team which is the modern firm could not use its specialized equipment if orders were simply passed down the line.

Models must remain simple if they are to be mathematically tractable. Does this mean a lag in the mathematical representation, by which social arrangements attain mathematical expression only when they are on the verge of becoming obsolete?

Most of the book is confined to expected values of stochastic processes. This is reasonable since the chief interest of any process is in its first moment; somewhat lesser interest attaches to second moments, and interest declines rapidly with the order of the moment. Attribution of the device used for the second moment is carefully made to John Pollard. The deterministic process, for the linear case identical with the first moment of the stochastic process, and hence the heart of the book, was extensively applied in this kind of work by P. H. Leslie, and before him by E. G. Lewis and Harro Bernardelli; none of these three names appears. This is a notable exception to the otherwise careful referencing throughout the book, commendable in a first survey of the field.

Misprints in a book such as this leave the reader for whom it is intended—the one who is trying to learn about stochastic models in social processes—uncertain and discouraged. At the top of page 23 the Q should presumably be O; in formula (2.1) on page 13 the pi should be p_i, and the i in parentheses should be j; the first n in (3.11) on page 79 requires a bar above. That such errors are difficult for author and publisher to avoid does not make them easier on the reader.

Whatever its minor faults, Bartholomew's clear and thorough exposition will be welcomed by all sociologists. Those whose teaching is on the mathematical side now have an instrument that will increase communication with their classes and enable them to cover more ground in an important sector than when the reading consisted of original articles. Sociologists in general will benefit by the removal of some of the mystery from mathematics applied to their field, helping it to become a universally available tool instead of a sectarian one.

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Asian Drama: An Inquiry into the Poverty of Nations, Vols. I, II, and III. By GUNNAR MYRDAL. New York: Pantheon Books, 1968. Pp. 2,284. $8.50 (paper).

Sociologists and colleagues from related disciplines who do not read the more than 2,000 pages of Gunnar Myrdal's book may gain the impression from press releases, reviews, or a quick look that it is a careful examination of the relationship between non-economic and economic factors in the development of Asia. Such a book is badly needed. Unfortunately, Professor Myrdal has hardly advanced understanding of the interaction of social and economic factors in development.

He takes his fellow economists severely to task for oversimplifying, for assuming that "Western" economics applies to the less developed countries, and for ignoring the effect of institutions, attitudes, values, and political systems. He is certainly right in suggesting that

some economists act as though theirs is an exact science, with laws that apply to all societies. But the majority of economists acknowledge that their discipline is far from exact and that seemingly "perverse" economic behavior occurs even in highly developed economies, for instance, as the result of a desire for prestige or approval. (Among widely recognized examples are firms that do not attempt to maximize profits and consumers who do not reduce their demand in response to rising prices. Firms may maximize growth, or protect markets, or avoid risk; consumers may derive prestige from buying expensive goods.) It is even more widely accepted that the economic behavior of large groups in less developed countries differs from that of their counterparts in developed economies. That farmers in some Asian countries are more tradition-bound, more risk-avoiding (for good reasons), and less market-oriented than their Iowa counterparts is hardly a startling statement. The complexity of the development process and the importance of factors other than investment in achieving growth have become increasingly evident over the last fifteen years to practically everyone concerned with development problems.

It is therefore no longer enough to point out that economists who assume simple, universal economic cause-and-effect relationships in development are naïve. It would be an important contribution to indicate the limits of economic influence and the impact of economic factors within those limits. It is not enough to say that institutions are important. It would be helpful to know *how* important they are and how they can be changed, or how their influence can be neutralized if it is pernicious. Myrdal contents himself with an attack on the naïveté of some economists writing in the 1950's. He fails to delimit the role of economic factors, of institutions, values, motives, political systems, or of the other non-economic influences which he stresses.

Some examples are necessary to support this statement, if it is not to be dismissed as the carping of an economist whose *amour propre* has been touched by the defection of a colleague. A basic Myrdal argument is that many persons in south Asia do not want to work. Anecdotal and some descriptive evidence is offered. The statement is undoubtedly true; presumably there are lots of people in all societies who do not like to work. But the interesting questions are whether governments can induce people to work by higher wages or higher taxes or by making more consumer goods available—questions of economics—or whether appeals to patriotism, religion, or communal solidarity will produce results. If the latter, it is important to know if such appeals can be framed. Or will none of these approaches be successful, with the result that fundamental changes in the value system may be required to improve work habits? These changes can probably result only from revolution or long-term evolution. These interesting questions are not really examined by Myrdal, although he does imply, without proof, that fundamental, probably revolutionary, changes are required.

A simpler example is provided by his statement that prices do not influence the behavior of peasants in Asia in the same way they affect that of farmers in developed countries. Again this is hardly a novel statement, qua statement. The interesting questions are whether prices influence the behavior of Asian peasants at all and, if they do, to what extent. And what aspect of prices exerts an influence? Is it higher absolute prices than in the past? Or is it the relationship between prices of output and those of input or consumer goods? Or is it price stability? To the extent that prices are ineffective, is the ineffectiveness due to ignorance which can be overcome by education, or to a value system which puts a premium on leisure? If it is the latter, how widespread is it? Or is the institution of land tenure all important? We get no answers to these questions from Myrdal.

This criticism is related to another shortcoming of his book—the cavalier neglect of numbers. Certainly one of the great advances of all the social sciences has been in the application of statistics. Admittedly this, like any methodological advance, can be carried too far, but the Myrdal book neglects quantification to an absurd degree. What is worse, statistical evidence is often available which contradicts his non-quantified conclusions. To use only the examples given above: there is evidence from India, Pakistan, and elsewhere that farmers *do* shift to more lucrative crops even if these crops require more work.[1] There is even evidence that agricultural output as a whole is related to

[1] E.g., Raj Krishna, "Farm Supply Response in India-Pakistan," *Economic Journal* (September, 1963), pp. 477–87; Walter P. Falcon, "Farmer Response to Price in a Subsistence Economy: The Case of West Pakistan," *American Economic Review*, LIV (May, 1964), 580–91.

relative agricultural prices. There is evidence from the same countries that factories can get all the unskilled labor they are prepared to hire, even if the work week is 50 hours or more. In East Pakistan, several hundred thousand workers participated in local rural projects when they were offered $0.30 to $0.40 per day for their labor. To repeat, this is not an argument that Myrdal is wrong and that economic factors explain everything. It is an argument that one must try to use whatever quantitative evidence is available to indicate the significance and the limits of the relationships one describes—surely a modest requirement.

This requirement is particularly important if one writes as a political economist, as Myrdal does. He draws important policy conclusions from his analysis. He is pessimistic about the future of Asia; he ascribes its misery to irrational attitudes and outmoded institutions, not to lack of resources; he downgrades, almost dismisses, foreign assistance. Before he draws such far-reaching conclusions, a social scientist really should have some fairly conclusive evidence.

Myrdal's analysis shares a weakness for warnings and problems with much past sociological writing about development, as well as a lack of concern with magnitudes, instruments, and policy conclusions. For instance, one is told by many sociologists that government development policies need to take religious beliefs into account and will fail if they do not. But there is little concern with policies that can achieve the desired objectives, given religious beliefs. This is unfortunate. The result is that administrators, and economists, often frame and carry out policies which ignore non-economic obstacles and consequences. (Admittedly they are also following their natural inclination.)

"Asian Drama" has many excellent sections. It addresses itself to some of the crucial problems of several Asian countries, including issues that have otherwise been neglected. Myrdal is obviously no narrow technician, but the compleat social scientist, who takes into account history and sociology, politics and economics. He deals with a crucial part of the world, and he speaks to a wide public, many of whom have had little or no exposure to the problems he treats. One only wishes that his book were not so uneven, so superficial, and so wrong in parts. Unfortunately, the generalist, even the general sociologist, may find it difficult to distinguish the places where Myrdal writes on the basis of a remarkable insight or about a question with no contradictory evidence, from those where he runs counter to available evidence and is almost certainly wrong. The specialist, on the other hand, unfortunately has to dig through hundreds of pages, some of them repetitive or containing quite uninformative generalizations sure to raise his blood pressure, in order to find a section with interesting ideas and useful analyses. This book will therefore not serve the generalist interested in gaining some understanding of Asian problems. It will be useful for the specialist willing to search for the nuggets of insight and unusual approach. Unfortunately, it is likely to serve most adequately those who want to wash their hands of the whole messy, complex, and drawn-out business which is economic development. They will find in it grounds for an intellectual rationalization of their feelings of tired unconcern.

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The Spirit of Chinese Politics: A Psychocultural Study of the Authority Crisis in Political Development. By LUCIAN W. PYE. Cambridge, Mass.: M.I.T. Press, 1968. Pp. xxii+255. $8.95.

For many years those who have read Lucian Pye's works in the field of "political culture" and who have known of his long familiarity with and interest in China have looked forward to the time when he would turn his attention exclusively to the problems of that country. Whatever is controversial in Lucian Pye, however, has certainly not been laid to rest by this volume. Those critical of Pye's previous efforts are not likely to be dissuaded this time; those who have found his work stimulating and heuristic will find in these pages all they wish to sustain them in their belief.

Pye begins with a main theme that the Chinese, unlike other transitional societies, have been spared a "crisis of identity" but have been left with a basic problem of how to create new forms of authority suitable for modernization and for reasserting a historic self-confidence. Around this theme is woven an analysis based on the central hypothesis that "the Confucian tradition, both structurally and ideologically, created forms of authority that gained strength by denying the legitimacy of sentiments of aggression. Therefore, once this system of

authority was disrupted, the problem of controlling aggression complicated the process of establishing new forms of authority" (p. 32). In succeeding chapters Pye points out that the modern Chinese have been decisively challenged in the very areas which they conceived to be the basis of their past greatness (governance and material civilization) and that the resulting frustration has led to shame and to the release in politics of emotion, particularly hatred. In the still-dominant traditional family socialization pattern, however, control of emotion, especially aggression, is heavily sanctioned. But the too frequent reality of impotent authority in home and society does not meet early expectations concerning competence, resulting in an emotional tension that dominates political behavior. There is thus a contradiction between emotional tension in politics and "self-controlled" organizational behavior which challenges modern leadership and which is the root of the Communist dilemma of "red" versus "expert."

This brief exposition cannot do justice to the richness of Pye's analysis, but it may serve as background for certain criticisms which limit a completely enthusiastic reception of this work. For, while the book is manifestly about the Chinese, Pye tempts the theoretically inclined reader with the statement in the beginning that he will view "China as a transitional political system in the light of our growing body of knowledge about the processes of political development" (p. xix). In fact, however, nowhere is there any serious attempt to analyze China in terms of theory that is generalized for transitional societies as a whole. It is true that comparative statements are made, sometimes profusely (although oddly enough Pye makes almost no use whatever of his previous work on Burma), but not in a rigorous systematic fashion. Perhaps this is because Pye believes that one "must know who the Chinese are before one can really know why something is done" (p. xv). If this is the case, however, then one can only be puzzled over the avowal that in this "psychologically oriented study of political culture" he need "not [be] concerned with questions about the actual distribution of attitudes and feelings throughout the Chinese population" (p. viii). Empirical work (content analysis, interviewing, questionnaires, etc.) is admittedly often boring and tiresome, but a case study without such data too often appears to be an incoherent assemblage of unsubstantiated hypotheses and generalizations. The fact is that, while Pye has said much that may be true and has given us some very powerful explanatory tools, he offers no real assurance of validity beyond his own expertise. His statements about such political behavior patterns as ritualization, mutual suspicion, and mutual aggression, his use of theatrical terms, the notion of being in a drama, etc., come painfully close to being merely modern rewordings of subjective characterological works of the past.

Despite a lack of any empirical anchoring in actual attitudes and feeling, Pye feels no hesitancy about generalizing to the macro level from hypotheses which deal almost exclusively with the individual or primary group. The result is not a happy one, for, although congruence of behavior among social interaction levels is frequently demonstrated, his implication of causality is never adequately substantiated. At best, therefore, one finds the discussion of macro-level politics in the last chapter on "The Political Process in Action: The Communes" an awkward appendage. At worst (although admittedly rarely) one finds a sort of stereotyping of national character which, if applied in our own national context, would be dismissed as rot.

In enhancing the use of "political culture" as a viable approach to the study of modern politics, therefore, this work is only a qualified success. As a guide to the understanding of modern Chinese politics, however, it is a useful and stimulating work.

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A Decade Later: A Follow-up of "Social Class and Mental Illness." By Jerome K. Myers and Lee L. Bean in collaboration with Max Pepper. New York: John Wiley & Sons, 1968. Pp. 250. $7.95.

Ten years after the 1950 survey by August B. Hollingshead and Frederick C. Redlich, a follow-up survey of the same New Haven psychiatric patients began. The first survey had presented evidence for the relationship between social class, on the one hand, and psychiatric diagnosis and treatment experience, on the other. The follow-up survey has investigated three hypotheses:

Hypothesis One. Social class is related to treatment status in 1960 at follow-up.

Hypothesis Two. Social class is related to the patient's treatment and readmission experience during the period from 1950 to 1960.

Hypothesis Three. Social Class is related to the former patient's adjustment in the community.

The reader will immediately be struck with the caution of these hypotheses. They do not suggest *how* social class affects psychiatric variables; they merely suggest that some relationship exists. It seems as if ten years later we ought to be able to move further—especially on the basis of the directions inherent in the earlier Hollingshead and Redlich findings, the critiques of them, and the studies of other workers. Surely we can hypothesize more precisely today what *sorts* of relationships might be found. Specifically, we should be able to predict that the more deprived statuses with respect to form of illness, treatment, and adjustment are correlated with more deprived socioeconomic status, for example. And having so hypothesized, we would also then be on the lookout for more precise and explicit ways of understanding and pinning down the exact relationship if it were indeed to be found.

As a matter of fact, the hypothetical formulation I have suggested does fit the new data in most respects. Yet the authors make too little of this possibility even in *post hoc* terms, not to speak of considerations of study design and plan. Thus the follow-up study will disappoint the reader who has watched the ups and downs of the Hollingshead-Redlich discovery.

Putting aside this fundamental disappointment, the reader can usefully consider the book if he himself wishes to take the forward step past the general examination of whether social class is at all relevant to variables in the total complexity of mental illness. The work is solidly and reassuringly executed on the basic technical level. Actually, the very difficult process of catching up with the previous subjects was quite successfully managed. Additionally, the authors were able to make some corrections and reviews of the earlier sample. Thereby they could actually increase the number of cases to be included in the basic 1950 prevalence group and could follow up these new subjects as well.

On their first two hypotheses, even controlling for a standard variety of features (for example, age, sex, type of hospital), the authors report that the 1950 lower-class hospital patients were more apt to be in hospitals in 1960 than higher-class patients. They were also more apt to be readmitted one or two times; more frequent readmissions were equally spread among the classes, although the higher-class hospital patients of 1950 were more apt to be in outpatient care in 1960. Among all those in outpatient care in 1950, lower-class patients were less apt to be again under outpatient treatment but were more likely to be hospitalized in 1960. Incidentally, this was calculated on patients' status at death, if not alive in 1960, and it was clear also that lower-class patients were more likely to have died in the interim. Indeed, the mortality rate for all patients was higher than the age-specific mortality rates for Connecticut as a whole.

Where the authors did move past the original study was in taking a look at the community adjustment of the patients. They asked whether, compared with controls, patients were different in 1950–1960 work records, current employment, and participation in a variety of social relations. Again, for the most part, as you might have expected (but the authors did not?) the lower the social class, the more problems here, too.

There are all sorts of complications, of course, in the findings that make the relationships not so clean and neat as my summary here. And they are reported carefully. Some day, I would predict, someone will take the careful work of these authors and do a secondary analysis to show that a lot more can be derived from their data than the authors themselves have presented.

STEWART E. PERRY

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Burmese Supernaturalism: A Study in the Explanation and Reduction of Suffering. By MELFORD E. SPIRO. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1967. Pp. x+300. $7.95 (cloth); $3.95 (paper).

Just occasionally one happens on a book which is compelling. This is one of those books. My reaction is surprising, too, because I have no particular interest in Burma and the book is not light entertainment. The book is compelling simply because Spiro has an active, well-trained mind and he lets the reader see it at work ferreting out crucial data, puzzling over anomalies, formulating hypotheses, and making inferences. This is not one of those beautifully formed eth-

nographic nomographs where the reader is presented with a graven image of the society, sans all extraneous and contradictory information, sans the author's doubts about whether he collected enough information or collected it from the right people. Spiro lets you in on the business of social science from the beginning—argues with you about alternative approaches to what he is doing, tells you about the paucity of his data and his biased sample of respondents, tells you about differences of opinion among his respondents, and grapples with one interpretation after another. If you come out agreeing with him in the end, it is not because he has given you only information to support his views, but because you have considered the alternatives with him and agree on your own account with his verdict. Honesty of this kind is a rare quality among us and reflects an unusual intellectual maturity.

Spiro's aim is explicitly stated at the outset of the book: "My main reason for undertaking field work in Burma was to explore the relationships among an otherworldly religious system (exemplified in Buddhism), the personality patterns by which it is maintained, and the social (and, specifically, the political) system by which the group is organized" (p. 8). Because of the visa policies of the Burmese government, the study was limited to fourteen months; an additional field trip which was planned could not take place. One village, Yeigyi, was used as a focus of study, but other villages in upper Burma were also studied for purposes of comparison. Some additional work was also carried out in Mandalay and Rangoon. The basic data were obtained in interviews conducted through an interpreter (although Spiro can apparently conduct a simple conversation in Burmese) and by participant observation of supernatural rituals and festivals.

Spiro is remarkable among ethnographers for his concern about sampling procedures. He attempted to measure as accurately as possible the distribution of cultural beliefs and ritual practices. The book abounds, for example, with such statements as the following: "In Yeigyi almost 60% of the entire male population believe that all shamans are dishonest, another 30% believe that most are dishonest, and only 10% believe that most shamans are to be taken at their word" (page 209). While this does not make for flowing prose, it will make sociologists feel more comfortable about the validity of Spiro's conclusions.

Spiro distinguishes three kinds of explanation in the social sciences, which he designates as causal, configurational, and functional. He proceeds to apply each type of explanation to Burmese supernaturalism. First, on the causal level, he traces the history of Burmese belief in supernatural beings called "nats," showing their emergence by a process of fusion between the animistic spirits of pre-Buddhist Burma and the old Buddhist "devas" or gods. Similarly, Burmese witchcraft beliefs can be traced to indigenous and Buddhist sources. The persistence of these beliefs is then explained by reference to basic personality needs induced by Burmese child-training practices. Following a period of warm nurturance and free indulgence, the Burmese mother subjects her children to sudden rejection. The images of Burmese nats are shown to be isomorphic in a number of dimensions with the rejecting mother. The nats are a useful mechanism, in Spiro's view, in a culture where hostile impulses create extreme guilt and where the value of reverence for parents would be challenged by attributing to them the personal rejection of their children.

Configural explanations show a structural relationship between religion and other institutional variables; religion, in other words, becomes part of a consistent cultural pattern. Spiro pursues this level of explanation by showing that each basic nat type is associated with each of the main structural levels of traditional Burmese society—household, kin, village, district, region, and nation. He argues that there is a perfect fit between nat structure and social structure. Furthermore, not only does this structure reflect the political order, it was created by the political order as part of a deliberate method of political domination and integration. The relationship is confirmed by the similarities in attitude on the part of the people to levels of government authority and to the nats: "Since they cause trouble, avoid them; if they cannot be avoided, placate them; if their assistance is desired, bribe (propitiate) them" (p. 138). This latter explanation is functional, since it explains religious phenomena by reference to their consequences for the maintenance of the system.

Spiro turns next to supernatural rituals and practitioners associated with illness. Particularly valuable are the carefully researched cases of supernaturally caused illness, the first-hand description of healing rituals, and detailed case studies of particular living shamans and exor-

cists. Once again, it should be stressed here that, although Spiro provides detailed descriptive materials, he is not *content* with description alone. He is intemperately curious and is always wanting to know *why* a particular practice exists. He formulates hypotheses and tests them against whatever data are relevant and available. If the evidence isn't available, he finds speculation irresistible, but he is quite frank about the speculative character of his remarks.

A significant portion of the book is concerned with supernaturally caused illness and its cure. A detailed description of an exorcistic seance provides an intimate glimpse into what these beliefs mean in practice and how they lock in with psychic conflicts of individuals. Students of group therapy will be interested in the use of group support for the therapeutic work of the exorcists, and in the legitimation by the group of the expression and articulation of repressed fantasies. Pursuing explanation even further, Spiro pushes his inquiry into the personal characteristics of people recruited to religious roles. Some of these roles, especially that of the shaman, are regarded with contempt by most of the society and often by the shaman himself. Such roles, Spiro argues, provide the kind of immediate and powerful gratification of basic drives generally denied in Buddhist society, and some segments of the population are particularly vulnerable to this appeal at some stages in the life cycle.

The book concludes with an attempt to resolve the apparent contradiction of conflicting beliefs in supernaturalism and Buddhism. Buddhism maintains as its central tenet that conduct in this world cannot affect this life but only subsequent lives. Burmese supernatural beliefs are attempts to produce specific changes of fortune in this life. Buddhism stresses morality, asceticism, rationality, serenity, and otherworldliness, while the nat cultus is amoral, libertarian, non-rational, sensual, and worldly. Spiro argues that Burmese supernaturalism is more than a veneer over an animistic belief system, that the two systems complement each other. The purity of Buddhism is preserved from adulteration, and the repressive features are preserved from overthrow by the channeling of impure and rebellious impulses into a parallel but subordinate system, the nat cultus.

While I have some reservations about particular points in Spiro's argument, these reservations are minor. What is important is that here is a remarkably good case study of religious doctrine and practice in a non-Western society. It stands in the Weberian tradition of accepting a creative tension between theorizing and facts. In my view the book will make a strong contribution to courses in theory, method, religion, and comparative studies in any of the fields of psychology, anthropology, and sociology. There are very few other books of which one could say this.

Dexter Dunphy

University of New South Wales, Australia

Chanzeau, a Village in Anjou. Edited by Laurence Wylie. Cambridge, Mass.: Harvard University Press, 1966. Pp. xxii+383. $8.95.

Osbert Sitwell, in the second volume of his autobiography, *Left Hand, Right Hand,* describes with less than utter sympathy those governesses who year in, year out taught proper French to youngsters of the upper classes in England. The youngsters never learned to speak French, but the teachers remained proud of their own devotion to the purity of their tongue. The breed is not unknown in American colleges, no doubt created by a system which demands "credits" which become "credentials" in subjects which no one expects the students to master. One way to see to it that our students don't speak Canadian French or Haitian creole is to get them through our system without learning to speak French at all; another way is to declare them linguistically qualified for the Ph.D. when they have painfully translated a few pages of sociological French or other pseudo-French in which data from living people could never be collected.

Laurence Wylie was surely never that kind of teacher of French. In the 1950's he went to the field in the south of France and wrote up his findings in *Village in the Vaucluse* (1957), a book which many of us had our students read. Now he has turned from more or less lone field observations to team study of a community as a device for combining teaching and research. He has had a seminar on French civilization (that is what he is professor of at Harvard), whose members work on his village in Anjou; some of them go with him there to work on documents or directly with the people.

The book reporting the study lists about twenty participants in the writing. The group has done a good job on the social, economic, and

political history of the village; and on farming, population, social organization, religion, politics, and on the life cycle of the individual in the community at present. The village is put in its place in the larger dynamic of French life with respect to all of these matters.

The village and the region are conservative and Catholic, devoted to de Gaulle, but disturbed by his policy of drastically reducing the number of farms in France in order to make French agriculture competitive with that of other countries. Chanzeau may become something close to an Appalachia if this program is pursued—or if it is not. It seems that wherever agricultural productivity per acre and per man soars, the poorer regions become poorer yet—relatively or even absolutely. The book gave me a much better understanding of the contradictions of French economics and politics; it serves as background for the understanding of Servan-Schrieber's *The American Challenge* (1968), which was published as *Le défi américain* (1967) last fall.

Wylie has carried out a very successful experiment in combining teaching and research, and of integrating several social sciences. Perhaps his success is in part due to having got his students to collaborate with him and with each other before they had become professionalized and specialized by graduate study. Their French has no doubt been ruined by provincialisms. I don't care what credits they get.

EVERETT C. HUGHES

Brandeis University

Five Towns: A Comparative Community Study. By LOIS R. DEAN. New York: Random House, 1967. Pp. xiv+174. $2.25.

Dean's five towns are in the Midwest, east of the Mississippi, and range from 10,000 to 100,000 in population. They run from a declining mining town, through a stagnant river town, a fairly prosperous city with a university, and an industrial city that is growing, to a completely non-industrial, white-collar suburb. Community leaders were interviewed in such a way as to catch their place on a scale of values with reference to particularism-universalism and the other pattern variables developed by Talcott Parsons. The leaders of the several communities varied, in the main, in the expected directions from the most stagnant and least hopeful town to the prosperous suburb inhabited by successful executives of successful large organizations. The results of the interviews are shown in tables and charts in an effective manner.

One must, at some point, put in a caution as to whether universal, plus "ism," is the right term for the particular worldview of cosmopolitan twentieth-century Western man. That question aside, Dean has done a workmanlike and useful comparison of the leaders of these American towns on dimensions which are important in American life, and which probably have predictive value for American politics and social action in a moderately short run. That is a good deal to have done. I think she does show that towns and cities may be taken as significant units of study, if careful account is taken of their place in larger systems.

EVERETT C. HUGHES

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War: The Anthropology of Armed Conflict and Aggression. Edited by MARVIN HARRIS, MORTON FRIED, and ROBERT MURPHY. Garden City, N.Y.: Natural History Press, 1968. Pp. xix+262. $6.95.

There is an old tradition in the Columbia Anthropology Department of active participation in wars. Ralph Linton took issue with Franz Boas and went off to join the A.E.F. in World War I. Boas himself took an active public role against the Nazis during World War II, and, although I cannot call to mind any specific excitement over the Korean episode, it is clear from the list of participants in this work that Columbia is interested in the Vietnamese war. There is even a rumor that the department took an active part in the recent conflict on its own campus. If we list the editors, the speakers, and the discussants in the present work, which was sponsored by the American Anthropological Association, more than half of them either are Columbia Ph.D.'s or are on the faculty there (or both).

The unevenness that is immediately apparent in reading through the book reflects primarily the philosophical conflict between the desire for active social participation in contemporary issues and the motivation to provide biological background, factual ethnography (description) or wide-ranging ethnology (historical interpretation). That part of the audience participation

in the actual symposium at the annual meeting of the association in December, 1967, at Washington which appears in print here indicates an even greater conflict. This polarity in approach to the question of war is summarized in the book's Foreword, entitled "Fink-out or Teach-in?"

The first two contributions, with their discussants, are earnestly biological or ethological, in the tradition of physical anthropology. F. B. Livingstone, discussed by F. Thieme, concludes that in terms of population genetics war has little or no effect, and R. L. Holloway, Jr., provides a wide-ranging programmatic survey of the biological basis of human aggressiveness which causes his discussant, C. R. Carpenter, to suggest that the paper be more aptly subtitled "The Need for an Integral System of Information." All speakers, however, agree on the relative invalidity of the Lorenz and Ardrey approaches for discussion of the subject.

Then there is a paper by A. Alland, Jr., discussed by B. D. Paul, which provides a sociomedical study of the ecological effects of biological warfare in Vietnam, particularly with reference to rats and plague there. The summation is provided in a quote, which defines biological warfare as "public health in reverse" (p. 75).

The two papers which form the middle section of the book are more classically anthropological. One by A. P. Vayda on "primitive" war is cogently criticized by A. Lesser, and the other, by N. A. Chagnon, is an exciting but complicated ethnographic description of the war systems of a Venezuelan tribe during the current sixties. In his discussion of this paper, E. R. Service has to admit that perhaps some of his own cultural evolutionary concepts should be modified, and he proceeds to demolish the tribe and the band in one fine gesture.

Part IV, which follows, on psychology, provides a contrast between the narrow, logical, and dispassionate arrangement of mobilization, authority, signal, and prevention by better administrative "fail-safe" mechanisms, which is presented by A. F. C. Wallace, and an eloquent and emotional evocation of neo-Marxist sentiments by S. Diamond, who provides disagreeing discussion.

In Part VI two anthropological seniors, Sol Tax and E. A. Hoebel, deal with Congress, the draft, and military in somewhat the fashion of Drew Pearson.

The final section of the book is given to Margaret Mead, although her paper on "Alternatives to War" is discussed by S. Melman, who was not described in the list of participants at the beginning of the book and whom I could not find in the current membership list of the anthropological association. Dr. Mead also contributes an Epilogue. Both Mead contributions are recommended as sophisticated discussions, the best of the book to this reviewer. Other good papers are those by Holloway, Chagnon, and Wallace.

JOHN L. LANDGRAF

New York University

Death in Life. Survivors of Hiroshima. By ROBERT J. LIFTON. New York: Random House, 1968. $10.00.

This book is evidently addressed to the layman, and as such, it gives an excellent description of the effects of a holocaust on large numbers of people. It is instructive and shows the signs of great effort. However, the book is really occupied with the small change of psychiatry; it is repetitious, and many of the discussions are irrelevant to the main theme. There are many philosophical discussions, mostly of the existential variety, which have no real bearing on the book. There are lengthy discussions comparing the victims of Hiroshima with the inmates of concentration camps and the like. These discussions are poor and distracting. The best parts of the book are taken up with the direct testimony of the survivors, their altered attitude to life, and their altered status in society.

But Lifton holds a professorship in psychiatry at Yale; and to the psychiatrist the book is a disappointment. The author shows himself ignorant of psychodynamics, and is apparently not acquainted with the literature on war neuroses, the psychopathology of which has been definitively established. Essentially, the experiences at Hiroshima were a special variety of traumatic experience, and nature does not invent new reaction types for every variety of trauma. Instead of addressing himself to this main topic, he has instead written a meandering piece of journalism in which the emphasis has been misplaced.

A. KARDINER

Emory University

The Nature of Conversion: A Study of Forty-five Men and Women Who Changed Their Religion. By ALBERT I. GORDON. Boston: Beacon Press, 1967. Pp. xii+333. $5.95.

It is perhaps significant that the *Encyclopaedia of the Social Sciences,* published in the 1930's, gave approximately two full pages to the subject of "Conversion, Religious," while the new *International Encyclopaedia of the Social Sciences,* published this year, does not list the subject as either a major or a minor heading, although it does treat conversion sketchily under the major heading, "Religious Observance," and under the minor heading, "The mass media and evangelism." It is unlikely that this de-emphasis reflects the lessening concern with conversion on the part of the Christian churches; it is certain that the de-emphasis reflects a diminution of interest among behavioral scientists in conversion as a research concern. It is surprising to discover, as one searches in vain for recent research data, how frequently the literature refers back to E. T. Clark's *The Psychology of Religious Awakening* (1929), William James's *The Varieties of Religious Experience* (1902); and E. D. Starbuck's *Psychology of Religion* (1899).

Into this lacuna, Albert Gordon's book comes as a welcome and needed contribution. Where the older research had studied conversion in the sense of intensification of religious experience, the fundamentalist Protestant salvation experience, Gordon's study is of ecclesiastical conversion, the changing of affiliation from one organized church to another. Gordon studied forty-five converts, twenty-nine women and sixteen men; thirty converts to Judaism, nine to Roman Catholicism, two to Protestantism, one to Unitarian Universalism. The book describes the relationship between conversion and mixed marriage or interdating in the lives of these converts, the role played by religion in their early lives, their early home lives, their personal independence as reflected in separation from parents and family in young adult life. It includes a typology based on reasons for conversion, and there is a chapter dealing with success and failure of the conversions. What appear to be verbatim transcripts of eighteen interviews are included in the text, and the Appendix includes synopses of twenty-four interviews.

The strength of *The Nature of Conversion* lies in its accumulation of empirical data in a field where previously there were practically none. The typology is thought-provoking. In this reviewer's opinion, the most valuable part of the book is that which deals with success and failure. Not that it tells us about the success or failure of the conversions (after all, a matter impossible to judge this side of Paradise!), but because it tells us about change, or lack of it, in the beliefs, values, attitudes, ritual practices, and identification of the converts. Gordon recognized the difficulty of assessing success and failure, and he chose his criteria consciously and explicitly.

Methodologically, this study leaves something to be desired. In the Introduction, the author makes the usual obeisance to the fact that his study group is not a representative sample and that consequently their experiences may or may not be representative of all ecclesiastical converts. His statement that "as a rabbi it was relatively easy . . . to check back over the years and find those converts whose life-stories were significant and meaningful" suggests a further bias in the selection of the study group. He then proceeds in the first chapter to make numerous generalizations about ecclesiastical conversion: it is seldom a sudden act; it need not imply the heightened emotional experiences that are generally associated with inner conversion; some form of conflict, mental or emotional, is as much involved in ecclesiastical conversion as in inner conversion. "The study of forty-five converts leads me to believe that the vast majority of all ecclesiastical converts have not had a religious experience" (p. 6).

The description of the interview technique is inadequate: The interviews were autobiographical and tape-recorded. They were usually conducted in the homes of the converts and all included the same question, although the order of asking varied in response to the interview situation. The dust jacket describes the interview as "depth," but the text makes no such claim. The interview schedule, if any, is not appended. What kind of interview technique the researcher used, what controls, if any, were placed on the interviews, are not divulged. Knowledge of the sophistication and expertise of the interview technique is crucial in assessing the validity of the analysis of motivation which underlies the typology of converts. Similarly, considering social psychology's painstaking,

continuing focus on the development of techniques adequate to the study of attitudes, Gordon's analysis of attitude change, or lack of it, based on his conversations with the converts, seems much too facile.

The author had named two objectives of this study: (1) "to obtain more detailed knowledge about ecclesiastical converts than has hitherto been available" (p. xii), and (2) "to understand the factors responsible for formal conversion" (p. xi). He has certainly achieved the first objective. However, much more research, and more disciplined research, needs to be done before we can generalize at all about the process of ecclesiastical conversion.

EDNA M. O'HERN

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Negroes and Jobs. Edited by LOUIS A. FERMAN, JOYCE L. KORNBLUH, and J. A. MILLER. Ann Arbor: University of Michigan Press, 1968. Pp. xv+591. $9.00.

This book brings together thirty-two papers by economists, sociologists, civil rights leaders, and others, and are mostly reprinted or excerpted from journals, books, government reports, and pamphlets.

The editors see the controversy over Negro poverty as being a battle between those who see the Negro as unique and those who see solutions in terms of changing persons instead of changing the system. In general, the papers make a convincing argument for the point of view that the Negro is uniquely the victim of a powerful and elaborate system of discrimination.

The papers by sociologists and psychologists are well selected. Jan Dizard's new look at the Negroes' adaptation to their inferior status is first printed here, but the others—by St. Clair Drake, Martin Deutsch, David P. and Pearl Ausubel, Charles C. Moskos, Jr., and Elliot Liebow—are readily available.

For the sociologist who already has access to the sociological references, the most useful papers may be those dealing with policy issues: papers by Bayard Rustin, James Tobin (on the negative income tax), and A. Philip Randolph (presenting the freedom budget), the recommendations of the White House Conference of 1966, and those dealing with fair employment practices legislation and discrimination in employment.

There is also some well-done material on job hunting and migration.

If there were only one book that a sociologist wanted to read on the status of the Negro, this would be a good choice; it touches all the bases, usually in readable fashion, and everyone will find something to disagree with. I hope it will be widely adopted by business schools.

ROBERT L. CRAIN

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Soziologie des Sozialen: Einfuehrung in das soziologische Denken. By HARRY HOEFNAGELS. Essen: Verlag Hans Driewer, 1966. Pp. 285. DM. 14.80 (paper).

What are the implications of the realization that modernization is not only an economic phenomenon but also a political phenomenon and a phenomenon of social differentiation? Does it matter that sociology developed as a reaction to French thought as well as to the industrial revolution, as Professor Nisbet recently reminded us? Without putting it quite in these terms, Father Harry Hoefnagels' book is addressed to questions like these.

In form, the book, which seeks to draw the reader into considering the "social," which is ordinarily invisible, starts from that which is visible, namely, "social problems." But in substance, it is a fascinating exploration of modern societies conceptualized as societies which constantly experience social problems. Following the European terminology, Hoefnagels' close relationship between social problems and *the* social problem, *the* social question—the problem of the revolutionary proletariat. The kind of analysis performed by this Dutch Catholic priest is essentially Durkheimian (a juxtaposition which will not surprise anyone who compares Durkheim's writings with the contemporary Catholic social thinking exemplified in the encyclical *Rerum Novarum*). The social question becomes a question in the course of the transformation of traditional into modern societies. It is a breakdown of the *conscience collective* with regard to beliefs about stratification, beliefs which Durkheim himself had stressed as central to the prevention of anomie (in *Suicide* [Glencoe, Ill., 1951], Vol. II, chap. v, Sec. 2). The liberal critique of the *ancien régime*

helped to create a climate in which, in the latter part of the nineteenth century, the growing working class refused to accept poverty, powerlessness, and status inferiority as either necessary or just. The social question is a crisis of legitimacy, in which society is unable to agree on what basis it should be built. As society becomes a problem to itself, sociology as a discipline arises.

In Hoefnagels' view, the social question was really not a crisis of revolution, but rather a demand for relative social recognition by the working class, a demand for the abolition of stratification beliefs which predicate different estates of, on the one hand, the rich, powerful, and honorable, and, on the other, the poor, powerless, and dishonored. He thus suggests that economic development and political crisis are linked together with demands which are ultimately for equal social standing, for a basic equality of status as citizens *within* which status differences will be acceptable when they arise.

Although he does not discuss in detail the process of the working out of the social question, Hoefnagels observes that linguistic usage shows that it is no loger a problem. It is now "unmodern" to speak of the "social question." Rather, one speaks of "social problems" (pp. 42–43). For him, the prototypical social problem is still the relationship between worker and employer. Social problems are social conflicts. But in Hoefnagels' view, Dahrendorf's conflict theory overlooks the fact that these conflicts are fought not only within a context of consensus on the rules of the game, but also of consensus on many substantive issues as well. As a result, conflicts are largely restricted to one issue at a time. Other issues become sources of conflict as the earlier issues are settled.

For Hoefnagels, modern society neither is nor should be conflict-free. He sees Durkheim's great error as insisting on a greater degree of consensus than is in fact necessary. Modern societies persist despite conflicts, in part because of the discipline learned by the working class in the course of organizing to fight *the* social conflict. And modern societies have an abundance of resources, so that total order does not have to be imposed and accepted merely to insure that all can eat. Demands for social justice, for equal social standing, can be raised and fought out.

Thus, modern societies are characterized by a lack of acceptance of the status quo, by conscious planning of change, by a striving for greater social justice. They are thus structurally characterized by the dialectical tension between the demands of social order and of social justice. The nature of modern society, its "being," is therefore a "becoming" (p. 115). Hence, functional theories which separate social stability from social change are inadequate to the reality of modern society.

I have tried to suggest that this is an important and enticing book, despite the fact that its idiom is not one with which American sociologists are comfortable. Those who wish to read empirical studies and nothing else will not be happy with it. Even those who, like myself, are willing to read works of theory and even of philosophy (in part because we believe that many "methodological" problems are really theoretical, and even philosophical, problems) will often find it irritating. It has its faults. If most American sociologists know little or nothing of European sociology, Hoefnagels' grasp of American work is not what it ought to be. It is hard to understand how a man whose doctoral dissertation, done under Raymond Aron, dealt largely with Durkheim ("La sociologie face aux 'problèmes sociaux' ") can persist in an interpretation of Durkheim's work which does not come to terms with Talcott Parsons' brilliant analysis in *The Structure of Social Action*. The treatment of functionalism (like many hostile treatments) is an exercise in straw-manship. Hoefnagels seems not to be acquainted with the recent work of Howard Becker and others stressing the importance of who defines deviance and how it is defined, an approach which would, I think, be useful in his own argument. But, given all the irritation and dissatisfaction (and what worthwhile book does not provoke such?), there is much to be gained from thinking through the ideas offered here. This Dutch work has been translated into French and German. Isn't there a publisher willing to have it translated into English? It would be a service.

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Father-to-Son Mobility: Australia in Comparative Perspective[1]

Leonard Broom and F. Lancaster Jones

ABSTRACT

Australia is widely regarded as one of the most equalitarian industrial countries. New data indicate that the gross rate of occupational mobility between fathers and sons in Australia is high compared with Italy but somewhat lower than in the United States. However, when structural determinants are taken into account, Australia is found to have the highest rate of circulation mobility of the three countries. Additional detailed evidence for Australian occupational mobility is briefly summarized and interpreted in the context of industrial change.

This report of findings from the first national survey of social stratification and social mobility conducted in Australia attempts to fill a gap noticed by students interested in the comparative analysis of mobility.[2] The findings of our Australian survey serve as the point of departure for comparisons with recent data for Italy and the United States. There have been a few small Australian studies of restricted localities and some interesting analyses of mobility into specific high-status occupations—typically, professional and administrative.[3] There is also one major study of career and generational mobility in New South Wales based on a random sample of marriage records filed in 1960.[4] Because of differences in study design, however, we do not attempt here to compare the results of that study with our own, which pertains to the adult male work force of the country as a whole.

[1] We acknowledge with thanks the collaboration of J. Zubrzycki, who participated in the design and administration of the survey upon which this paper is based. We also thank the Australian National University for funding the study and the Roy Morgan Research Centre (Australian Gallup Poll) for conducting the interviews. The research was facilitated by W. D. Borrie, P. Partridge, Ralph Tyler, and W. Gordon Whaley.

[2] Seymour Martin Lipset and Reinhard Bendix, *Social Mobility in Industrial Society* (Berkeley: University of California Press, 1959), pp. 29, 67; S. M. Miller, "Comparative Social Mobility," *Current Sociology*, IX (1960), 24.

[3] For generational mobility, see O. A. Oeser and S. B. Hammond (eds.), *Social Structure and Personality in a City* (London: Routledge & Kegan Paul, 1954), p. 239; and Alan Davies, *Images of Class* (Sydney University Press, 1967), p. 72. For inflow analyses, see H. Y. T'ien, "A Profile of the Australian Academic Profession," *Australian Quarterly*, XXXII (March, 1960), 66–74; G. W. Bassett, "The Occupational Background of Teachers," *Australian Journal of Education*, II (1958), 79–90; A. F. Davies and S. Encel, *Australian Society: A Sociological Introduction* (Melbourne: F. W. Cheshire, Ltd., 1965), pp. 27–28; Robert M. Pike, "Some Social Aspects of Recruitment to Public School Teaching in New South Wales," *Australian and New Zealand Journal of Sociology*, II (October, 1966), 94–106.

[4] John D. Allingham, "Class Regression: An Aspect of the Social Stratification Process," *American Sociological Review*, XXXII (June, 1967), 442–49. This study employs the novel technique of linking marriage and birth records to obtain information on son's occupation at marriage, father's occupation at son's marriage, and father's occupation at son's birth. Because his study relies on in-state records, only marriages involving men born and married in New South Wales were retained in the sample. For this reason, and because the study design necessarily generated an age distribution that does not correspond to the work force as a whole, the findings of this important study cannot readily be generalized to the male work force of New South Wales or of Australia as a whole. It does, however, illustrate the possibility of reconstructing mobility data from public records and provides an opportunity to examine age-specific mobility patterns, because of the simultaneous control of father's and son's age obtained by sampling marriage records.

METHOD

The data reported in Tables 1 and 3 were obtained in a sample survey of Australia's adult male work force conducted between February and May, 1965. We designed the survey and interviews were carried out by the Roy Morgan Research Centre (the Australian Gallup Poll), with some supplementary interviews conducted by the Department of Sociology at the Australian National University. Electoral rolls were used as a sampling frame in twenty-three of 122 federal electorates in Australia, and ten names (starting addresses) were randomly selected from the rolls for each sampled electorate. Interviewers were instructed to call at the starting address and adjoining residences until ten interviews had been completed wtih an adult male worker twenty-one years of age or older (or with his wife). Only one interview per dwelling was attempted in 2,319 "eligible" dwellings, yielding 1,925 completed interviews, an over-all response rate of 83 per cent.

The geographical representativeness of the sample was good, with 56 per cent of respondents resident in capital cities, 27 per cent in "other urban" centers, and 17 per cent in rural areas. The comparable figures for the 1966 Census of Australia (total male population) were 57, 25, and 18 per cent. There was some underrepresentation among younger workers, the foreign-born, and the lower occupational strata, where the young and the foreign-born tend to be concentrated. A more detailed statement on the representativeness of our sample must await the publication of the relevant 1966 census tabulations. No adjustments of our data have yet been attempted.

Occupational data for three points were secured: (1) the occupation of the respondent at interview; (2) his occupation when he bgan regular employment; and (3) his father's occupation at the time of interview or, if the father was deceased or retired, his last regular job. The survey also explored the major elements of the Australian stratification system and called for information on other measures of social rank, and on attitudinal and life-style correlates of social rank.[5] This paper is limited to an analysis of what is customarily called intergenerational occupational mobility, that is, the relationship between the present occupation of respondents and that of their fathers.

In Table 1, our findings are presented in terms of the conventional non-manual, manual, and farm division of occupations. The detailed list of sixteen occupational categories shown in Table 3 was derived from the Australian Census classification of occupations. Census code books were used in allocating responses to specific occupational groups.[6] Although the sixteen categories form a scale of occupational prestige, we have preserved meaningful industrial divisions where possible. It should be noted that shop assistants (in American commerce, low-skilled retail store employees) are ranked below craftsmen, a position that reflects more accurately the standing of these occupations in Australia, the United States, and the United Kingdom.[7] Our non-manual category consists of

[5] Leonard Broom, F. Lancaster Jones, and Jerzy Zubrzycki, "Social Stratification in Australia," *Sociological Studies*, I (Cambridge: Cambridge University Press, 1968), 212–33, and "Career Mobility in Australia," presented at the meeting of the Pacific Sociological Association, San Francisco, March, 1968.

[6] Leonard Broom, F. Lancaster Jones, and Jerzy Zubrzycki, "An Occupational Classification of the Australian Work Force," *Australian and New Zealand Journal of Sociology*, Vol. I (Suppl.; October, 1965).

[7] Oeser and Hammond, *op. cit.*, p. 280, found that shop assistants were seen by both middle- and working-class respondents as belonging predominantly to the working class. Congalton's survey of occupational prestige in Australia placed shop assistants and sales persons (chain store) at the bottom of the clerical and sales group, with a rank-score comparable with several semiskilled and service occupations (Athol A. Congalton, *Occupational Status in Australia* [Kensington: University of New South Wales, School of Sociology, 1963], pp. 13, 15). See also Robert W. Hodge, Paul M. Siegel, and Peter H. Rossi, "Occupational Prestige in the United States," *American Journal of Sociology*, LXX (November, 1964), 286–302; Peter M. Blau

groups 1, 3, 4, 5, 7, and 8 as shown in Table 3; manual, of groups 9, 10, 11, 12, 13, 14, and 16; and farm, of groups 2, 6, and 15. The location of farm occupations at three points in the hierarchy recognizes different levels of prestige in rural work.

INTERNATIONAL COMPARISONS

The main value of statistics presented in the broad categories of Table 1 is for comparative analysis. Comparisons should, however, be confined to surveys whose data derive from similar sampling frames and whose units (occupations) are reported in comparable categories. Prior Australian data are unsuitable for comparisons, for reasons mentioned above. We therefore limit ourselves to comparisons with two recently published national surveys on Italy and the United States which appear to satisfy the criteria mentioned.[8]

Table 1 shows the outflow mobility from fathers' occupational strata to the strata currently occupied by their sons, as recorded in surveys in Australia in 1965, Italy in 1963–64, and the United States in 1962. An immediate observation is the large proportion immobile in non-manual and manual occupations in all three countries. In Italy and America seven out of ten sons from non-manual backgrounds were themselves in non-manual jobs at the time of interview. The Australian figure, while less (six out of ten), is nevertheless high in absolute terms. In all three countries, between one-quarter and one-third of sons of non-manual fathers were downwardly mobile into manual occupations and a much

TABLE 1

FATHER-TO-SON MOBILITY IN AUSTRALIA, ITALY, AND THE UNITED STATES

Father's Occupation (Columns)			Son's Occupation* (Row Percentages)			
Occupational Division	Number	Percentage	Non-manual	Manual	Farm	Total
Australia:						
Non-manual........	486	26	*60*	35	6	101
Manual............	935	51	31	*65*	4	100
Farm..............	427	23	19	40	*41*	100
Total.............	1,848	100	35	52	13	100
Italy:						
Non-manual........	209	16	*70*	26	4	100
Manual............	526	39	20	*76*	5	101
Farm..............	603	45	8	39	*53*	100
Total.............	1,338	100	22	51	26	99
United States:†						
Non-manual........	7,517	22	*69*	29	1	99
Manual............	15,985	47	36	*62*	2	100
Farm..............	10,470	31	22	56	*22*	100
Total...............	33,972	100	39	53	8	100

* Cells are to be read as follows: Of Australian workers whose fathers were non-manual workers, 60 per cent were themselves non-manual workers, 35 per cent were manual workers, and 6 per cent were farm workers.

† The American numbers (in thousands) represent population estimates derived from an initial sample of about 20,700 respondents.

SOURCE.—Australia, 1965 survey referred to above; Italy, Lopreato, *op. cit.* (1963–64 survey); and the United States, Blau and Duncan, *op. cit.*, (1962 survey).

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and Otis Dudley Duncan, *The American Occupational Structure* (New York: John Wiley & Sons, 1967), pp. 26–27; and D. V. Glass (ed.), *Social Mobility in Britain* (London: Routledge & Kegan Paul, 1954), pp. 29–32, 63.

[8] Joseph Lopreato, "Social Mobility in Italy," *American Journal of Sociology*, LXXI (November, 1965), 311–14; and Blau and Duncan, *op. cit.* For purposes of comparison, the category of retail salesman in the U.S. study has been included among manual occupations.

smaller proportion were mobile into farm occupations.

The pattern of mobility among sons of manual fathers, while similar in general outline in all three countries, exhibits greater variety. The figures for upward mobility into non-manual jobs are particularly interesting. More than one in three sons from manual origins in the American sample were upwardly mobile, compared with only one in five in the Italian sample. The Australian figure is intermediate, but is closer to the American than to the Italian experience. If this movement can be taken as indicative of the degree of openness in the stratification system, then the United States has the most open, and Italy the least open, stratification system of the three countries.

The pattern of mobility from farm origins varies markedly and throws light on the processes of occupational mobility in societies at different stages and with different rates (perhaps different types) of industrial change. The United States shows by far the highest rate of mobility out of farm occupations: only one of five sons from farm origins remained in farm work, three moved to manual, and one into non-manual occupations. In Australia twice as many, proportionately, remained immobile in farm jobs. While farm to non-manual mobility was comparable with the high U.S. rate, mobility from farm to manual jobs in Australia was only two-thirds that of the American figure. The Italian pattern is similar to the Australian, except that its larger degree of immobility is associated with quite low mobility into non-manual jobs.

Observed differences and similarities in mobility rates illuminate, and are illuminated by, differences in the opportunity structures within which they occur. For example, the similarity between the percentage immobile in non-manual occupations in Italy and the United States masks the different implications of these statistics, which must be seen in the context of a more rapid expansion of non-manual job opportunities in the United States. As Table 1 shows, in the American sample there were almost twice as many non-manual sons as fathers (39 and 22 per cent, respectively), whereas in the Italian sample this relative difference was much less pronounced (22 per cent, compared with 16 per cent). This means that while the percentage of immobile non-manual sons was the same in the two countries, the opportunities for upward mobility among sons of manual or farm origins were much greater in the United States than in Italy. In the Australian sample, the comparable difference in the relative number of non-manual jobs was less marked than in the United States but greater than in Italy. However, because of lower immobility in the non-manual stratum, the rate of movement from manual to non-manual jobs more nearly approached the United States figure than the outflow percentages suggest. Viewed from an inflow perspective, the proportions of non-manual sons from manual origins were similar in Australia and the United States (44 and 43 per cent, respectively), but rather lower in Italy (35 per cent). These percentages are not shown in the table.

In interpreting comparative rates of occupational mobility, whether between countries at about the same time or within the same country at different times, it is essential to partition observed mobility into two components: structural, or "forced," mobility and circulation, or "free," mobility. Structural mobility occurs wherever the relative occupational distributions of fathers and sons differ, when it is structurally impossible for all sons to occupy the same strata as their fathers because of differences in the relative size of those strata. If, as is usually the case, the marginal distributions of an occupational mobility table differ, a minimum—and variable—amount of structural mobility is required simply to transform the origin distribution of fathers' occupations into the destination distribution of respondents' occupations. To put it concretely, Table 1 shows that in each of the three countries considered, *all* respond-

ents from non-manual and manual origins, but only *some* respondents from farm origins, could have remained immobile, simply because many more fathers than sons had farm occupations. In Australia, for example, 187 men from farm origins (10.1 per cent of all respondents) had to be displaced from the farm stratum, given the present occupational distribution of the sample. In Italy, structural mobility amounted to 18.8 per cent, and in the United States it was even higher—23.1 per cent.

These differences in structural opportunities partly—but only partly—reflect gross movements over time in the occupational composition of the whole labor force. It is a simple matter, for example, to document the long-term decline in the relative number of farm occupations in each of the three countries concerned. Two erroneous inferences, however, need to be avoided. The first is that the fathers "represent" any pre-existent labor force, and the second is that the fathers and sons represent *a* generation of fathers and *a* generation of sons. To digress briefly, it is clear that in these and similar sample surveys, the respondents (sons) do indeed represent a cross-section of an existing labor force. The fathers, however, are not similarly isomorphic to any pre-existent labor force. The fathers of some young respondents may still be at work, while the fathers of the oldest respondents may be long dead. The fathers of immigrants belonged to the labor force of another country, and differential fertility (counterbalanced to some extent by differential mortality) insures the overrepresentation of some occupational groups and the underrepresentation of others. But the most important, and elementary, difficulty is that although all men *have* fathers, not all men *are* fathers. At any point in time the work force of a country will include many men who do not, and never will have a son. Indeed, it is likely that in industrialized countries a majority of the male work force consists of men without sons.

We can illustrate this point by considering men who (if married) have wives who are, on average, beyond childbearing age. The 1961 Census of Australia indicates that 9 per cent of male workers aged fifty to fifty-nine had never married. They are *ipso facto* (apart from illegitimate issue) without sons. Among those of this age who did marry, it can be estimated that about 20 per cent had no offspring and that a further 17 per cent who did have children did not have a son. To the extent that these estimates derive from fertility figures for currently existing marriages, they are probably slight overestimates.[9] But even assuming identical fertility patterns for separated, divorced, and widowed women, it is clear that a substantial minority—between an estimated minimum of 36 per cent and an estimated maximum of 44 per cent—of men between fifty and fifty-nine in 1961 did not have a son. In other words, when present-day respondents are asked about the occupations of their fathers, they can tell us nothing about those many men who left no sons to speak for them and to place them in "intergenerational" mobility tables. Clearly, any attempt to reconstruct the past with such evidence is subject to serious limitations. If such reconstructions are to be attempted, it is necessary not only to clarify trends and quality inferences by making corrective reference to census data, but also to obtain information on men who have no sons. While the strategy of asking men about their fathers is eminently suited to answering questions about social origins (inflow analysis), a complete account of occupational change between generations involves statements about men (and women) who fail to contribute representatives to the next generation, as well as statements about those who do. In short, we need to ask men about their sons (if any) and to identify the differentiating charac-

[9] The figures are for currently married women aged fifty to fifty-nine (Australian Bureau of Census and Statistics, *Census of the Commonwealth, 1961*, Vol. VIII: *Australia: Statistician's Report* [Canberra, 1967], chap. xxi).

teristics of men with sons and men without sons.[10]

A second erroneous inference is that the sons and fathers represent distinct generations. However the concept "generation" is defined, any sample of adult workers contains more than one generation.[11] The age spread among respondents in our Australian sample is about forty years, and because fathers vary in the age at which they become fathers, their age spread is even greater. Some of the fathers may have been born as recently as 1925, while others were presumably born as long ago as the 1860's. A sample of respondents who themselves vary in age by as much as forty years cannot be meaningfully termed "a" generation. This usage is even less meaningful when applied to fathers. In kinship terms, each individual son and his father represent two generations, but sons and fathers of widely different ages represent several cohorts and do not form two distinct labor-force generations. Greater attention needs to be given to identifying distinctive cohorts of sons and fathers, either through the analysis of certain types of registration data[12] or (most definitively) through the use of larger sample surveys to allow the analysis of mobility patterns among specific age cohorts.[13] Our own sample is not sufficiently large to sustain such detailed analyses, except for gross occupational categories.

Bearing these qualifications in mind, we can return to the interpretation of the observed rates of outflow mobility shown in Table 1. Table 2 presents some summary measures of generational mobility.[14] The first statistic requires no explanation and represents the percentage of respondents who occupied a different occupational stratum from their fathers. It is obtained simply by expressing the number of mobile respondents (the total sample minus the cases in the diagonal) as a percentage of all respondents. The United States emerges with the highest rate of observed mobility (48.8 per cent), Italy with the lowest (35.4 per cent), while Australia (41.6 per cent) is midway between the two. For those who might question the substance of percentage differences of "only" 6 or 7 per cent, we

TABLE 2

SUMMARY STATISTICS OF MOBILITY FOR AUSTRALIA, ITALY, AND THE UNITED STATES

Mobility*	Australia (%)	Italy (%)	U.S.A. (%)
1. Total observed mobility	41.6	35.4	48.8
2. Structural mobility	10.1	18.8	23.1
3. Circulation mobility	31.5	16.6	25.7
4. Expected mobility	61.6	64.4	64.0
5. Deviation of observed from expected	−20.0	−29.0	−15.2

* For definitions of these statistics, see text. Row 3 is the difference between rows 1 and 2. Row 5 is the difference between rows 1 and 4.

[10] For related discussions of methodological difficulties in the interpretation of mobility data, see Blau and Duncan, *op. cit.*, pp. 82–84, and the chapters by Otis Dudley Duncan and Natalie Rogoff Ramsøy in Neil J. Smelser and Seymour Martin Lipset (eds.), *Social Structure and Mobility in Economic Development* (Chicago: Aldine Publishing Co., 1966).

[11] Norman B. Ryder, "The Cohort as a Concept in the Study of Social Change," *American Sociological Review,* XXX (December, 1965), 843–61.

[12] For example, marriage and birth records (Allingham, *op. cit.*) or population registers (Gosta Carlsson, *Social Mobility and Class Structure* [Lund: Gleerup Bokförlag, 1958]).

[13] See Blau and Duncan, *op. cit.*, pp. 177–88.

[14] These are the measures defined by Elton F. Jackson and Harry J. Crockett, Jr., "Occupational Mobility in the United States: A Point Estimate and Trend Comparison," *American Sociological Review,* XXIX (February, 1964), 5–15.

point out that if the observed rate of mobility in the United States had been as low as Australia's, about 2.5 million additional American workers would have remained immobile. If it had been as low as Italy's, the comparable figure would have been 4.5 million.

The high rate of observed mobility in the United States partly reflects the rapidly changing composition of the American labor force. Census statistics show that, in the fifty years from 1910 to 1960, the proportion of farm workers fell from 35 to 8 per cent of the male labor force in the United States, from 54 to 28 per cent in Italy, and from 37 to 15 per cent in Australia.[15] Thus, the absolute percentage decline in the farm labor force is about the same in the United States (27 per cent) and Italy (26 per cent), but in Australia this push toward occupational mobility has been less strong (22 per cent). Of course, the relative rates of decline are as important as the absolute changes, and the decline of 27 per cent in the United States from a base of 35 per cent clearly indicates a greater shift in industrial structure than does the decline of 26 per cent in Italy from a base of 54 per cent. Moreover, the possibility for continued declines in the agricultural sector is now quite limited in the United States, whereas in Italy, and to a lesser extent in Australia, further declines may be expected.

For identifying the amount of structural mobility in these three countries, the important consideration is the relative distribution of fathers and sons in the different samples rather than the secular changes in the labor force as a whole. Among fathers, 23 per cent in the Australian sample, 45 per cent in the Italian sample, and 31 per cent in the United States sample were in farm occupations. Among sons, the comparable figures were 13, 26, and 8 per cent, yielding relative differences (in this case identical with the amount of structural mobility) of 10, 19, and 23 per cent. By subtracting these last figures from the figures for total observed mobility, we obtain the amount of circulation mobility, which measures the portion of the observed mobility that was not structurally determined. This statistic (given in row 3 of Table 2) is of considerable interest, since it suggests how open the occupational system would be in the absence of structural demands for mobility. It shows that Australia has a very high degree of circulation mobility, which lends support to the often expressed view that Australia is the most equalitarian of the Anglo-American democracies.[16] The United States has a lower degree of circulation mobility and Italy the lowest, a ranking which coincides with informed opinion about the degree of openness of the social stratification systems in these countries.

To this point we have interpreted the data of Table 1 in terms of departures from what can be termed a maximum stability model. We can also interpret them in terms of departures from a perfect mobility model, in which the occupation of a son is assumed to be statistically independent from his father's. In such a world of "perfect equality" we would expect sons from all social origins to have equal access to any occupational stratum: their chances of entry are proportional not to the social rank of their father's occupation but only to the number of job opportunities in a given stratum of destination. This is the model of conventional contingency analysis, and row 4 of Table 2 indicates the amount of mobility that would be expected by applying this assumption to the three samples considered. The expected rate of mobility differs little from one country to another, from 62

[15] For American data (occupations), see Otis Dudley Duncan, "The Trend of Occupational Mobility in the United States," *American Sociological Review*, XXX (August, 1965), 491–98; for Italian data (industry), see SVIMEZ, *Un secolo di statistiche italiane: Nord e sud: 1861–1961* (Roma, 1961); for Australian data (industry), see Australian Bureau of Census and Statistics, *op. cit.*, p. 252.

[16] Cf. Seymour Martin Lipset, "The Value Patterns of Democracy: A Case Study in Comparative Analysis," *American Sociological Review*, XXVIII (August, 1963), 515–31.

per cent in Australia to 64 per cent in Italy and the United States. Since the full-equality model accepts the marginal distribution of sons and fathers as given, the appropriate comparison is between rows 1 and 4 of Table 2. In all three countries the observed rate of mobility falls short of that expected from the full-equality model, but also greatly exceeds the minimum structural movement, particularly in Australia.

Italy has the lowest rate of total mobility (but still relatively high, amounting to just over one-third of the total sample), and structural mobility slightly exceeds circulation mobility. Observed mobility attains just over half the level expected from the assumption of full equality, and Italy is clearly less mobile and inferentially less equalitarian than the other two countries. These general conclusions are partly supported by the over-all relationship between fathers' and sons' occupations.

TABLE 3

FATHER-TO-SON MOBILITY IN AUSTRALIA (PERCENTAGES)

Father's Occupation	Son's Occupation																	
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Total %	No. of Fathers
1. Upper professional	*27*	...	11	19	2	2	18	4	12	2	2	...	2	...	...	...	101	56
2. Graziers	4	*45*	1	7	...	7	4	1	8	2	4	7	1	...	4	4	99	91
3. Lower professional	20	...	*8*	24	...	...	24	...	4	4	...	4	4	...	4	4	100	25
4. Managerial	10	4	7	*21*	1	3	19	1	13	2	8	3	4	...	2	3	101	173
5. Shop proprietors	6	...	2	10	*12*	6	14	2	15	4	14	8	2	2	2	2	101	51
6. Farmers	1	4	1	10	1	*35*	4	0	12	2	10	9	3	1	3	4	100	276
7. Clerical workers	13	1	6	15	2	1	*21*	1	17	2	10	3	3	1	2	2	100	146
8. Armed services, police	14	...	3	11	...	3	14	*6*	17	3	3	6	9	...	...	11	100	35
9. Craftsmen	4	0	4	10	1	1	12	1	*39*	2	9	5	4	1	0	7	100	341
10. Shop assistants	13	...	...	9	...	...	13	4	39	*17*	4	...	...	...	...	...	99	23
11. Operatives	3	1	4	9	1	7	11	1	26	1	*17*	9	5	...	1	5	101	148
12. Drivers	2	...	2	12	1	3	11	1	26	1	11	*17*	7	2	...	5	101	119
13. Service workers	6	...	4	17	...	2	14	1	29	4	8	4	*7*	1	..	3	100	91
14. Miners	...	...	...	5	2	2	7	...	32	3	11	3	7	*14*	...	14	100	57
15. Farm workers	2	2	2	10	3	5	5	2	20	5	8	8	7	...	*8*	13	100	60
16. Laborers	3	...	4	7	1	3	14	...	28	3	13	6	5	...	4	*10*	101	156
17. Not stated	4	1	...	8	3	1	9	1	17	4	12	18	4	3	5	10	100	77
Total %	6	3	4	12	1	7	12	1	23	3	9	7	4	1	2	6	101	1,925
No. of sons	109	64	70	225	26	145	228	19	434	48	182	132	79	19	37	108	1,925	.....

The following conclusions seem justified. The United States has the highest rate of observed mobility but also the highest rate of structural or "forced" mobility. Additional (circulation) mobility slightly exceeds that necessary to transform the occupational distribution of fathers into that of their sons, and the total observed mobility amounts to fully three-quarters of the level expected from the full-equality model. Australia has a somewhat lower rate of total observed mobility, but by contrast with the United States, circulation rather than structural mobility is more important in the total mobility pattern. However, largely because of a less rapid transformation of the labor force, total observed mobility amounts to about two-thirds of the level expected from the full-equality model. In a word, Australia displays less total occupational mobility than the United States, but more circulation mobility. On the basis of this last figure, Australia emerges as more equalitarian.

AUSTRALIAN TRENDS

In Table 3 we present our findings for Australia in more extended form. The first

point that merits comment is the over-all low degree of immobility. Only one in four sons (24 per cent) is found in the same occupational niche as his father, and in most categories the figure is much smaller. To some extent the high level of mobility reflects the number of measurement categories, but even so it is noteworthy that in no case does the diagonal element (the percentage immobile) exceed 50 per cent. Occupational inheritance is most marked among graziers (large scale pastoralists), craftsmen, farmers, and to a lesser degree upper professionals, but in the remaining twelve categories an average of only one in seven sons filled the same occupation as his father. The impression of high mobility is further strengthened by the fact that the father's occupation was the commonest destination of his son in only five of the sixteen categories.

In most groups, mobility was more heavily concentrated within the broad divisions of non-manual, manual, and farm jobs. Thus, while only 27 per cent of the sons of upper professional fathers were themselves in that stratum at time of interview, the majority of the mobile sons were in other non-manual rather than in manual jobs; and of those in manual jobs, two out of three were skilled workers. A similar pattern can be observed in the lower professional category and to a lesser degree among sons of managerial and clerical fathers. Only among shop proprietors and members of the armed services and police (a small but inevitably heterogeneous category) was mobility into manual jobs as high as the percentage remaining within the non-manual category.

For the sons of manual fathers, it can be similarly observed that a minority left manual for non-manual strata. The rate of upward mobility was lowest among miners (16 per cent), somewhat higher among unskilled, semiskilled, and skilled manual workers (29–32 per cent), and highest of all among shop assistants and service workers: about two in five sons from these strata of origin were mobile into non-manual jobs.

The pattern among farm occupations is less uniform. Only among graziers did a majority of sons remain in farm occupations (56 per cent). Slightly less than half this number moved into manual jobs; one in six moved to a non-manual job. In all three farm categories, most mobile sons entered manual rather than non-manual jobs, a pattern evident in the Italian and American figures cited above. It is also interesting that the rate of mobility from farm occupations was inversely related to prestige. It was highest among farm workers (85 per cent), lower among farmers (68 per cent), and lowest among graziers (44 per cent).

The differential nature of farm mobility directs attention to the changing composition of the rural labor force and to the selective social characteristics of rural-urban migrants. In advanced industrial societies, the character of rural work itself is changing. As the scale of agricultural enterprise increases, urban-industrial technology and formal education supplant traditional farming methods and the managerial revolution extends to farm as well as non-farm occupations. It is tempting to interpret the higher movement from non-manual to farm jobs in Australia (Table 1) as evidence that the translation of urban skills into the rural environment has proceeded further than in the United States or Italy and to suggest that the superficial similarity between the Italian and Australian figures for non-manual–farm mobility reflects in fact opposite processes.

The Australian scene is peculiar and prophetic. Australia has never experienced the smallholder settlement of the New World or the peasant agriculture of the Old. Large-scale primary industries geared to export markets have long dominated the Australian economy. Under such circumstances, there is little transitional strain to the further rationalization of capital-intensive, highly skilled, large-scale agricultural enterprises, which we perceive as the emergent and characteristic mode of rural activ-

ity in advanced societies. As this transition is completed, and if, consequently, the relative distribution of primary, secondary, and tertiary jobs approaches stability in industrial nations, structural determinants of occupational mobility between the generations will diminish.[17] Observed differences in mobility between industrial nations would then no longer be related to changing opportunity structures, but would reflect the degree of openness of their stratification systems.

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[17] We do not accept the primary-secondary-tertiary industrial classification as an immutable and universally valid scheme, but to extrapolate present trends into a new technological era lies beyond the purposes of this paper and the prescience of the writers.

Marks of Distinction: An Analysis of British Mass and Prestige Newspaper Editorials[1]

J. Zvi Namenwirth

ABSTRACT

A content analysis of editorials produced a number of orientational dimensions which differentiate between three British prestige and three British mass papers. These orientations are interpreted as formal (rather than substantive) marks of distinction, which not only characterize the differences between prestige and mass newspaper editorials but also the different cognitive styles of elites and masses in general. These orientations, then, are seen as specifications of more abstract and general principles of information processing for a particular time and place.

PREMISE

The concept "elite" is charged with political passion. Leaders are bad enough, but elites are a particularly evil breed. In the predominant, egalitarian view, the elite's claim to special qualities is false. The claim to privileges is therefore unfounded, and, were it not for dark conspiracies, their existence would soon be leveled. Theoreticians who argue otherwise are called "elitists" because their ideas are seen as acts of will which create and maintain the inequalities of mass and elite society. No doubt, the egalitarian view exaggerates the power of ideas as well as the power of the will. Distinctions between the masses and the elites seem a persistent fact of life. Probably produced by the mechanisms of social structure rather than the will of man, they remain a central topic for sociological inquiry. In this paper I shall study the contrast between mass and prestige newspapers as a representation of this larger issue of social inequality.

Much empirical research[2] takes the distinction between mass and prestige papers for granted. Differences in circulation size support this assumption: prestige papers serve the few, mass papers the many.[3] Furthermore, there are well-known qualitative differences. Prestige papers, for example, have more extensive news coverage, more foreign news, and more specialized news about such things as finance, science, and the arts. Mass papers have less news, more purely local stories, feature articles, political cartoons, "lonely heart" columns, and comic strips. And these quantitative and qualitative differences are not fortuitous: they are believed to reflect fundamentally opposed orientations toward the audience and the world which pervades all newsprint. In this view, prestige papers are wedded to studied impartiality, while mass papers are characterized by strident partisanship. To support such observations,

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[2] For example, Ithiel de Sola Pool *et al.*, *The "Prestige Papers": A Survey of Their Editorials* (Stanford, Calif.: Stanford University Press, 1952).

[3] See Vilfredo Pareto, in S. E. Finer (ed.), *Sociological Writings* (New York: Frederick A. Praeger, Inc., 1966), esp. pp. 152–54.

these researchers implicitly resort to theory rather than fact by relating the various interests and predilections of sponsorship, editors, and readership. According to this theory, the editors of the elite press are at once the mouthpiece of the government and the representatives of a knowledgeable elite, thus transforming, reinforcing, and sanctifying national elite orientations. Consequently, prestige newspaper content becomes an authoritative source of governmental orientations; indeed, frequently the only source, since spokesmen of these orientations are often personally inaccessible.[4]

In accord with these arguments, I analyzed the contents of prestige newspaper editorials from various countries to assess and compare these countries' elite orientations.[5] The premises of such research, however, need to be re-examined. Not only has the existence of national elites been questioned, there is also the fact that many countries have more than one prestige newspaper.[6] This raises the question of whether the various prestige newspapers express a unified elite orientation or whether each paper has its own peculiar elite following and orientation. If elite newspaper editorials indeed reflect a unified orientation of national elites, then the orientations of such editorials should be quite similar in a number of these papers. The question, of course, is how to define "similar" in an operational manner.

Having already examined editorial positions regarding European unification and Atlantic issues in French, German, British, and American elite organs, I proceeded to examine these same matters in two additional British prestige newspapers and three British mass newspapers. Based on earlier research, the similarity test was specified according to two different points of reference: foreign prestige newspapers and British mass papers.

If the orientations in British prestige newspapers are indeed alike, then the editorials of the *Manchester Guardian* and the *Daily Telegraph* should be closer to the orientations of the London *Times* than to those of foreign prestige papers (in this case, the *New York Times*, *Le Monde*, and the *Frankfurter Allgemeine Zeitung*). The discriminant analysis considered editorials from the *Manchester Guardian* and the *Daily Telegraph* no more similar to London *Times* editorials than to editorials from foreign newspapers.[7]

If, however, the criterion of similarity is that the differences *among* British prestige paper editorials are fewer than such differences between these editorials and mass newspapers, then the conclusion must be more ambiguous. In that case, one can say, at best, that there are some formal rather than substantive distinctions between mass and prestige papers. In this paper I shall describe, interpret, and explain these distinctions.

DESIGN

To describe newspaper editorials, I have analyzed a sample of editorials chosen at random[8] from a complete list of all editorials dealing with Atlantic and European issues which appeared in 1963 in three British prestige papers (London *Times*, *Daily Telegraph*, and *Manchester Guardian*) and three mass papers (*Daily News*, *Daily Express*, and *Daily Mail*). The sample consisted of twenty-four editorials for each newspaper—144 editorials in all.

[4] It is often extremely difficult to use interview procedures for the assessment of orientations held by ruling elites, and these procedures are completely impossible for historical research.

[5] J. Zvi Namenwirth and Thomas L. Brewer, "Elite Editorial Comment on the European and Atlantic Communities in Four Countries," in Philip J. Stone *et al.*, *The General Inquirer: A Computer Approach to Content Analysis* (Cambridge, Mass.: M.I.T. Press, 1966), pp. 401–27.

[6] Cf. Bernard C. Cohen, *The Press and Foreign Policy* (Princeton, N.J.: Princeton University Press, 1963), pp. 136–37.

[7] The result of this analysis will be published elsewhere.

[8] Specifically, a stratified random sample, six editorials per quarter, four quarters per year, thus insuring a similarity of information flow.

To describe the editorial orientations, the study utilized the *General Inquirer* computerized content-analysis procedures developed by Philip J. Stone and associates.[9] Briefly, these procedures involved the following steps. First, a list of content categories was established, some of which were created to describe changing concern with European and Atlantic unity.[10] The remainder were adapted from the Harvard Third *General Inquirer Dictionary*.[11] The categories were then defined by the construction of a "dictionary"; that is, several hundred frequently used words from the text to be analyzed were entered under one or more of the ninety-nine categories.[12] These lists of words established the operational definitions of the categories.[13]

Matching the dictionary with the full text of the editorials produced profiles of frequency counts on each of these ninety-nine categories for each editorial. Comparison of these frequency profiles across different editorials revealed differential concerns among editorials. To control for the variable length of the editorials, each category count was expressed as a percentage of the total number of words counted in the editorial.[14] This percentage was the basic datum for subsequent statistical analyses.

"Nested one-way analysis of variance"[15] was the statistical model used in the comparison between editorials, grouped by each newspaper, and subsequently by the two categories of newspapers (prestige newspapers and mass papers).[16] The analysis of variance revealed that fifty-seven of the ninety-nine categories discriminated significantly between either prestige and mass newspapers or among the six newspapers themselves.[17]

Since the meaning of each of these fifty-seven discriminating categories is not immediately clear, it seems more enlightening to describe the findings in terms of patterns (or themes) among these categories. To reveal these themes, I used a factor analysis which involved the following steps: first, all discriminating categories[18] were correlated across the 144 editorials, producing a 56×56 correlation matrix. Second, factor analysis of the matrix[19] produced nineteen factors with eigenvalues of more than one, isolating the patterns of associations among the categories. Six of these factors were retained for further inquiry. These six factors together explained 38 per cent of the total variance, so that the six independent content orientations or themes explain slightly more than one-third of the total variation

[9] For an extensive discussion of the procedures of analysis, see Philip J. Stone *et al.*, *op. cit.*, esp. pp. 169–265.

[10] "Namenwirth Political Dictionary" (New Haven, Conn.: Yale University, n.d. [mimeographed]).

[11] Stone *et al.*, *op. cit.*, pp. 170–86.

[12] Most words were categorized in more than one category. For a justification of the system of double and triple classifications, see Stone *et al.*, *op. cit.*, pp. 183 ff.

[13] The choice of categories and the inclusion of words in those categories is a theoretical enterprise based mainly on the concerns of the researcher and various semantic considerations.

[14] To stabilize variances across units of comparison, the percentages were subjected to an arc-sine square root transformation which was used to compute *F*-scores, ω^2, and other statistics.

[15] See George W. Snedecor and W. G. Cochran, *Statistical Methods* (Ames: Iowa State University Press, 1956), pp. 264–68. Also B. J. Winer, *Statistical Principles in Experimental Design* (New York: McGraw-Hill Book Co., 1962), pp. 184–91.

[16] The variance of the newspaper effect is estimated by pooling the variances of the three prestige and three mass papers. Because the two effects are fixed, the pooled variances of the 144 "nested-within editorials" estimate the error variance.

[17] There were far more significant discriminators than would be expected by chance alone. Nonsignificant categories were eliminated in subsequent analyses. For significant discriminators, the amount of variance explained, or ω^2 (William Hays, *Statistics for Psychologists* [New York: Holt, Rinehart & Winston, 1965], p. 407), has also been computed. Computational values of *F* less than one have been set at one; negative values of ω^2 have been set at zero.

[18] With the exception of those with poor distribution, substantive misclassification, or zero correlations with other discriminating categories.

[19] Factor loadings are based on a "principle component" analysis with identity in the diagonal.

in symbol associations. Conversely, two-thirds of this variation is not explained and is best attributed to either measurement error or unique patterns which vary from editorial to editorial. Third, although the first two factors could be readily interpreted, the remaining four factors did not approach simple structure, and the substantive meaning remained difficult to interpret. Consequently, I used a varimax rotation,[20] producing new solutions for the last four factors which did not greatly affect the first two. Table 1 presents the internal rearrangement of the factor structure after rotation. Fourth, for the interpretation of the rotated factor structure, the analysis relied heavily on the available simple structure. In this interpretation, categories with loadings of less than .30 are omitted. Fused positions,[21] however, further substantiated these interpretations. A clinical comparison of editorials with extremely high and low factor scores contributed, in all cases, further evidence which supported the interpretation of the simple structure. Fifth, there is one exception to this rule. None of these procedures provided an intelligible interpretation of Factor V, which, for this reason, was eliminated from further consideration.[22] Consequently, only five factors are retained in the analysis, and the new Factor V is in fact Factor VI. Sixth, since each factor is a scale or composite variable, a score was computed for each editorial on each of the five factors. Subsequently, the grouped editorials were compared using the previously outlined nested analysis-of-variance design for five composite variables.

TABLE 1

PRINCIPAL COMPONENT FACTORS BEFORE AND AFTER ROTATION

Factor	Before Rotation (% of Variance Explained)	After Rotation (% of Variance Explained)
I	9.2	7.2
II	8.0	7.2
III	6.8	7.3
IV	5.3	6.2
V*	4.5	4.7
VI	4.1	5.4
VII*	3.7	....

* Eliminated from further analysis.

It is one thing to describe the mathematics of factor analysis and another to interpret the meaning of factors thus produced for content analysis. In my view, factors represent an interrelated set of thematic concerns, each of them describing a set of problems and issues commented upon by the editorial writers. And the two ends of each factor consist of opposite responses which indicate contrasting orientations toward the underlying problems and issues. In the following sections I shall interpret these issues and orientations and describe the ways prestige and mass papers differ in this regard.

FINDINGS AND INTERPRETATIONS

Factor I: Conflict resolution: Nationalism versus defensive internationalism.—This factor describes an underlying concern with the decreasing power of Great Britain. Two juxtaposed orientations toward this issue appear in the editorials as indicated in Table 2. First of all, the nationalistic orientation holds that the singular pursuit of British national interest will not only preserve British power in the world but also produce solutions to international conflicts which will prove most beneficial for the maintenance of world peace. To preserve peace, it is insisted that any and all encroachments upon British sovereignty must be resisted. Accordingly, nationalistic editorials contain frequent references to matters *British*[23] and to the seat of the

[20] Varimax rotation of identity matrix. Five and seven factor rotations were also tried, but they did not improve simple structure.

[21] Categories with loadings equal to or greater than .30 on more than one factor.

[22] Factor V was probably a residual factor, explaining the least amount of variance of all the factors.

[23] All matters British are categorized by the "Namenwirth Political Dictionary" (a modified

British government, for example, to London as *Political*[24] or *Social Place*.[25] Moreover, the tendency to identify with Great Britain as an actor in the preservation of world order is revealed in the frequent invocation of the category *Selves*[26] and symbols, such as "we," "us," "our," and "ourselves." Finally, references to the category *American*[27] express a persistent fear of American political encroachments upon the freedom of British action (e.g., the insistence of the United States on the integration of military strategy and hardware as indicated by the category *Technological*).[28]

To highlight this orientation, let me quote in full the short editorial, "Peacemaker: Good News from Moscow" (*Daily Express*, April 25, 1963).[29]

The visit of the British and American ambassadors to Mr. Khrushchev may lead to a summit meeting on nuclear disarmament. How is Britain able to take this initiative along with America? Because she is strong. Because like Russia and the U.S. she is armed with the H-bomb. But if Britain alone abandons her nuclear defenses she will no longer be able to work effectively for peace between East and West. Those who urge Britain to keep the bomb and her independence are not only realists, they are the true pacifists.

The internationalistic approach, on the other hand, holds that Great Britain must assume an active role in the maintenance of world peace. Enjoying a long history of diplomatic skill and intervention in international affairs, Great Britain must continue to act as mediator in present-day power struggles, a function commensurate with her power and her resources. In this way Britain has a role to play in the transformation of the world order, and only by seizing this responsibility can she maintain and strengthen her prestige in the world. This approach stresses international co-operation, co-ordination, and conciliation, as indicated by frequent references to the categories *Sign Accept*[30] and *Approach*.[31]

TABLE 2

FACTOR I: CONFLICT RESOLUTION

Categories	Loadings
Nationalism:	
British	.60
Social Place	.43
Selves	.35
American	.31
Technological	.30
Political	.30
Internationalism:	
Understate	−.60
Overstate	−.60
Time Reference	−.58
Academic	−.48
Attempt	−.45
Thought Form	−.44
Artistic	−.40
Sign Accept	−.34
Approach	−.33
Sensory Reference	−.32

At the same time, the commentators of the internationalistic persuasion recognize the fact that much time (*Time Reference*)[32] and effort (*Attempt*)[33] are required to accomplish greater accord among

version of the Harvard Third Dictionary) in the category "British."

[24] The category *Political* is the category of words specifying the political context of rules and actions (Stone *et al.*, *op. cit.*, p. 176).

[25] The category *Social Place* includes references to buildings and building parts, "political, social, and economic locations" (*ibid.*, p. 174).

[26] The category *Selves* includes all pronoun references to the inclusive self (*ibid.*, p. 174).

[27] The category *American*, according to the "Namenwirth Political Dictionary," includes all references to matters American.

[28] The category *Technological* includes such words as "job, ability, engineer, hunter, print" (Stone *et al.*, *op. cit.*, p. 176).

[29] Spelling has been Americanized, as have all quotes from editorials, to match existing American dictionary spellings.

[30] The category *Sign Accept* includes "words implying interpersonal acceptance" (Stone *et al.*, *op. cit.*, p. 176).

[31] In the "Namenwirth Political Dictionary," the category *Approach* consists of words which "indicate conciliatory feelings and moods."

[32] The category *Time Reference* includes "all references to measurement of time" (Stone *et al.*, *op. cit.*, p. 175).

[33] The category *Attempt* includes "goal-directed activities implying effort" (*ibid.*, p. 175).

nations. Related to this is the continuing wariness that an internationalistic approach will limit British independence and freedom of action. As a result, an element of tentativeness pervades the internationalistic position. This tentativeness contrasts sharply with the unequivocal nationalistic approach and is expressed by the use of the categories *Academic*[34] and *Thought Form*,[35] which betrays the uncertain and abstract level of discussion in internationalistic editorials. Even more than tentative, the tone of such editorials is defensive, as revealed by the co-occurring references to *Overstate*[36] and *Understate*,[37] which from other research is known to indicate defensiveness, if not repression, usually of aggressive drives.[38] This last generalization flavors the interpretation of the factor because the nationalistic orientation stresses recourse to instruments of war for the resolution of international conflicts, while the internationalistic position represses such aggressive tendencies and ostentatious display of patriotic feelings.

These qualities of the internationalistic approach are well illustrated in the following editorial (*Manchester Guardian*, January 23, 1963):

"Two Within Six"

An end to centuries' old rivalry, such as the Franco-German treaty signed yesterday, ought to be welcomed without restraint. . . . It is a noble achievement. A shadow, however, falls across the scene. . . . It must now be asked whether the Franco-German alliance is meant to be something beyond a warm and friendly burying of old hatreds. Is it meant to become the dominating force within the whole of Western Europe? . . . The anxiety is lest this (French-led) Europe is to be a closed community, looking in on itself and its own interests alone.

In such a Europe lies the seeds of a future war. Not only will the West as a whole be wrecked by the division and withering of the Atlantic Alliance but at the same time Western Europe's exclusiveness may bring it into conflict instead of cooperation with groups in Africa, Asia, and perhaps Eastern Europe. . . . If gradually it (Alliance) merges into a more liberal community, with others sharing in the leadership, it can be healthy. . . .

TABLE 3

NEWSPAPER SCORES ON FACTOR I

PRESTIGE NEWSPAPERS				MASS NEWSPAPERS			
Times	*Telegraph*	*Guardian*	$\bar{x}$	*Mirror*	*Express*	*Mail*	$\bar{x}$
−0.58	−0.40	−0.34	−0.44	−0.26	+1.30	+0.28	+0.44

	F	d.f.	*P*	ω^2(%)
Prestige vs. mass newspapers	45.56	1,138	<.001	19.0
Six newspapers	12.57	4,138	<.005	19.7

NOTE: + = nationalism; − = internationalism.

[34] The category *Academic* specifies "the academic context of the discussed rules and actions," and in the editorials its use is indicative of the abstract level of the argument (*ibid.*, p. 175).

[35] The category *Thought Form* in general refers to "units and styles of reasoning" (*ibid.*, p. 174).

[36] The category *Overstate* consists of "emphatic or exaggerated words," usually adjectives or adverbs modifying nouns and verbs or objects and processes of concern (*ibid.*, p. 176).

[37] The category *Understate* includes "words which are often adjectives or adverbs connoting doubt or uncertainty" (*ibid.*, p. 176).

[38] For instance, Dexter C. Dunphy, "Social Change in Self Analytic Groups," in Stone *et al.*, *op. cit.*, pp. 310–11.

The two orientations toward British power in a changing world are not equally represented in mass and prestige papers. In general, prestige papers are more internationally oriented, while mass papers are more nationalistic. The average differences, however, hide the large and significant differences in this regard among the six newspapers, especially among the mass papers (see Table 3).

Factor II: World issues: European integration versus the Cold War.—Editorial writers are always confronted with a great variety of issues, news items, and so forth, and they must first classify them, either implicitly or explicitly, before deciding whether to spend time and space on one or another group of events. Factor II reveals that in this process a distinction is made between divisive economic issues in the European environment and largely military problems in the confrontation between East and West.[39] It thus appears that these editorials deal either with agricultural and other economic problems of European integration, or with the military problems of the Cold War.[40]

Editorials concerned with the problems of European integration are found to contain many references to matters *European:*[41] European national and supranational—*International Institutions,*[42] their personnel, European politicians, and other European symbols (see Table 4). They also include many references to symbols of *Unification,*[43] as indicated by words such as "integration," "co-operation," "unification," "unity," and "supranational."

France, and especially de Gaulle and Gaullism, are the major concern with matters *French;*[44] they are seen as retarding the process of integration by imposing tariffs and generally favoring controlling, planning, and regulating *Control,*[45] and agricultural development is *Work*[46] in the European communities. Belgium, the Netherlands, and Italy are rarely mentioned (*Italy-Benelux*),[47] but if mentioned at all, it is in the context of European unification. An example of this concern with European integration is found in the following editorial (*Daily Telegraph,* November 6, 1963):

TABLE 4

FACTOR II: WORLD ISSUES

Categories	Loadings
Integration:	
Europe	.85
International Institutions	.77
French	.43
Unification	.43
Work	.38
Control	.31
Italy-Benelux	.30
Cold War:	
Soviet	−.77
Approach	−.55
American	−.49
Atlantic	−.42
Not	−.34

"Doctor Mansholt's Plan"

How France, or Germany for that matter, will react towards the agricultural trading arrangements now being proposed . . . is hard to forecast. But there is no doubt that these proposals would help greatly towards the kind of European economic rapprochment which every sensible Briton has desired since the Brussels

[39] Cf. Namenwirth and Brewer, *op. cit.*, p. 425.

[40] *Ibid.*; also J. Z. Namenwirth and H. D. Lasswell, *Changing Language in American Party Platforms: A Computer Analysis of Political Values* (forthcoming).

[41] This category in the "Namenwirth Political Dictionary" contains references to all matters European, including institutions and politicians.

[42] A list of words in the "Namenwirth Political Dictionary" naming those institutions and offices.

[43] A category in the "Namenwirth Political Dictionary" listing words connoting the process of unification.

[44] A category in the "Namenwirth Political Dictionary" listing all matters French.

[45] A category of words indicating a limitation of action of a social-emotional kind (Stone *et al., op. cit.*, p. 175).

[46] A category of words listing personal task activities (*ibid.*, p. 175).

[47] A list of words in the "Namenwirth Political Dictionary" naming these countries, their cities, provinces, institutions, and political leaders.

breakdown. . . . First, the six are to be drawn together in a common agricultural price system. Then the six, Britain, Denmark, the Commonwealth, and others, are to be drawn together in an organized agricultural market. . . . But in judging the details as they emerge, two things must be borne in mind. One is that agricultural products are going to be a managed market, whether we like it or not. . . . The other is that it is worthwhile putting up with a good deal of irritating detail for the sake of getting the six, the rest of Europe, and the Commonwealth a real agricultural community. . . .

A second group of editorials (negative Factor II) centers its discussions around issues relating to the Cold War, as indicated by the large number of references to *Soviet*,[48] *American*,[49] and *Atlantic*[50] problems. Such editorials contain many references to national politicians and symbols, especially Russian ones. The high concern with matters Russian is usually placed in a bipolar confrontation between the United States (and matters *American*) and its allies (*Atlantic*) on the one hand, and Russia and her allies on the other.

Note, furthermore, that the category *Not*[51] is often used in this context to deny either base intentions or bad consequences of the Russian-American détente. Indeed, in 1963 these concerns are discussed in a conciliatory manner, mentioning the test-ban treaty and the East-West détente in general *Approach*.[52]

The following editorial (*Daily Express*, August 23, 1963) rather brutally represents this concern with Cold War issues from a nationalist point of view:[53]

TABLE 5

NEWSPAPER SCORES ON FACTOR II

Prestige Newspapers				Mass Newspapers			
Times	*Telegraph*	*Guardian*	$\bar{x}$	*Mirror*	*Express*	*Mail*	$\bar{x}$
+0.19	+0.42	+0.34	+0.32	−0.79	+0.27	−0.44	−0.32

	F	d.f.	P	$\omega^2(\%)$
Prestige vs. mass newspapers	17.42	1,138	<.001	9.4
Six newspapers	4.43	4,138	<.005	7.9

NOTE: + = integration; − = Cold War.

"Account Settled"

The Germans say that they fear their interests will not be considered while accounts are settled between Russia and the West. The Germans are right. Accounts with them were settled by blood and tears between 1939 and 1945. There is no disposition in Britain to consider German interests at all.

[48] A "Namenwirth Political Dictionary" category listing all matters Russian and Communist.

[49] The category *American* occurs on both Factors I and II; but, whereas on Factor I it indicated a juxtaposition to British national interests, on Factor II it describes the conflict between Russia and the United States.

[50] "Namenwirth Political Dictionary" category listing the names of Atlantic institutions, offices, and their personnel.

[51] A category of "words denoting negation" (Stone *et al.*, *op. cit.*, p. 175).

[52] The category *Approach* loaded on both Factors I and II; but, whereas on Factor I it indicates a concern with a more universalistic inclusion of all nations toward a world community, on Factor II it is specifically related to the Cold War.

[53] Although this particular editorial is characterized by the nationalist orientation (positive Factor I), there is an equal number of editorials which demonstrate an international orientation (negative Factor I) toward European issues.

Table 5 indicates that the prestige newspapers are generally more concerned with European issues, while the mass papers are more concerned with problems of the Cold War. These differences are somewhat greater than the pooled differences among the newspapers.

Regarding differences among the papers, the *Times* (of London) is the most deviant prestige paper (i.e., the most deviant from the mean of all prestige papers) and far more concerned with Cold War issues. The *Daily Mail* is the most deviant mass newspaper.

Factor III: Issue participants: Collective versus individual.—Surveying events in the world, commentators must inevitably ask questions, not only about the nature of the events and their geographic location, but also about their chief protagonists. Factor III (see Table 6) indicates that editorial writers distinguish between two classes of actors: political leaders as individuals and collectivities (such as nations, governments, peoples, and parties). These two types of protagonists are rarely mentioned together in one editorial. In general, the stressing of either personalities or collectivities is most likely a reflection of the author's preference for one of two opposing philosophies of history—the collective approach or the biographical approach.

The personal, biographical orientation of the factor (negative pole) is better defined than the opposite, positive end. In the "personal" view political conflicts are caused by clashing personalities. Accordingly, in this view a change in leadership will bring about a change in political conflicts.[54]

Individual-oriented editorials are thus characterized by frequent references to the categories *Political* (see n. 24), *Job Role,*[55] *Higher Status,*[56] and *Male Role,*[57] indicating a concern with political leaders and their pronouncements and actions. French and German actors are often discussed, as indicated by frequent references to Dr. Adenauer and Dr. Erhard (defined under category *Medical*[58] because of the title Dr.), but at times MacMillan and Kennedy are also mentioned. This kind of editorial commentary uses indirect quotes, saying that so and so *Sense*(s),[59] views, regards, or recognizes. This orientation, which pervades all editorials with a high negative factor score, is well represented in

TABLE 6

FACTOR III: ISSUE PARTICIPANTS

Categories	Loadings
Collective:	
Collective-Dynamic	.38
Message Form	.33
Individual:	
Higher Status	−.91
Job Role	−.91
Male Role	−.72
French	−.53
Political	−.50
German	−.46
Sense	−.45
Medical	−.39
Unification	−.32

the following untitled editorial (*Daily Mirror,* October 16, 1963):

Now that Dr. Adenaeur has departed, will West Germany wear a new look in European and world affairs? . . . Is it likely that the new chancellor, Dr. Erhard, will break fresh grounds? . . . West Germany has now got the equivalent of MacMillan. . . . Mr. Adenauer ruled for fourteen long years. His achievements were many. But he did not seek change. . . . His (Erhard's) election will no doubt bring a

[54] The category *Unification* has loadings on both Factors II and III. Whereas on Factor II it pertains to unification in the general European context, on Factor III it pertains especially to the increasing co-ordination of French and German politics.

[55] The category or words describing "all roles with clear occupational reference, theoretically open to both sexes" (Stone *et al., op. cit.,* p. 174).

[56] The category of "all words with higher status connotations" (*ibid.,* p. 176).

[57] The category of "all roles with specific male references" (*ibid.,* p. 174).

[58] Category of words classifying the medical context of rules and actions (*ibid.,* p. 176).

[59] A list of words describing perception and awareness (*ibid.,* p. 175).

breath of fresh air. . . . For instance, he will do what he can to foster closer British contacts with the Common Market. He will not be so dogmatic about negotiations with the Russians. . . . Of course, Dr. Erhard will be just as keen on Allied and European unity as Dr. Adenauer. He will essentially be carrying on the same policy. . . . Dr. Erhard is of the generation of MacMillan. Nobody knows who will follow them. Similarly, nobody knows who will be representative of West Germany in the latter part of the 1960's.

The opposite, positive end of Factor III is not defined by categories with high loadings, and its interpretation is therefore conjecture. There are two possibilities: if the negative pole represents a personal orientation, then the positive pole should represent either the absence of a personal orientation or the opposite of a personal orientation, that is, an impersonal orientation.

An investigation of the two categories with positive loadings makes the second interpretation more likely. The category *Collective-Dynamic*[60] includes in this case such words as "plans," "planners," "planning," "producers," "reform," and "program"; and the category *Message Form*[61] contains such widely divergent words as "money," "insurance," "treaty," "tariff," and "proposal." A concern with these two groups of words indicates attention to collectivities and to abstract, rather than personal, processes. These impersonal processes and collectivities are probably held responsible for political conflict and clashes. This interpretation is further confirmed by reading editorials with high positive factor scores. For example:

"The Power That We Have"[62]

Parliament should have just one aim when it debates the government's White Paper on defense to make Britain strong and independent. The Labor Party has a special responsibility in defense matters. It has rightly proclaimed its belief in an independent British foreign policy. But by some strange logic it spurns an independent deterrent. . . .

A great deal of nonsense is talked about the "unbearable" cost of maintaining nuclear forces. The present British deterrent costs less than one percent of the national income, less than is spent for drink and tobacco over the Christmas period. Less than one-quarter of what is spent every year on gambling.

As an insurance for peace, as a bulwark of British diplomacy which provides us with the great equalizer in power politics, the nuclear expenditure is justified a thousand-fold. Let all parties accept this fact as fundamental to Britain's security. Let them prod, cajole, and hammer the government until it produces the British weapons that match the nation's needs.

TABLE 7

NEWSPAPER SCORES ON FACTOR III

PRESTIGE NEWSPAPERS				MASS NEWSPAPERS			
Times	*Telegraph*	*Guardian*	$\bar{x}$	*Mirror*	*Express*	*Mail*	$\bar{x}$
+0.25	−0.08	−0.21	....	−0.33	+0.43	−0.06	....

	F	d.f.	*P*	ω^2(%)
Prestige vs. mass newspapers	1	1,138	N.S.	0
Six newspapers	2.55	4,138	.05	4.2

NOTE: + = collective; − = individual.

[60] A category of the "Namenwirth Political Dictionary."

[61] This category includes the names of communication media, in a broad sense, including art objects and money (Stone *et al.*, *op. cit.*, p. 174).

[62] This editorial has also a high positive factor score on Factors I, II, and IV and a high negative factor score on Factor V.

And the peoples' will [*Daily Express*, February 21, 1963].

There is no statistically significant difference between prestige and mass papers on this factor. And there is only a small difference among the six newspapers ($\omega^2 = 4.2\%$) (see Table 7). Therefore, this set of themes distinguishes between editorials, and this distinction is largely irrespective of the divergent characteristics of newspapers in which such editorials appear.

Factor IV: Policy justification: Ideological versus quasi-factual.—Editorial writers do not merely identify and classify actors in the political arena; they also support or criticize their actions and policies. Factor IV (see Table 8) describes two different modes used in the defense of policy advocacy. Ideological justification evaluates policies in terms of more general and abstract moral principles, while quasi-factual justification builds its case on selected data derived from national statistics, opinion polls, and other sources of information. And, as is the case with all factors, the choice of either mode of policy justification is, on the average, unrelated to the editorial characteristics encompassed by the first three factors. In the final analysis, the approach of ideological justification shares with all idealism a denial of the problems of means and their efficacy in realizing abstract principles. The quasi-factual approach, on the other hand, shares with empirical positivism a denial of the moral problem of goal selection.

Editorials which express ideological justification are characterized by a high concern with the categories *Ideal Value*[63] and *Action Norm*.[64] The ideological aspects of this approach are further revealed by the categories *Sign Accept* (words implying interpersonal acceptance), *Thought Form*,[65] and *Attempt* (goal-directed activity implying effort). In addition, the categories *Dynamic*,[66] *Individual Dynamic*,[67] and *Collective Static*[68] indicate a concern with maintaining or changing the status quo, that is, current policy. The following editorial (*Daily Express*, July 29, 1963) illustrates the ideological approach to justification, defending policies in this case in terms of the principle that might is right:

TABLE 8

FACTOR IV: POLICY JUSTIFICATION

Categories	Loadings
Ideological:	
Ideal Value..........	.71
Individual Dynamic..	.63
Action Norm........	.56
Collective Static.....	.47
Attempt............	.41
Sign Accept.........	.35
Technological........	.34
Thought Form.......	.30
Dynamic...........	.30
Quasi-factual:	
German............	−.39
Social Place.........	−.38
Think..............	−.31

"The Grand Design for Peace"[69]

"By what right has the British government taken a lead in these mighty affairs?" said Mr. MacMillan. "By what authority have we proposed these conferences to secure a test-ban? . . . It is by the right and authority of our nuclear power that we lead test-ban talks," said Mr. MacMillan. So speaks Mr. Harold MacMillan. At the moment of his triumph in the cause of peace he pays tribute to the policy of strength which made the test-ban possible.

The Prime Minister knows, from experience,

[63] This category includes "culturally defined virtues, goals, valued conditions and activities" (Stone *et al.*, *op. cit.*, p. 174).

[64] This category defines "normative patterns of social behavior" (*ibid.*, p. 174).

[65] A list of words indicating concern with units and styles of reasoning (*ibid.*, p. 174).

[66] A "Namenwirth Political Dictionary" category of words indicating concern with changing the status quo.

[67] A list of words indicating a concern with the change of the status quo with the individual as agent or object of preferred actions.

[68] A category or words indicating a defense of the status quo with the collectivity as agent or object of such actions.

[69] This editorial has also a high positive factor score on Factor I and high negative factor scores on Factors II, III, and V.

that in the hard world of international politics good will is not enough. No negotiator, however skilled or dedicated, can achieve anything without power to back his words. Mr. MacMillan has buttressed his patient diplomacy with the nuclear deterrent. He has allied strength with wisdom in the search for peace. It is a design which commands the respect of other nations. And the wholehearted support of the British people.

The factor structure poorly defines the alternative orientation. The interpretation of the factors is consequently based largely on conjecture and a close reading of editorials with high negative factor scores. Editorials reflecting quasi-factual justification attempt to persuade the reader of the advantage of a preferred policy, orientation, or action, by using factual documentation in the advocacy of moral preferences.[70] And, in contrast with the general and abstract ideological approach, the quasi-factual strategy is far more specific, describing and naming governments, their policies, and representatives to be rejected.[71] Finally, the *German*[72] government, nation, and people especially bear the brunt of hostile feelings and critical appraisal in the British press. Be all this as it may, the quasi-factual approach and its various characteristics are well illustrated in the following editorial (*Daily Express*, December 18, 1963):

"What They Think"[73]

Bad, bad news for Common Market enthusiasts comes from a *Reader's Digest* survey. In Britain only 37 percent of those questioned by the survey thought they would benefit if Britain were a member of the Common Market. In the Common Market, only one in five feels better off than five years ago. And only two out of every five feel that their country has benefited. Those who expected to gain from the Common Market are bitterly disillusioned. Those who have escaped from it can rejoice.

TABLE 9

NEWSPAPER SCORES ON FACTOR IV

Prestige Newspapers				Mass Newspapers			
Times	*Telegraph*	*Guardian*	$\bar{x}$	*Mirror*	*Express*	*Mail*	$\bar{x}$
....	....	....	+0.18	....	....	....	−0.18

	F	d.f.	*P*	ω^2(%)
Prestige vs. mass newspapers	4.53	1,138	.05	2.4
Six newspapers	1	4,138	N.S.	0

NOTE: + = ideological; − = quasi-factual.

Notwithstanding the common assumption that the elite press is less ideological than the mass press, the prestige newspapers in this analysis tend toward ideological justification, while the mass newspapers tend toward quasi-factual defense. The differences among the six newspapers, however, are not significant (see Table 9).

Factor V: News interpretation: Casual inquiry versus teleological explanation.—

[70] Interestingly enough, quasi-factual justification is more often used in rejecting governmental policies, while ideological justification is more often invoked in support of such policies.

[71] The category *Social Place* (see n. 25) refers predominantly to capital cities as a collective label for foreign and domestic governments in political commentary.

[72] A "Namenwirth Political Dictionary" category of words describing German politicians, organizations, places, etc.

[73] This editorial has also a high negative factor score on Factors I and V and high positive scores on Factors II and III.

The final factor represents an issue inherent in all editorializing. It poses the question of how the news relates to a more general understanding of the past, present, and future. As Table 10 reveals, two distinct and exclusive modes of news interpretation resolve this question in the editorials studied. One mode seeks the central organization of events in the psychological motivations and intentions of the actors, the other in probable or possible political consequences.

The causal orientation is open-ended. It considers a wide array of events to formulate possible explanations, and the presentation of events is thus organized into subsequent hypotheses which could account for the news. It is this predilection for open-ended inquiry which gives a tentative flavor to the editorial commentary. The frequent use of the category *Nonspecific Object*[74] is often an indication of defensive prose, as are the frequent co-occurrences of the category *Not*.[75] Denials are used to develop an interpretation, and in the process of argumentation subsequent hypotheses are often rejected. To illustrate, consider the sentence: "It is not this; it is not that; what [also *Nonspecific Object*] is it?"

A further intriguing feature of this open-ended style of reasoning is the use of physical metaphors for psychological events and motivations. The use of such metaphors places the sequence of described events in a natural, if not mechanistic, context which, in turn, gives prestige to the offered explanations. It is this feature which explains the co-occurrence of categories such as *Natural World*[76] (political weather, political climate, political storms), *Technological*[77] (bridges to understanding and peace, engineering solutions, ship of state), *Work*[78] (to exert influence, to impress, to save or gain prestige, to work out problems), and *Move*[79] (falling expectations, a turn of events). The central qualities of this type of interpretation appear very clearly in the

TABLE 10

FACTOR V: NEWS INTERPRETATION

Categories	Loadings
Causal inquiry:	
Nonspecific Object	.61
Natural World	.59
Technological	.47
Female Theme	.46
Move	.44
Possess	.40
Work	.40
Collective-Dynamic	.38
Recreational	.35
Not	.34
Teleological explanation:	
Political	−.29
Follow	−.26

following editorial (London *Times*, June 21, 1963):

"The President's Trip"

Unless he catches a diplomatic cold, President Kennedy will arrive in Germany on Sunday. . . . That his trip to Germany is ill-timed is now sufficiently obvious. He is at the thick of a social crisis at home. Dr. Adenauer is about to retire; there is controversy about Mr. MacMillan's future; Rome will barely have time to produce a prime minister and a pope; General de Gaulle is in isolation. . . . That said, grumbling should now cease and the best be made of a difficult job. Atlantic relations always provide work to be done, and in spite of everything there is something to be said for the President's decision not to put it off. . . . One of these is that, whereas the United States tends to regard the new relationship as an opportunity to share the burdens of the Cold War, Europe suspects America of merely trying to solve her balance of payments problems without giving up anything that really matters, such as her virtual monopoly of nuclear power. And there is some truth in both sides. There is a need for a bridge between them. . . . What

[74] List of words of "abstract references to objects connoting intellectualization" (Stone *et al.*, *op. cit.*, pp. 174, 179).

[75] Words denoting negation (*ibid.*, p. 175).

[76] A category of geographical places, weather references, and cosmic objects (*ibid.*, p. 174).

[77] A list of words specifying the technological context of rules and actions (*ibid.*, p. 176).

[78] A category of words denoting "task activity" (*ibid.*, p. 175).

[79] A category of words describing physical motion (*ibid.*, p. 182).

President Kennedy needs to save him from the trip as a whole is evidence that an American is not as unwelcome in Europe as President de Gaulle has tried to make out. . . . There is therefore something to be gained from the unfortunate timing of the trip.

The opposite orientation is poorly defined in the factor-analytic results. Only two categories have negative loadings at all, and these loadings are low (*Political* = —.29 [see n. 24]; *Follow*[80] = —.26). I surmised, therefore, that this response is less restrained and tentative, indicating a closed- rather than open-ended approach to interpretation and an absence or rejection of metaphorical and mechanistic allusions to social reality. A close reading of the pertinent editorials further specifies the underlying continuum. The editorials characterized by the negative pole of the factor also attempt to organize the news and events into a meaningful scheme, but in this view the approach is teleological rather than causal.

The teleological approach prefers organicistic interpretations, a fact which explains the absence of physical metaphors. It is not surprising that this approach is the opposite of open-ended and tentative because the proponents of the teleological approach believe that the present carries within it a design which will unfold during history, and this design is not described as a result of open-ended inquiry but presumed to be there before the data are at hand. In the editorials, this design is seen as largely and narrowly political, and the universe of editorial discourse is therefore equally restricted to political matters only. According to the teleological approach, the task of the editorial writer is to enlighten readers of the news by pointing out future political consequences of news events. This task, however, is not limited merely to pointing out consequences because the manifestation of the design still depends on active human intervention. Consequently, commentators will exhort and urge decision makers to bring the former consequences about. And, unless the decision makers so decide, there is no possible meaningful interpretation of the news, as there is no meaning to the news. This approach to news interpretation is well illustrated in the following editorial (*Daily Express*, January 14, 1963):

"Crumbling Hopes in Brussels"[81]

Great is the dismay in the Common Market camp. Negotiations between Britain and the Six may well be on the verge of collapse. Baffled and bewildered, the Common Market supporters see their case crumble. Now they know that there will be no concessions for British agriculture, no opening for cheap Commonwealth food in the market, no protection for the British housewife against soaring prices.

Desperately, the marketeers cry out to the government, "Sign," "sign the treaty of Rome and then bargain for better terms." A vain hope. Once in, Britain would have to conform to the rules of the Market. There would be no bargains, only obedience. The government must realize that the Common Market gamble has failed. There is no dishonor in facing the truth. There would be lasting shame in maintaining the pretense. It is time to end the sad farce at Brussels.

As Table 11 underlines, prestige newspapers rely far more on causal inquiry in the interpretation of the news, while the mass newspapers rely far more on teleological explanation.

CONFIRMATION

The orientations which discriminate between prestige and mass papers were described by a variety of quantitative procedures. But these statistical manipulations describe the findings only partially, a good part of the analysis was based on interpretation, and the inspection of both clusters and prose was characterized by the absence or presence of these clusters. Inevitably, there must arise questions about the stability and validity of these latter interpretations; do these interpretations really characterize all editorials? To further con-

[80] List of words indicating submissive action (*ibid.*, p. 175).

[81] It will be noted that this editorial preceded the French veto to British entry in the Common Market.

firm these interpretations, all editorials were classified in three percentile groupings (0–33⅓ percentile, 33⅓–66⅔ percentile, and 66⅔–100 percentile), eliminating all editorials falling in the middle percentile grouping on any of the five factors.[82] The remaining twenty-three editorials, therefore, have very high positive or negative factor scores on each of the five factors, and the content of these editorials should be well predicted by the factor interpretations. Checking each of the twenty-three predictions with the actual prose of the editorials revealed a very close match. Ideally, one would include the full text of these twenty-three editorials so that the reader could compare this subjective judgment with his own. For reasons of space, however, I include here only the full text of three randomly selected editorials.[83] The factor interpretation predicts that the first of these (November 29, 1963) deals with European issues (Factor II, positive) in a nationalist rather than internationalist manner (Factor I, negative), mentions collectivities as issue participants (Factor III, positive), uses teleological explanation (Factor V, negative) to interpret the news, and uses quasi-factual justification to defend its policy preferences (Factor IV, negative). The text of this editorial is as follows:

"Britain's Good Fortune"

The Common Market Parliament approves a plan to fix the price of wheat at 36 pounds a ton. The price of wheat here is 23 pounds a ton. So if we had joined the Common Market we would have been forced to pay 13 pounds a ton more for our wheat. As a result, the price of bread would have risen considerably. Even more important, animal feeding stuffs, which include wheat, would have become much more expensive. The price of pork, poultry, and eggs would have soared. Inflation which now grips Continental countries, would have crossed the Channel. Every day demonstrates how fortunate we are to be outside Europe, to be spared the troubles of the sorrowful Six.

TABLE 11

NEWSPAPER SCORES ON FACTOR V

Prestige Newspapers				Mass Newspapers			
Times	*Telegraph*	*Guardian*	$\bar{x}$	*Mirror*	*Express*	*Mail*	$\bar{x}$
+1.26	+0.15	−0.13	+0.42	−0.58	−0.89	+0.20	−0.42
				F	d.f.	P	ω^2(%)
Prestige vs. mass newspapers				46.16	1,138	.001	17.5
Six newspapers				18.35	4,138	.001	26.8

NOTE: + = causal inquiry; − = teleological explanation.

The two other randomly chosen editorials (*Daily Express*, June 21, 1963, Factor I, positive, and Factors II, III, IV, and V, negative; and *Daily Mirror*, June 27, 1963, Factors I, II, and V, negative, and Factors III and IV, positive) also confirm this interpretation, as the reader can ascertain for himself.

"All Mixed Up"

Mr. Dean Rusk, the U.S.A. Secretary of State, is to come to Britain next week to persuade this country to join the NATO mixed-

[82] There were thirty-two (2^5) possible combinations. Of these, seventeen were represented in the sample. Of the 144 editorials only twenty-three fitted the criteria of selection. In some cases, more than one editorial fitted the same combination of factor orientations.

[83] Randomly selecting a table of random numbers, a column of numbers therein, and the number of selected editorials.

manned Polaris fleet. He will not find much support here. The idea is senseless, militarily and dangerous politically, as well as being a prodigious waste of public money. The only ones to benefit would be the Germans, who would get their trigger finger on nuclear weapons. Mr. Rusk should be told firmly that in Britain's view the whole mixed up notion should be scuttled.

[Untitled]

The British and American governments have for years pinned their hopes on an agreement of some kind with the Russians on a nuclear test ban pact.

The hopes, which are not pitched high, will once again be put to the test next month—this time in Moscow and not Geneva. One popular misconception about the test ban negotiations has always been that a purely scientific approach would lead to success. This view was repeated yesterday in the House of Lords.

Lord Henderson went so far as to say that it was significant that Lord Hailsham, Minister of Science, had been picked to lead the British delegation. Some people feel that Lord Hailsham is not a suitable Minister to face a Russian delegation in this kind of negotiation, where patience and equanimity are all-important.

The essence of the test ban problem is not scientific but political. Will the Russians cooperate in an international inspection system which must inevitably modify the well-known preoccupation with secrecy? What is vital is that a test ban agreement should provide the absolute assurance that everybody is abiding by its terms.

Britain and America have always been ready to provide such an assurance. The political question is—will the Russians do the same? Unfortunately there are no signs that they will.

Scientific advances since the test ban talks began have now nearly defeated their original objective. Originally scientific devices were not good enough to detect underground explosions which might, or might not, be produced by nuclear testing. But now devices have been developed that can even detect quarry-blasting in central Russia.

What but an international inspection team on the spot is ever going to be able to say what was a quarry blast or a test? We are really back to the problem of finding a political solution.

EXPLANATION

What remains to be done is to formulate an explanation of these questions: why do editorials display this particular set of orientations, and why do prestige papers differ in this regard from mass papers? Clearly, my answer to this question is an explanation which is after-the-fact and speculative.

The exigencies of news reporting offer a unifying principle explaining the findings. In the pursuit of meaning, the editorial writer is constrained by the mechanisms of information processing, and these mechanisms define the intelligibility of editorial comment. News events occur constantly and everywhere. Only a few events are even considered news. The editorial commentator must select the news events, guided by implicit principles which are buried in the very nature of language, precedent, and custom. Implicitly, he must ask the question: Is it important? The criteria of importance are part of an established classification scheme.

The present investigation indicates at least five criteria of relevance: (1) Can the event be construed as affecting the power position of the paper's country (Factor I)? (2) Is it part of the military conflicts between the two major powers, or does it pertain to the economic disputes of the western European states (Factor II)? (3) Is the conflict caused or to be resolved by individuals or by anonymous collectivities (Factor III)? (4) Does the event require a response, and how shall this response be justified (Factor IV)? (5) How does the news event relate to what is known about its causes and consequences (Factor V)? Therefore, if any event is not pertinent to these five questions, it would not be found in the editorials.

Speculating along these lines, I believe that the five orientations are specifications of more abstract and general principles of

information processing for a particular time and place. For instance, the editorial responses to events affecting the power status of Great Britain are a specification of the universal problem of what to do in the face of forces which affect one's power position. Assuming the universal desirability of power, shall one aggressively respond or repress this aggressive response in search of non-violent solutions (Factor I)?

In the same vein, the juxtaposition of European economic disputes with Cold War military conflicts (Factor II) is not merely a classificatory distinction which is predicated by specific events in a specific time and place. A similar juxtaposition was found in other places.[84] In classifying particular events a far more universal principle is used, one which attempts to distinguish between two approaches to controlling the hostile environment: farming versus hunting, the prototypes of present-day economic versus military responses.

If newsmakers create the world of man, the meaning of the world depends on a search for those who make the news, are responsible for the news, and who will determine future developments (Factor III). Before modern secularization there were two classes of actors: men and the gods. In a secularized society, however, it takes the utmost tragedy before the gods are named as causes of events. Instead, the erstwhile dichotomy has been replaced by the humanist predilection for a particular individual and the collectivist predilection for anonymous forces and entities—they, the masses, the party, and other collectivities. The distinction made in the editorials between two classes of actors in the European and Atlantic arenas is but a specification of this more universal tendency in information processing.

To account intelligently for that which is emergent, by distinguishing it from that which is merely transient, there are two lines of argument (Factor V). According to the causal approach, the news is real because it has been caused in the past by universal mechanisms so that its present manifestation is only another replication of universal principles. On the other hand, the teleological view argues that the news is real because it is a necessary chain in a future development which is part and parcel of the nature and history of things.

Justification of policy preferences requires arguments of a different order, which are reduced to the two following contrasts (Factor IV). One kind of argument (quasi-factual) states that "everybody does it," or "all important people prefer it." Such arguments have one thing in common; they do not measure the chosen means against some absolute standard; they are amoral. Furthermore, they create an aura of objectivity by using scientific or quasi-scientific claims to persuade an audience of the rightness of a particular preference. The opposite argument proceeds from the belief that there is one right solution and one solution only. Furthermore, the rightness is predicated by one or more absolute and universal criteria. It would be argued, for instance, that "might is right," or "this is the way it should be." This line of argument and the objectivity of its criteria of preference are not derived from empirical observation but from immanent or transcendental knowledge of one sort or another.

The next question is why prestige papers and elites in general would take one stand in regard to these orientations, and the populace another. It is my contention that the differences found in newspaper editorials are marks of distinction which have contrasted elites and masses since time immemorial. And, no doubt, the very social processes which create and maintain social structure and inequality equally sustain these divergent cognitive orientations. But it is not feasible to document or even argue for the validity of this explanation in this article. To illustrate the factual point, let us consider for a moment those two timeless representatives of the elite versus mass contrast as pursued to its ab-

[84] See Namenwirth and Brewer, *op. cit.*; also Namenwirth and Lasswell, *op. cit.*

surd limits: Don Quixote and Sancho Panza. The knight displays an utter disdain for facts and detail and an unquestioned obedience to higher principles. However ridiculous the premises, he shows an absolute faith in the power of rational and causal arguments. And, in war as much as in love, there is this utter reliance on form, on etiquette which first harnesses the passions to render them more effective in the end (in contemporaneous terms, arms control rather than disarmament). Juxtaposed to Don Quixote is Sancho, a man of the people with an unquestioned submission to the obvious common sense and nonsense, unfaltering belief in arguments by design, and great devotion to truth rather than form and a blind abandon to the passions, total havoc, or abject surrender.

But why did I find an elite preference for economic rather than military solutions to British problems? Contemporary economy (with its boards, staffs, lines of command, gains) is far removed from the erstwhile grocer filling bags with sweets and peas, or the second-hand car dealer wheeling and dealing for a precarious existence. Even so, the economic approach is surely a slow one; it is an arduous task of expanding markets, of tooling, training, and trading; and it is far less heroic than the glorious defense of splendid Albion. The slow and protracted change of heart by the British elite in regard to European unification seems not less related to this orientational dilemma than similar aversions on the part of President de Gaulle.

To conclude, in the processing of information I discovered some major dimensions of news interpretation. These dimensions not only distinguished between particular British prestige and mass papers, they probably mark far more universal distinctions between the elites and masses in general. They are, therefore, principles which not only organize editorial content but probably also structure the two worlds of mass and elite action and reflection.

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Minority Group Status and Fertility

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ABSTRACT

Most studies of minority group fertility assume that as assimilation proceeds the fertility of minority and majority populations will converge. Differences between minority and majority fertility are usually treated as temporary phenomena and often are interpreted in terms of the social, demographic, and economic characteristics of minority group members. Empirical evidence, however, does not fully support the "characteristics" explanation of Negro, Jewish, Japanese-American, or Catholic fertility. An alternative hypothesis is presented with respect to the independent effect of minority group status on fertility. Some parameters of the interrelationship of minority group status and fertility are discussed.

The implicit assumption guiding most research on minority group fertility is that, in the process of minority group acculturation and assimilation, fertility behavior and attitudes of minority and majority populations will converge. The underlying argument appears to be that fertility changes are features of social cultural change which, for minority groups, represent one aspect of behavioral and cultural assimilation. In the United States, behavioral and cultural assimilation parallel structural separation of racial, religious, and ethnic populations. Indeed, reproductive behavior and fertility norms have been employed as indicators of acculturation.[1]

It follows that the distinct fertility of minority group members at any point in time merely reflects a matrix of social, demographic, and economic attributes which characterize the minority group. Minority group identification is often treated as an indicator of social class (educational attainment, occupational distribution, and income level), degree of urban concentration and rural experience, and social mobility patterns. Accordingly, it is not minority group status per se but rather the social, demographic, and economic characteristics which minority group membership connotes that determine fertility levels, trends, and differentials.

It may be deduced from this interpretation of minority group fertility that when the social, demographic, and economic characteristics of minority and majority populations are similar—through standardization and statistical controls or at some time in the future—differences in fertility should be eliminated. The current fertility of American minority groups is therefore considered a temporary phenomenon which represents in some cases (e.g., Negroes) social and economic "backwardness" and in other cases (e.g., Jews) a foreshadowing for the Protestant majority.

A sufficient amount of empirical evidence on the fertility of American minority groups is now available, through census data and special sample surveys, to permit a direct test of the "characteristics" explanation of minority group fertility. Essentially, we want to ascertain whether there is empirical support for the hypothesis that the fertility of minority and majority populations is indistinguishable when social, demographic, and economic characteristics are "controlled." If fertility differences persist, the task will be to determine whether, and in what ways, and under what conditions, minority group status operates as an

[1] Milton M. Gordon, *Assimilation in American Life* (New York: Oxford University Press, 1964), pp. 191–92.

independent factor influencing fertility. The minority groups to be considered include: American Negroes, Jews, Japanese-Americans, and Roman Catholics.

THE EVIDENCE

Negroes.—The data and literature unanimously point to the higher fertility of Negro Americans when compared to white Americans.[2] The explanation of higher Negro fertility is almost always couched in terms of the "deprived characteristics" of the Negro population in the United States. Petersen argues that "Negroes have no culture trait that affects natality independently of their occupation or education. Race is not a cause of family size but an index of social class."[3] Furthermore, he suggests that, given increasing levels of urban concentration and social mobility among Negroes, "there is good reason to expect that the differential birth rates by race will begin to converge again in the near future."[4] T. Lynn Smith states that "because Negroes are still residents of the most rural region of the country (the South) and probably because they tend to live in the most rural sections of that region, their rate of reproduction is slightly higher than that of whites."[5] Similarly, Thomlinson suggests that "racial differences in fertility are largely explained by differences in social and cultural environment . . . [and] fertility rates by color appear likely to continue to converge."[6] The Lees, writing with respect to Negro-white differential fertility in 1950, point out that "the pattern of Negro fertility is remarkably similar to that of native whites; . . . the fertility of the Negro most closely approached that of native whites where the Negro has been permitted to share most freely in the general culture."[7] Regarding the future fertility of American Negroes, they suggest that "the higher Negro fertility can be explained in terms of differences in education and socio-economic level. Whenever Negroes and whites are equated in these matters, no matter how roughly, Negro fertility seldom appears much higher than white and it is often lower."[8]

If these widely held *ad hoc* interpretations are entirely correct, it is reasonable to expect similar fertility among Negroes and whites when educational level and region of residence are controlled. The data, however, consistently indicate that this is not the case. United States Census data of 1960 (Table 1) show that urban non-whites (over 90 per cent of whom are Negro) with at least four years of high school education have *lower*, rather than similar, fertility when compared to whites. The fertility difference is substantial: among urban college graduates, family size of non-whites is 20–25 per cent smaller than whites, and family size among urban residents who have more than a college education is between 30 and 40 per cent smaller for non-whites than whites.

Moreover, non-whites within each educational level—from elementary only to college graduates and above—who live in the north central region of the United States have consistently lower fertility than whites. Even in the South, non-white college graduates have substantially lower fertility than college-educated whites. Patterns of lower fertility among non-whites, compared with whites, who are high school

[2] On the higher fertility of American Negroes, see e.g., Wilson H. Grabill, Clyde V. Kiser, and Pascal K. Whelpton, *The Fertility of American Women* (New York: John Wiley & Sons, 1958), chaps. iv–vi; Anders Lunde, "White-Nonwhite Fertility Differentials in the United States," *Health, Education, and Welfare Indicators* (September, 1965).

[3] William Petersen, *Population* (New York: Macmillan Co., 1961), p. 226.

[4] *Ibid.*, p. 228.

[5] T. Lynn Smith, *Fundamentals of Population Study* (New York: J. B. Lippincott Co., 1960), p. 322.

[6] Ralph Thomlinson, *Population Dynamics* (New York: Random House, 1965), p. 178.

[7] Anne and Everett Lee, "The Differential Fertility of the American Negro," *American Sociological Review*, XVII (August, 1952), 446.

[8] Anne and Everett Lee, "The Future Fertility of the American Negro," *Social Forces*, XXXVII (March, 1959), 231.

graduates and residing in northern regions of the United States have also been noted in analyses of 1940 and 1950 Census data.[9]

Additional support for the lower, rather than similar, fertility level of non-whites compared with whites when both are similar in social and economic characteristics may be found in the second "Growth of American Families" study.[10] Although only 270 non-white (256 Negro) women 18–39 years of age, married, living with husband, were interviewed, the results clearly revealed the lower fertility behavior and attitudes of selected, but significant, subgroups within the non-white population (Table 2). College-educated non-whites expected 20 per cent fewer children than whites (2.4 vs. 3.0) and fewer than a matched sample of whites. By the time of the study (1960), non-white, college-educated women had

TABLE 1

CHILDREN EVER BORN PER 1,000 WOMEN EVER MARRIED BY COLOR, AGE, EDUCATION, AND SELECTED RESIDENTIAL AREAS, 1960

RESIDENCE	WHITE			NON-WHITE		
	35–39	40–44	45–49	35–39	40–44	45–49
U.S. urbanized:						
Total	2,427	2,270	2,066	2,647	2,361	2,190
High school completed	2,333	2,132	1,856	2,310	2,080	1,865
College 1–3	2,351	2,146	1,826	2,071	1,971	1,694
College 4	2,404	2,244	1,856	1,886	1,789	1,358
College 5+	2,034	1,933	1,604	1,405	1,261	923
U.S. south:						
College 4	2,312	2,148	1,790	1,828	1,599	1,448
College 5+	2,014	1,916	1,580	1,384	1,174	921
U S. north central:						
Total	2,743	2,611	2,417	2,718	2,444	2,191
No education	3,807	3,579	3,266	....	....	2,748
Elementary 1–4	3,301	3,290	3,267	3,058	2,628	2,272
Elementary 5–7	3,231	3,178	2,962	3,172	2,646	2,397
Elementary 8	3,004	2,883	2,730	2,955	2,583	2,252
High school 1–3	2,825	2,639	2,435	2,866	2,608	2,338
High school 4	2,621	2,440	2,156	2,321	2,142	1,834
College 1–3	2,644	2,485	2,161	2,207	1,827	1,515
College 4	2,603	2,446	2,008	1,961	1,960	1,138
College 5+	2,207	2,034	1,758	1,480	1,111	....

SOURCE.—U.S. Bureau of the Census, *U.S. Census of Population: 1960. Subject Reports. Women by Number of Children Ever Born* (Final Report PC (2)-3A, Washington, D.C., 1964), Table 25, pp. 100–108.

[9] Some studies have shown the lower fertility of Negroes compared with whites at lower than high school educational levels or in other areas than in northern urban sectors (1940 and 1950). We have isolated the high school graduate and urban-northern segments of the Negro population, since we wanted to (1) eliminate the question of possible fecundity differentials by race at lower socio-economic status levels, (2) eliminate the problem of rural slave background and culture, and (3) examine subgroups within the Negro population which may foreshadow trends for the future Negro population as a whole. For evidence on Negro lower fertility in 1940, 1950, and more recent dates, see *ibid.*, pp. 229–30; Clyde V. Kiser, "Fertility Characteristics of the Nonwhite Population in the United States," *Bulletin de l'Institut International de Statistique*, XXXVI, No. 2 (1958), 296–304; Clyde V. Kiser, "Fertility Trends and Differentials among Nonwhites in the United States," *Milbank Memorial Fund Quarterly*, XXXVI (April, 1958), 149–79; "New Patterns in U.S. Fertility," *Population Bulletin*, XX (September, 1964), 136–38; Peter R. Uhlenberg, "Negro Fertility Patterns in the United States," *Berkeley Journal of Sociology*, XI (1966), 54–65.

[10] Pascal K. Whelpton, Arthur Campbell, and John Patterson, *Fertility and Family Planning in the United States* (Princeton, N.J.: Princeton University Press, 1966), chap. ix.

fewer children than whites even when matched very closely to whites on duration of marriage, wife's age at marriage, wife's religion, region of residence, and other selected variables. Furthermore, non-whites of all educational levels living in the northeast expected fewer children than the total or matched white sample. Finally, non-whites in the northeast and north central regions and those with at least some high school education wanted significantly fewer children when compared with total or matched whites.

TABLE 2

MOST LIKELY EXPECTED BIRTHS, BIRTHS BY 1960, AND NUMBER OF CHILDREN WIFE WANTED FOR SELECTED SUBGROUPS WITHIN NON-WHITE, MATCHED WHITES, AND TOTAL WHITE POPULATION (GROWTH OF AMERICAN FAMILIES STUDY, 1960)

	Non-Whites	Matched Whites	Total Whites
Most likely expected total births:*			
Northeast	2.9	3.1	3.2
College	2.4	2.8	3.0
Births by 1960:*			
College	1.7	1.9	2.0
Number of children wife wanted:†			
North east	2.5	3.5	3.5
North central	2.8	3.3	3.4
College	2.4	3.1	3.3
High school 4	2.7	3.0	3.2
High school 1-3	2.7	3.2	3.3

* Adapted from Pascal K. Whelpton, Arthur Campbell, John Patterson, *Fertility and Family Planning in the United States*, Table 183, p. 339.

† *Ibid.*, Table 189, p. 350.

Studies in Detroit in 1954 revealed that Negroes expressed significantly lower family-size ideals than whites. Negroes thought the ideal number of children for a young couple to have should be on the average of 2.3 compared with 3.1 expressed by whites. This differential was the largest found between any major strata, including religion and social class, and the ideal family size expressed by Negroes was the lowest obtained for any subgroup in their 1952 or 1954 samples. Furthermore, 40 per cent of persons stating an ideal family size of less than two children were Negroes, whereas Negroes comprised only 14 per cent of the population.[11] Similar findings were obtained in recent studies of Negroes in Chicago: ideal family size was lower among Negroes compared to whites for the total Chicago sample, as well as when education, income, and occupation were controlled; the proportion approving family limitation was *greater* among Negroes.[12]

Although data on family-size "ideals," "norms," and "desires" are not necessarily the basis for predicting actual reproductive behavior, family size is not likely to exceed "ideals" or "desires" among those segments of the Negro community which plan their families and use effective means of contraception (i.e., college-educated, middle-class, urban Negroes). Moreover, even if fertility norms and behavior are not perfectly correlated, the question remains why Negroes express a lower ideal family size, want fewer children, and approve family limitation in larger proportions than whites.

Taken together, the information available suggests that other factors independent of social and economic characteristics depress the fertility of selected segments of the Negro population below white levels. The explanation of Negro fertility in terms of the characteristics of a deprived population is inadequate precisely among groupings within the Negro population which are the models for the future pattern and direction of fertility of the Negro population as a whole (i.e., the educated, urban, and middle classes). The data strongly point to the probability of lower, rather than similar, fertility of Negroes compared with

[11] Ronald Freedman, David Goldberg, and Harry Sharp, "'Ideals' about Family Size in the Detroit Metropolitan Area: 1954," *Milbank Memorial Fund Quarterly*, XXXIII (April, 1955), 191-93.

[12] Annie O. Blaire, "A Comparison of Negro and White Fertility Attitudes," and Elvira Mendoza, "Socio-economic Correlates of Attitudes toward Family Size," in Donald J. Bogue (ed.), *Sociological Contributions to Family Planning Research* (Chicago: University of Chicago Press, 1967), pp. 8, 42-50.

whites as social and economic equalization with whites occurs at upper and middle socioeconomic levels. Although at ages below twenty-five, Negroes appear to have higher fertility than whites at all educational levels, this seems merely to reflect family formation differences, particularly earlier ages at time of marriage among Negroes, rather than completed family size. It is quite clear that the fertility variation within the Negro population (from an average of more than six children among Negroes in rural-farm areas of the South to an average of less than two children among college-educated northern Negroes) and the higher total fertility of non-whites compared with whites reflect, in large part, social and economic differences. Yet the accentuation of lower fertility among subgroups within the Negro population which most closely approximate the white community must be explained by other factors.

Jews.—At the opposite extreme from the generally higher fertility of American Negroes is the lower fertility of the Jewish population. The evidence from census materials and sample surveys consistently points to the lower fertility of Jews.[13] As with explanations of Negro fertility, lower Jewish fertility is usually interpreted as the consequence of the social and economic characteristics of the Jewish population. The authors of the "Growth of American Families" studies suggest that the long urban experience of Jews and their concentration among the educated, professionals, and middle class result in the lower fertility of Jews when compared with Protestants or Catholics.[14] Similarly, Thomlinson argues that the lower fertility rates of Jews "may be explained by their concentration in cities, their higher occupational standing, their higher educational achievement, and their liberal attitudes towards contraception."[15] Rosenthal dismisses the theory that minority group status exerts an independent effect on Jewish fertility and states that "the religio-cultural complex called Jewishness is not, as has often been theorized, a major factor in the Jewish fertility rate."[16]

Obviously, the concentration of Jews among the urban middle-class population and among the college-educated professionals account, in part, for their lower fertility, just as the "deprived characteristics" of the Negro population partially explain their higher fertility. Nevertheless, several major empirical findings with respect to Jewish fertility reveal the incompleteness of the "characteristics" explanation. First, studies have documented the lower fertility of Jews compared with non-Jews when major social and economic characteristics have been standardized or controlled. For example, Canadian data reveal the lower fertility of Jews in both urban and rural areas (Table 3). Indeed, Jewish women 40–59 years of age in rural areas of Canada have lower fertility than *urban* Protestants. Jews in the United States have lower fertility than Protestants or Catholics when education is controlled.[17] The Princeton fertility studies consistently show that the lower Jewish fertility behavior and atti-

[13] See Calvin Goldscheider, "Fertility of the Jews," *Demography*, IV, No. 1 (Spring, 1967), 196–209.

[14] Ronald Freedman, Pascal K. Whelpton, and John W. Smit, "Socio-economic Factors in Religious Differentials in Fertility," *American Sociological Review*, XXVI (August, 1961), 608–14; Ronald Freedman, Pascal K. Whelpton, and Arthur Campbell, *Family Planning, Sterility and Population Growth* (New York: McGraw-Hill Book Co., 1959), p. 104; Whelpton, Campbell, and Patterson, *op. cit.*, pp. 72–73.

[15] Thomlinson, *op. cit.*, p. 179; see also, Petersen, *op. cit.*, p. 223.

[16] Erich Rosenthal, "Jewish Fertility in the United States," *Eugenics Quarterly*, VIII (December, 1961), 198–217. Gordon, reviewing the report by Freedman, Whelpton, and Smit (*op. cit.*), concludes, "Jewish values on fertility which at first appeared to differ from those of other Americans, turned out to be a function of urbanization and socio-economic position, and thus are seen to be the same as those of Protestants with the same socio-economic distribution and degrees of urban residence" (Gordon, *op. cit.*, p. 192).

[17] Calvin Goldscheider, "Socio-economic Status and Jewish Fertility," *Jewish Journal of Sociology*, VII (December, 1965), 221–37.

tudes are not solely a function of metropolitan residence or social class variables, since, when these are jointly controlled, Jews retain lower levels of fertility when compared with Protestants or Catholics.[18]

The only contradictory evidence indicating that social, demographic, and economic characteristics of Jews account for lower Jewish fertility is the widely cited precision-matching study of sixty-six Jewish couples with Protestants and Catholics.[19] The small number of Jewish cases included in this study raises questions of sample variation, sample representativeness, and sampling error. Averages are presented rather than distributions; differences in family building and formation which directly affect fertility were not considered. However, if we grant that the matching was methodologically sound and if we disregard other evidence which dismisses the "characteristics" interpretation, we must still explain why matched Jewish and Protestant couples differed significantly in the proportion of those who planned the number and spacing of all pregnancies: 47 per cent of the Jews compared with 33 per cent of the matched Protestants.

TABLE 3

NUMBER OF CHILDREN EVER BORN PER 1,000 EVER MARRIED WOMEN BY AGE, RELIGION, AND RESIDENCE, CANADA, 1961

Age and Religion	Total	Urban	Rural
25–29:			
Catholic	2,278	2,077	2,929
Major Protestant	2,120	1,942	2,568
Jewish	1,729	1,728	1,833
30–34:			
Catholic	3,019	2,708	4,013
Major Protestant	2,588	2,386	3,106
Jewish	2,229	2,237	1,111
35–39:			
Catholic	3,590	3,152	4,876
Major Protestant	2,757	2,531	3,323
Jewish	2,263	2,261	2,625
40–44:			
Catholic	3,983	3,408	5,481
Major Protestant	2,744	2,460	3,394
Jewish	2,106	2,104	2,400
45–49:			
Catholic	3,950	3,326	5,570
Major Protestant	2,566	2,275	3,239
Jewish	1,950	1,949	2,200
50–54:			
Catholic	4,143	3,497	5,734
Major Protestant	2,495	2,202	3,181
Jewish	1,873	1,875	1,200
55–59:			
Catholic	4,499	3,869	6,020
Major Protestant	2,630	2,342	3,288
Jewish	2,060	2,062	1,500
60–64:			
Catholic	4,966	4,423	6,321
Major Protestant	2,839	2,531	3,548
Jewish	2,368	2,365	3,333

SOURCE.—Dominion Bureau of Statistics, *Census of Canada* (1961), Bulletin 4:1-8, Table H8, pp. H8-1–H8-4.

Moreover, the evidence of lower Jewish fertility in the United States extends back to the 1880's and in European countries for the last seventy-five years. The matrix of social and economic characteristics of Jews at earlier points in time were those usually associated with higher, rather than lower, fertility.[20] Hence, other factors besides the "characteristics" of Jews must be investigated to account for their lower fertility. As with segments of the Negro population, the consistently lower fertility of Jews over time, in the United States and Western countries, and controlling for residence, social class, and other significant sociological and demographic variables suggests that interpretations of lower Jewish fertility based solely on the characteristics of the Jewish population is inadequate.

Japanese-Americans.—There has been relatively little attention focused on the fertility of American minority groups other than racial (Negro) or religious (Jews and Catholics). Nevertheless, U.S. Census data on Japanese-Americans further bring into question the adequacy of the "characteristics" interpretation of minority group fertility. Although no direct data on the fertility of Japanese-American women by social class measures are available, data on the

[19] Charles Westoff, Robert Potter, Philip Sagi, and Elliot Mishler, *Family Growth in Metropolitan America* (Princeton, N.J.: Princeton University Press, 1961); Charles Westoff, Robert Potter, and Philip Sagi, *The Third Child* (Princeton, N.J.: Princeton University Press, 1963).

[19] Freedman, Whelpton, and Smit, *op. cit.*, pp. 608–14.

[20] Goldscheider, "Fertility of the Jews," *op. cit.*

children ever born by age and residence are revealing (Table 4). Japanese-American women in specific age groups (20–39) have lower fertility than native white Americans when either the total population or the urban sectors are examined.[21] Japanese Americans living in areas of lower Japanese concentration accentuate the trend toward lower fertility (Table 5). The Japanese-Americans in regions outside the West are generally younger, second generation, and characterized by higher socioeconomic status than those in the West, and, hence, distance from the West may be considered an indicator of acculturation. The lower Japanese-American fertility in these areas and in urban areas in general parallels the findings noted earlier among Jews and segments of the Negro population and contradicts the argument that fertility levels converge as acculturation proceeds. Again, other factors, independent of social and economic characteristics, may be operating

TABLE 4

CHILDREN EVER BORN PER 1,000 WOMEN AND PER 1,000 EVER MARRIED WOMEN, NATIVE WHITE AND JAPANESE BY URBAN RESIDENCE AND AGE, 1960

	PER 1,000 TOTAL WOMEN		PER 1,000 EVER MARRIED WOMEN	
	Total	Urbanized	Total	Urbanized
20–24:				
Native white	1,000	866	1,376	1,269
Japanese	436	370	940	860
25–29:				
Native white	1,980	1,806	2,189	2,047
Japanese	1,279	1,178	1,469	1,381
30–34:				
Native white	2,418	2,244	2,584	2,434
Japanese	1,798	1,672	1,968	1,865
35–39:				
Native white	2,488	2,273	2,643	2,445
Japanese	2,165	2,073	2,379	2,314

SOURCE.—U.S. Bureau of the Census, *op. cit.*, Table 8, pp. 20–23.

TABLE 5

CHILDREN EVER BORN PER 1,000 EVER MARRIED NATIVE WHITE AND JAPANESE-AMERICAN WOMEN, AGED 35–44, SELECTED STATES: 1960

STATE*	CHILDREN EVER BORN	
	Native White	Japanese-Americans
California	2,361	2,364
Colorado	2,729	2,487
Hawaii	2,614	2,794
Illinois	2,458	1,913
New York	2,363	1,722
Oregon	2,666	2,671
Washington	2,642	2,196
Other non-Western states	....	1,980

* States with at least 5,000 Japanese-Americans.

SOURCE.—U.S. Bureau of the Census, *op. cit.*, Table 50, pp. 264–314.

[21] Age differentiation among the Japanese-Americans is a sensitive indicator of generation and, in turn, acculturation. The younger second generation (Nisei) differs substantially from the first-generation Japanese-Americans (see Toshio Yatsushiro, "The Japanese Americans," in Milton L. Barron [ed.], *American Minorities* [New York: Alfred A. Knopf, Inc., 1962], p. 323; Dorothy Swaine Thomas, "The Japanese Americans," in Joseph B. Gittler [ed.], *Understanding Minority Groups* [New York: John Wiley & Sons, 1964], pp. 84–108). On the rapid acculturation of second generation Japanese-Americans, see William Petersen, "Family Structure and Social Mobility among Japanese Americans" (paper presented at annual meetings of the American Sociological Association, August, 1967, San Francisco, California).

to depress Japanese-American fertility below native white levels.

It may be noted that the fertility rates of Chinese-Americans in urbanized areas are also below those of native whites but only for the two youngest groups. Age may be viewed as a rough indicator of generation status and, in turn, of acculturation. Consequently, it is particularly significant that, among the younger Chinese-Americans, there appears to be lower fertility. Since the fertility data on Chinese-Americans are sketchy, we can only speculate that, similar to other minority groups, when Chinese-Americans, particularly second and third generations, acculturate and are socially mobile, fertility levels will be lower than comparable socioeconomic status groups among native whites.

It is important at this point to note that the Negro, Jewish, and Japanese-American populations do not have specific norms or religio-cultural ideologies which encourage large families. The identification of persons with these minority communities would in no way require large family-size ideals or behavior. Except for numerically insignificant sectors of the Negro (Black Muslims) and Jewish (Hasidim and some ultra-Orthodox Jews) populations, membership in these minority communities implies no religious, cultural, or social ideology which values large families or any group norm which prohibits the use of efficient contraceptive methods.

Roman Catholics.—Perhaps because of the relatively small proportion of Negroes, Jews, and Japanese-Americans in the U.S. population, few research studies which focus on their fertility have been undertaken. The number of couples who identify with these minority groups who have been included in major surveys of American fertility has been small and almost always limited detailed investigation. Moreover, implicitly, the assumption is often made that their unique fertility represents a temporary phenomenon, reflecting a cultural or social lag. It is interesting to note that this same faulty assumption characterized the early studies of American Catholic fertility.[22] But because their relative numerical representation in the U.S. population is greater, more data are available, and their unique fertility patterns have been extensively studied.

In the past, most American demographers had assumed that the higher fertility of American Catholics represented a "cultural lag," but it is quite clear that "the distinctive Catholic pattern is not a result of low social or educational status or of recent urbanization. In fact, it is most distinctive among the well-educated urban group."[23]

The evidence available on Catholic fertility irrefutably dismisses the "characteristics" hypothesis. However, it is not at all clear what factors interact to produce the observed Catholic–non-Catholic fertility differential. Part of the differential may be attributed to the opposition of the church to efficient methods of contraception and to the normative encouragement of the church for large families.[24] Yet, the role of the church in the United States, and perhaps also in the Netherlands, appears to be different from that in other countries. Moreover, the higher fertility of American Catholics cannot be attributed to one specific

[22] Samuel A. Stouffer, "Trends in the Fertility of Catholics and Non-Catholics," *American Journal of Sociology*, XLI (September, 1935), 143–66; Freedman, Whelpton, and Smit, *op. cit.*, p. 609. The first major study of American fertility, the Indianapolis study, focused on native white Protestants, since "it was believed that this group was setting the fertility pattern to which other population groups would eventually conform" (Clyde V. Kiser, "Aims, Methods and Some Results of the Indianapolis Study," *Report of the Proceedings of the Fifth International Conference on Planned Parenthood* [Tokyo, Japan, October, 1955], p. 98).

[23] Ronald Freedman, "American Studies of Family Planning and Fertility: A Review of Major Trends and Issues," in Clyde V. Kiser (ed.), *Research in Family Planning* (Princeton, N.J.: Princeton University Press, 1962), p. 224.

[24] See Judith Blake, "The Americanization of Catholic Reproductive Ideals," *Population Studies*, XX (July, 1966), 27–43.

doctrinal element.[25] Nor could one predict from Catholic doctrine the actual fertility behavior of contemporary Catholics. The fertility norms and behavior of Catholics accentuate the "large" family only in comparison with Protestants but within a narrow range of two to four children.[26]

Part of the explanation of the higher fertility of subgroups within the American Catholic population (e.g., better educated, more church affiliated, or those exposed and trained in Catholic-related schools) may reflect the church's religious ideology with respect to family size and contraception. But ethnic fertility differences, particularly changes within the Irish and Italian segments of the American Catholic community[27] and other fertility differentials[28] would best be treated in the larger context of minority group assimilation and acculturation. It appears from the various findings with respect to Catholic fertility that Catholics consistently have higher fertility than Protestants but that this difference varies with the nature of church identification, which in turn may reflect ethnic acculturation types among Catholics.

The general and widely accepted notion that the peculiar fertility of American Catholics is a function of specific norms about family size and contraception appears superficial as well as inadequate. This level of explanation is analogous to explaining the unique voting or political behavior and attitude of Catholics in terms of *specific* church norms with respect to voting or political behavior. But, just as when we deal with other social behavior of Catholics we attempt to invoke the broader socio-cultural complex of the Catholic situation in the United States, it seems necessary to broaden our explanation of Catholic fertility behavior. The distinctions between the fertility of Irish and Italian Catholics are not so much in terms of specific commitments of these ethnic subgroups to the family size and contraceptive norms of the church. Instead, the distinctions tend to be between the integration of these groups into the institutionalized Catholic structure, the assimilation-acculturation patterns of the two groups, and the more general social and cultural milieu of the American Catholic subgroup.[29]

MINORITY GROUP STATUS AND FERTILITY HYPOTHESIS

From the review of Negro, Jewish, Japanese-American, and Catholic fertility, it appears that one element, which has been neglected in previous investigations of minority group fertility, is the independent role of minority group status. The explanation of the lower fertility of Jews and segments of the Negro and Japanese-American populations may reside in the interaction of minority group status and social and economic status. Membership in, and identification with, a minority group which does not have a normative system encouraging large families, and which does not prohibit or discourage the use of efficient methods of contraception, depresses fertility below majority levels. This is not to argue that a significant part, of unknown magnitude, of the generally higher fertility

[25] Thomas K. Burch and Henry J. Jacek, Jr., "Church Teaching and the Fertility of Catholic Americans: A Partial Replication" (paper presented at the annual meeting of the Population Association of America, April 1967, Cincinnati, Ohio); see also, Ronald Freedman, "The Sociology of Human Fertility," *Current Sociology,* X/XI, No. 2 (1961–62), 63–64.

[26] See Kingsley Davis, "Values, Population and the Supernatural: A Critique," in George Mair (ed.), *Studies in Population* (Princeton, N.J.: Princeton University Press, 1949), pp. 135–39; Whelpton, Campbell, and Patterson, *op. cit.*, pp. 71–72; Burch and Jacek, *op. cit.*; Freedman, "American Studies . . . ," *op. cit.*, p. 216, and sources cited.

[27] See John M. Goering, "The Structure and Process of Ethnicity: Catholic Family Size in Providence, Rhode Island," *Sociological Analysis,* XXVI (Fall, 1965), 129–36.

[28] On suburbanization and Catholic fertility, see Basil G. Zimmer and Calvin Goldscheider, "A Further Look at Catholic Fertility," *Demography,* III (Fall, 1966), 462–69.

[29] On the Irish and Italian socio-cultural situation, see Nathan Glazer and Daniel Moynihan, *Beyond the Melting Pot* (Cambridge, Mass.: M.I.T. Press, 1963).

of Negroes and lower fertility of Jews is not a direct result of the social, demographic, and economic characteristics of these groups. Rather, the question remains as to what happens when the discrepancies in social characteristics are eliminated and no longer operate to differentiate fertility behavior and attitudes. In general, the residue lower fertility of minority groups may result from the insecurities associated with minority group status.

If minority group integration and identification (which are natural rather than temporary phenomena in a structurally pluralistic society) imply greater commitments to a religious ideology or socio-cultural norm encouraging large families or restrictions on the maximum choice with respect to contraception usage, then minority group status will operate to enhance the differential between minority and majority groups through higher minority group fertility. Differences within the Catholic community reflect in part the degree of minority group integration, that is, receptivity to the norms governing Catholic identification.[80]

The isolation of minority group status as an independent factor in fertility is only a starting point. The significant, yet unresolved, issue is how does minority group status depress fertility, and what dimensions of minority group identification affect fertility. Some tentative generalizations emerge from our review of the fertility of racial (Negroes), religious (Jews and Catholics), and nationality (Japanese-Americans) groups in the United States. These generalizations require rigorous testing but are presented here as hypotheses deduced from the available evidence.

First, as a general principle, it is clear that Negro, Jewish, Japanese-American, and Catholic fertility must be treated within a broader context of other social behavior characterizing these minority groups. Minority group status and fertility may very well be a special case of the general relationship of minority group status and social behavior. Since these minority groups have experienced social and cultural changes of various velocities at different points in their American history, the examination of minority group status and fertility must be considered within a dynamic framework of socio-cultural change. The changing relationship of minority group status and fertility operates within the particularized social situation of minority groups. We need to treat the fertility of minority groups in a more cogent and coherent sociological frame of reference as a substitute for *ad hoc* interpretations.

Second, a key element in the relationship of minority group status and fertility is the degree of, and desire for, acculturation. The degree of acculturation and the desire for acculturation do not necessarily imply structural integration or total assimilation.

Rather, it may be the combination of cultural and behavioral convergences with the majority and structural separation which produces the insecurities of minority group status.[31] The struggle to advance up the educational and other social and economic scales appears to be harder for minority group members. As minority group persons enter more generally into competition with the majority community, they

[80] See Lincoln Day, "Fertility Differentials among Catholics in Australia," *Milbank Memorial Fund Quarterly*, XLII (April, 1964), 57–83; F. Van Heek, "Roman Catholicism and Fertility in the Netherlands: Demographic Aspects of Minority Status," *Population Studies*, X (November, 1956), 125–38; Lincoln Day, "Catholic Teaching and Catholic Fertility" (United Nations World Population Conference, Belgrade, Yugoslavia, September, 1965 [B.2/V/E/202]); Thomas K. Burch, "The Fertility of North American Catholics: A Comparative Overview," *Demography*, III (Spring, 1966), 174–87.

[31] The literature on "marginality" suggests that persons in this position frequently are characterized by feelings of insecurity, strong self-consciousness, and irrationality in behavior (see Arnold Green, "A Re-examination of the Marginal Man Concept," *Social Forces*, XXVI [December, 1947], 161–171; Aaron Antonovsky, "Toward a Refinement of the 'Marginal Man' Concept," *Social Forces*, XXXV [October 1956], 57–62; Alan Kerckhoff and Thomas McCormick, "Marginal Status and Marginal Personality," *Social Forces*, XXXIV [October, 1955], 48–53).

may tend to counteract some of their disadvantages by deferring or limiting childbearing.[32] If the desire for acculturation is not an integral part of the social situation of the minority group, members of minority groups often become concerned with group preservation and quantitative strength.[33] Resistance to assimilation tends to enforce the persistence of traditional patterns of family life conducive to high fertility in spite of technical and economic conditions that might otherwise lead to lower fertility.[34]

Third, within the context of acculturation, the social and economic characteristics of minority groups are of critical importance. The relationship between minority group status and fertility does not operate at all socioeconomic levels. On the one hand, equalization of social and economic characteristics implies, at a minimum, similar access and knowledge regarding birth control methods and similar social and economic motivations and social pressures for their effective use. On the other hand, the dynamic aspect of minority group acculturation implies that real or perceived opportunity for social mobility may substitute for equivalent social characteristics. Thus, achievement values must be present for minority group members to translate the "goals" of social mobility and concomitant acculturation for themselves and their children into "means" which include family-size limitation. The desire to improve and achieve within an acculturation situation implies deferred gratification greater than members of the majority group who do not suffer the disadvantages and ambiguities of minority status. Moreover, the insecurities associated with minority group status are manifest most clearly when minority group members are in "marginal" positions vis-à-vis the majority and minority communities. In most cases, this marginality occurs among the second and third generations when there is social mobility out of the lowest social classes often accompanied by geographical mobility out of the "ghetto." Among immigrant groups (Japanese, Chinese, Jews, Catholics), age may serve as a rough indicator of second-generation marginality and acculturation. Among Negroes, the acculturation process typifies second-generation urban in-migrants at the middle- and upper-class levels. Consequently, the insecurities associated with minority group status will be greater among non-white college women than among non-white women of low education, and will vary for immigrant minority groups in terms of the degree and swiftness of their particular acculturation patterns. Hence, Jews have experienced fertility reductions earlier than Italian Catholics, Japanese earlier than Chinese.

Finally, the deviant Catholic fertility pattern, that is, higher rather than lower fertility in the acculturation process, suggests that, on one hand, specific norms regarding family size and birth control must be considered. Obviously, identification with a minority group characterized by a large family-size norm and ideological prohibitions against efficient contraceptive methods raises rather than lowers fertility. The distinction between changes in the fertility of Irish and Italian Catholics reflects, in part, the differential acculturation of the two groups in conjunction with their differential integration and identification with the church. Furthermore, although

[32] For the recognition of the possible role of minority group status in Negro fertility, see Lee and Lee, "The Future Fertility of the American Negro," *op. cit.*, p. 231; on Catholic fertility and minority group status, see Burch and Jacek, *op. cit.*, and references cited in n. 30 above; on Jewish fertility and minority status, see Nathan Goldberg, "The Jewish Population in the United States," in the *Jewish People, Past and Present* (New York: Central Yiddish Culture Organization, 1949), Vol. II; Charles F. Westoff, "The Social-psychological Structure of Fertility," *International Population Conference* (Vienna, 1959), pp. 361–62; Goldscheider, "Fertility of the Jews," *op. cit.*

[33] This appears to be true of such diverse groups as Black Muslims, Hasidim, and Hutterites.

[34] See the discussion of the Amish in Pennsylvania and the Mormons in Utah by Frank Lorimer, *Culture and Human Fertility* (New York: UNESCO, 1954), p. 184.

specific norms with respect to family size and contraception are significant, the broader socio-cultural situation of Catholics must be considered.

As a general hypothesis we would argue that the insecurities of minority group membership operate to depress fertility below majority levels when (1) acculturation of minority group members has occurred in conjunction with the desire for acculturation; (2) equalization of social and economic characteristics occurs, particularly at middle and upper social class levels, and/or there is a desire for social and economic mobility; and (3) there is no pronatalist ideology associated with the minority group and no norm discouraging the use of efficient contraceptives.

Obviously, the nature of minority group identification assumes that intragroup social contact is fostered, particularly primary group relations.[35] Indeed, the quality of minority group cohesion and integration becomes a key axis of fertility heterogeneity within the minority group. The degree of minority group integration and the accentuated marginal position between acculturation and structural separation will determine the behavior patterns of minority group members, all other things considered, vis-à-vis the majority community.

The analysis of minority group fertility suggests that previous explanations based on social and economic characteristics have been incomplete. The alternative hypothesis that, under given social and economic changes and concomitant acculturation, the insecurities and marginality associated with minority group status exert an independent effect on fertility appears consistent with the evidence. Whether the disadvantages and insecurities of minority group identification in fact are related to the fertility behavior and attitudes of minority group members should now be directly tested. Future research in addition might focus on the social psychological mechanisms which may be operating and extend the investigation to incorporate other minority populations within and outside the United States. One thing, however, emerges from our review: the "characteristics" interpretation of minority group fertility is theoretically and empirically limited. Only when minority groups are viewed as involving subcommunities and subcultures and only when fertility is viewed as social behavior or social process can we expect beginning solutions to the sociological understanding of the fertility patterns of minority populations.

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[35] It is of major significance that religious and racial groups are highly endogamous and that the close relatives and friends of a majority of minority group members share the same religious or ethnic identity. While members of different minority groups interact in economic and other "impersonal" spheres of activity, they frequently do not interact in primary group relations where much of the shaping of values and internalization of norms takes place. The more isolated and self-contained the minority community, the greater the pressure toward conformity to group norms (see Gerhard Lenski, *The Religious Factor* [New York: Doubleday & Co., 1961], pp. 36–40, 320–26; Gordon, *op. cit., passim;* Sidney Goldstein and Calvin Goldscheider, *Jewish Americans: Three Generations in a Jewish Community* [Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968]).

Italians and Crime in Chicago: The Formative Years, 1890–1920

Humbert S. Nelli

ABSTRACT

To most contemporaries in the period to 1921, Italian crime in the United States and specifically Chicago meant the Black Hand or Mafia. Critics ignored—or were too involved to perceive—important aspects of Italian crime, including its role in group adjustment, its breakdown into two major levels (one operating entirely within the ethnic colony, the other in the wider American community), the variety of non-Black Hand crimes within the colony, and the strong groundwork laid in the pre-Prohibition era for Italian dominance of Chicago's crime during the decade of the 1920's.

I

Americans reacted to crime among Italian newcomers with a frenzy of emotion aroused by no other immigrant activity. Official reports, books, pamphlets, magazine articles, and newspaper stories criticized and analyzed, lamented and decried Italian criminality which, in the period to 1921, invariably meant the Black Hand, or Mafia. Some writers argued that Italians naturally possessed criminal inclinations, others blamed slum conditions in American cities, and still others denied the existence of lawbreaking organizations in the United States patterned after the Sicilian Mafia or the Neapolitan Camorra.[1]

Contemporaries emphasized problems such as possible operating procedures of the Black Hand among Italians in America, whether Italian crime represented a reaction to new-world conditions or a carry-over of old-world traditions, and whether Italian criminals worked from centralized headquarters or as independent groups and individuals. Preoccupation with these and similar aspects obscured factors of deeper and more long-lasting significance. First, the groundwork of Italian dominance of Chicago's crime after 1920 was laid in the years prior to national prohibition legislation. In addition, Italian lawbreaking in the era before 1920 involved a wide variety of operations, many of them with no Black Hand connections. In those years also—a time of mass immigration from the Italian kingdom—crime served a useful (although not necessarily desirable) function as one facet of group adjustment to a new envi-

[1] For a variety of opinions on the questions of Italian criminality and whether the Black Hand existed see: John Chetwood, Jr., *Immigration Fallacies* (Boston: Arena Publishing Co., 1896), pp. 141–42; Arthur Train, *Courts, Criminals and the Camorra* (New York: Charles Scribner's Sons, 1912), p. 214; U.S. Senate, *Reports of the Immigration Commission* (1911), IV, 209; Isaac A. Hourwich, *Immigration and Labor: The Economic Aspects of European Immigration to the United States* (New York: B. W. Huebsch, Inc., 1912), pp. 358–61; Eliot Lord, John J. D. Trenor, and Samuel J. Barrows, *The Italian in America* (New York: B. F. Buck & Co., 1905), pp. 209–10, 216; William S. Bennet, "Immigrants and Crime," *Annals of the American Academy of Political and Social Science*, XXXIV, No. 1 (July, 1909), 120–21; Arthur H. Warner, "Amputating the Black Hand," *The Survey*, XXII (May 1, 1909), 167; Editor's Note to anonymous article, "The Black Hand Scourge," *Cosmopolitan*, No. 47 (June, 1909), p. 31; Gaetano D'Amato, "The 'Black Hand' Myth," *North American Review*, CLXXXII (April, 1908), 544; Tomasso Sassone, "Italy's Criminals in the United States," *Current History*, XV (October, 1921), 23; *Chicago Record-Herald*, March 26, 1910; John Landesco, *Organized Crime in Chicago. Part III of the Illinois Crime Survey* (Chicago: Association for Criminal Justice, 1929), pp. 938, 941–42; *L'Italia* (Chicago), January 24, 1891, October 8, 1892, December 7, 1919; *Vita Nuova* (Chicago), April, 1925; *La Parola dei Socialisti* (Chicago), March 28, 1914.

ronment. Finally, two separate and relatively distinct levels of criminal activity involving Italians developed during the formative period of Italian crime in Chicago. One was entirely within the immigrant community and affected only its residents; the other took place as Italians began to move into "big time" crime which operated within the American community. Before January 16, 1920, most Italian crime took place only within the colony. Prohibition encouraged many to move into organized crime, which they eventually came to dominate in Chicago.

II

A series of events that began in New Orleans in 1890 had a profound effect on the Chicago Italian community. On October 20, 1890, New Orleans Superintendent of Police Peter Hennessey was murdered. City residents assumed Sicilians to be responsible, for Hennessey had been engaged in a crackdown on crime in the Italian community. In a city-wide atmosphere of near hysteria, the police arrested hundreds of Italians for the crime and brought nine to trial. To the consternation of the American community, the jury found six defendants "not guilty" and could reach no verdict on the other three. Rumors of bribery and intimidation of witnesses filled New Orleans; public officials and city newspapers demanded that "the failure of justice" be remedied. With widespread local support, a mob raged to the parish prison, dragged from it eleven Italian prisoners, and lynched them.

The incident quickly grew into a national crisis and an international affair. Italy demanded punishment of the lynch mob and financial compensation for the victim's families. For a short time in 1891, war between Italy and the United States appeared to be a distinct possibility. President Harrison eased the situation somewhat when he spoke of the New Orleans affair in his annual message to Congress (on December 9, 1891) as "a most deplorable and discreditable incident, an offense against law and humanity." Relations between the two countries improved in the following months, and the breach healed rapidly after Harrison offered an indemnity payment.[2]

Chicago's Italian leaders sympathized with the New Orleans Italian community and subscribed money to pay for the defense of the nine accused men. They emphasized the need for group solidarity and co-operation and tended to view the New Orleans affair as part of a general pattern of anti-Italian attitudes on the part of Irish policemen and English-language newspapers in New Orleans, Chicago, and other American cities. Applauding the Italian government's involvement on behalf of the families of the eleven murdered men, they strongly supported the mother country in her diplomatic exchanges with the United States. They were shocked by Italy's acceptance of the American government's offer to pay what *L'Italia* described on March 19, 1892, as "a small indemnity" without recognizing or suitably condemning American responsibility in the affair.

Prior to 1890, crime in Chicago's Italian community had consisted of illegal activities in employment, child labor, crimes of passion growing out of alcoholic indulgence or presumed insults to female family members, and vendettas from the old country. Outlaws within the community had not organized, and the term "Mafia" received little attention in the city's newspapers in connection with the city's Italians. The New Orleans affair marked a complete turning point.

To their horror, Italians in Chicago found themselves objects of open fear and contempt during the course of the affair. The word "Mafia" now appeared frequently in Chicago's newspapers, and many tabloids proclaimed the criminal inclinations of all Italians, doubtless as an outgrowth

[2] John E. Coxe, "The New Orleans Mafia Incident," *Louisiana Historical Quarterly*, XX, No. 4 (October, 1937), 1067–1110; John S. Kendall, "Who Killa de Chief?" *Louisiana Historical Quarterly*, XXII, No. 2 (April, 1939), 492–530; A. Pierantoni, *I Fatti di Nuova Orleans e il Diritto Internazionale* (Rome, 1891).

of the popular assumption that the Mafia had effected Hennessey's murder. Americans concluded that the Mafia flourished wherever "Southerners" (Sicilians and mainland Italians from the area south of Rome) lived. This belief strongly affected Chicagoans, for by 1891–92 the city contained a sizable and rapidly growing Italian community composed largely of "Southerners." Italian leaders reacted for personal reasons (in order to gain American acceptance), as well as through ethnic pride, by denying the existence of the Mafia in Chicago. Some, like Oscar Durante, editor and publisher of Chicago's *L'Italia,* insisted that it did not exist even in Italy.[3]

In order to make clear to Americans that old-world criminal patterns and groups did not carry over in immigrant-colony crime, Italian-language papers began, shortly after the turn of the century, to use the term "Black Hand" to identify crimes within the immigrant community. This term became favored by both American and foreign-language journals in the first two decades of the twentieth century. From 1904, for example, "Black Hand" supplanted "Mafia" as the preferred term for crimes committed by Italians against each other, at least so far as *L'Italia* was concerned, and the replacement remained in use until the 1920's.[4]

Whichever name they used to publicize crimes, Italian-colony newspapers at first denied the existence of organized gangs. In an article entitled, "The 'Black Hand' in Chicago, Threatening Letter Causes Panic," printed in 1904, Durante repeated a belief he had often stated in the past in *L'Italia:* "The 'Black Hand' does not exist, the crimes must be treated singly as individual events which do not have any relation with organized groups." Over the passage of time, the press attributed an increasing number of crimes to the Black Hand. Even *L'Italia* credited a quantity of blackmailings, bombings, and murders to the secret society: "Black Hand in Chicago. Blackmail Letter for $300" (July 14, 1906); "Italians on Trial. 32 Italians Accused as Members of the Black Hand" (April 27, 1907); "Once Again the Black Hand" (June 5, 1909).[5]

In 1910 *L'Italia* reported the arrest of Black Hand leaders and hopefully echoed city authorities: "Chicago police are elated over the arrest of four Italians accused of being leaders of the so-called Black Hand organization of Chicago." As in earlier cases, however, crimes continued after the reported capture of Black Hand leaders and the smashing of criminal societies.[6]

Oscar Durante admitted in February, 1911, that "if we continue at the present rate, the Italian colony of Chicago will soon take the lead over that of New York, in the matter of bombings, murder, and blackmail." In fact, the number of criminal activities in the Italian colony increased

[3] *L'Italia,* October 25, November 1, 8, 22, and 29, December 13 and 27, 1890; March 7, 14, 21, and 28, April 4, 18, and 25, May 9 and 23, July 18, August 8, September 12, November 28, December 12, 1891; February 6, March 19, April 16, 1892.

[4] Several theories have been offered to explain the appearance of the term "Black Hand" to describe certain types of Italian colony crime. Alessandro Mastro-Valerio, publisher and editor of *La Tribuna Italiana Transatlantica* (Chicago), February 1, 1908, claimed that Carlo Barsotti, editor of New York's *Il Progresso Italo-Americano,* had coined the term in order to avoid using the word "Mafia," and in hopes that the offenses thus identified would be viewed as responses to American conditions. Perhaps the most acceptable was one advanced by Gaetano D'Amato, a New York City Italian community leader and friend of police Lieutenant Joseph Petrosino of New York's Italian detective squad, in a 1908 article, "The 'Black Hand' Myth," p. 548. D'Amato stated that "the term 'Black Hand' was first used in this country about ten years ago, probably by some Italian desperado who had heard of the exploits of the Spanish society, and considered the combination of words to be high-sounding and terror-inspiring. One or two crimes committed under the symbol gave it a vogue among the rapacious brotherhood; and, as it looked well and attracted attention in their headlines, the newspapers finally applied it to all crimes committed by the Italian banditti in the United States."

[5] *L'Italia,* August 27, September 10, 1904; July 14, 1906; April 27 and August 10, 1907; March 7, 1920. See also *La Tribuna Italiana Transatlantica,* November 9, 1907; February 8, 1908.

[6] *L'Italia,* April 16, 1910; August 10, 1901; *La Tribuna Italiana Transatlantica,* February 8, 1908.

as the community grew in inhabitants, although few contemporaries recognized the relationship between crime and population growth. The editor blamed Italian-Americans for the fear-ridden situation, for "when they are questioned by the police, they become as dumb as fish and answer with the usual shrug of the shoulders so that . . . the criminal element flourished until its effect is felt from one end of the city to the other." Betraying his middle-class orientation, he complained that these conditions worked "to the detriment of the better class Italians."[7]

Although in April, 1910, *L'Italia* congratulated the police on their activities against the Black Hand—"This arrest is proof that the forces of law and order in our city are always on the alert"—one year later Durante held local officials responsible for the Black Hand's existence. "There have been 34 murders in the past two years, and not one of these murders has been solved by the police. This lack of capacity shown by the police gives criminals the courage to strike again. The police must cooperate with us to clear up this situation." Mastro-Valerio and other community spokesmen agreed that corrupt, incompetent police encouraged or permitted widespread criminal activity in the Italian colony. Many Americans reached the same conclusion. The Black Hand, they argued, could not possibly function without police complicity or inability. The Massachusetts Commission of Immigration, for example, identified police corruption, "which takes the form of protection of criminals," as the factor primarily responsible for Black Hand successes in cities outside Massachusetts, including Chicago.[8]

Fear and concern pervaded the Italian community because of the Black Hand's notoriety. Editor Mastro-Valerio warned in *La Tribuna Italiana Transatlantica* of the all-encompassing menace of the secret crime society: "Yesterday this one, today that one, tomorrow it may strike even you." Durante felt compelled to publicize the seriousness of the Black Hand problem, which he had for so long denied: "Day after day, week after week, more and more crimes are being committed, the perpetrators of which are invariably Italians." Admitting that almost every crime involved the name of the Black Hand, he described a typical method of acquiring money. Blackhanders simply demanded cash "from innocent people who work for a living." If the gangsters received nothing, they murdered their victim; should they spare his life, they bombed his home. "Naturally, these frequent crimes give a bad impression of the Italian colony to other nationalities. . . . Italians! We must do something to prevent these crimes."[9]

As the numbers of Chicago's "Southerners" increased, they became an increasingly tempting source of money, more readily intimidated by threats because of the reputation of the Mafia in Sicily. After the publicity surrounding the New Orleans affair, the American press quickly labeled all crimes in the immigrant community Mafia acts. The successes or failures of Chicago's Italian-community gangs, however, depended less on old world techniques and personnel than on local conditions and opportunities.[10]

III

In 1907 and 1908 leaders of the Italian community co-operated with leaders of the press in a concerted effort to combat and eliminate Black Hand crimes. On November 11, 1907, they organized the White Hand Society, largely through the efforts of Italian consul Guido Sabetta. The Unione Siciliana played a leading role in

[7] *L'Italia*, February 4, 1911.

[8] *L'Italia*, April 1, 1911; *La Tribuna Italiana Transatlantica*, November 9, 1907; Grace Abbott, *Immigration Problems in Massachusetts. Report of the Commission on Immigration* (Boston: Wright & Potter Printing Co., 1914), pp. 105–6.

[9] *La Tribuna Italiana Transatlantica*, November 9, 1907; *L'Italia*, April 1, 1911.

[10] The Italian "White Hand" Society in Chicago, Illinois, *Studies, Action and Results* (Chicago, 1908), pp. 25–26.

the formation and operation of the White Hand, a significant fact in the light of that society's involvement with criminals in the 1920's and the last years of the preceding decade. The Unione's unfortunate entanglement with criminals in later years should not detract from its sincere concern in 1907 and 1908 over the unfavorable image which Black Hand activities presented of "Southerners" and especially Sicilians, and its efforts to eradicate crime within the immigrant community.

Community newspapers, particularly *L'Italia* and *La Tribuna Italiana Transatlantica*, gave full support to the new organization, as did the Italian ambassador in Washington and the Italian Minister of Foreign Affairs in Rome. The organizers felt that a glorious era had begun with their "war without truce, war without quarter." Bubbling enthusiasm filled early meetings, held to plan action against the enemy.[11]

By the end of November the idea of the White Hand had spread to other cities, and the founders conceived the notion of organizing a White Hand "in all the cities which contain large Italian colonies, which suspect the existence of mafiosi or cammoristi in their midst." The eventual goal, "the day in which the Italian colonies, free of the festering evil of the Black Hand, will become models among the foreigners," unintentionally publicized the problem of crime among Italians in the United States. Ironically, many of the leaders who formulated this goal had minimized the seriousness of Italian crime or publicly denied the existence of the Black Hand. Some continued to deny its existence while supporting White Hand activities against it.[12]

The White Hand was legally incorporated in December, 1907. Joyously the members announced that at last Chicago's *cafoni*, or ignorant "Southern" peasants, too often the victim of Black Hand hoodlums, had "a protector in the White Hand." As stated in the society's constitution, the White Hand intended "to paralyze and eradicate individual and organized crime which exists in the midst of the Italian colony, forcing it to submit to threats and violence," to eliminate a pervasive atmosphere of "mystery and terror," and to "present to the American public the truth concerning Italy and the Italians, frequently misrepresented by incorrect reports."[13]

In January, 1908, the White Hand called for closer co-operation with various Italian societies in the city and for a program to educate *cafoni* (in Chicago and other cities) as to their duties and obligation. The society also announced its first tangible success, claiming that it had driven ten of Chicago's most dangerous criminals out of the city. By February the society listed additional successes in Chicago and similar achievements in other American cities. *L'Italia* jubilantly announced, "Black Hand in Hot Water. The Arrest of One of the Heads Places the Black Hand in the Hands of the Police." The White Hand's attorney, Stephen Malato, played a major role in this capture.[14]

Never again did the White Hand achieve such glory, although it remained in existence for a number of years. It soon became a dead letter, failing in large part because of inadequate financial support. White Hand leaders lamented as early as 1908 that "so wealthy a colony as that of Chicago should have given such niggardly support to a movement intended to . . . purge it" of the Black Hand menace. Indifference, a more basic factor, lay behind this financial distress. Reluctantly the society's leaders admitted that they could not gain, much less hold, the support of immigrant-

[11] *La Tribuna Italiana Transatlantica*, November 16 and 23, 1907; *L'Italia*, November 16, 1907.

[12] *L'Italia*, November 30, 1907. For the appearance of the White Hand in New Orleans see *La Tribuna Italiana Transatlantica*, December 7, 1907.

[13] *La Tribuna Italina Transatlantica*, December 14 and 28, 1907. See Italian "White Hand" Society, *Studies, Action and Results*, pp. 21–22, for aims of the organization. (The text is in both Italian and English.)

[14] *La Tribuna Italiana Transatlantica*, January 11, February 1, 15, and 22, March 7, 1908; *L'Italia*, February 15, March 14, May 23, 1908.

community residents, the American public, or the police. The society maintained that immigrants believed that it planned to use the money collected from impoverished workers "to defend the lives and wealth of the prominent men of the colony." Among upwardly mobile members of the community the conviction had grown that, by admitting the gravity of the problem of crime among Italians, the White Hand had "thrown suspicion and discredit upon the Italian name."[15]

More fundamental to rank-and-file immigrants, however, was their sense of self-preservation which, rather than apathy or desire to protect their Italian good name, determined their behavior. "Southerners" knew that crime pervaded their communities; they knew also that authorities could or would do little to terminate it. They believed the White Hand to be powerless in the face of official corruption or tolerance of corruption, and they had no desire to involve themselves with it.[16]

While this situation existed in Chicago, it did not necessarily operate elsewhere. Local factors at work in individual cities strongly influenced and determined the course taken by Black Hand gangs. The Illinois Crime Commission noted that "in some American cities where the law is effectual, the 'black handers' have ceased to operate." According to John Landesco of the Illinois Crime Survey, who observed conditions in Chicago and Milwaukee for a number of years, the Black Hand in Milwaukee had a much shorter life, and "Southerners" adjusted to their new surroundings with a minimal amount of crime and dealings in corrupt politics. John S. Kendall stated that in New Orleans the Sicilian immigrants "under the leadership of the better elements of their own nationality, . . . set their faces against the activities of such organizations," with the result that the Black Hand disappeared in that city in the years after 1907.[17]

The adjustment of Italians in Boston and elsewhere in Massachusetts contrasted even more sharply with the Chicago experience. In a study published in 1902, social worker Robert A. Woods maintained that few murders committed by Italians in the North End of Boston could be attributed to premeditation, for in the majority of cases impulse and passion formed the motivating forces. Furthermore, no murder had been traced "directly or indirectly" to the Mafia. Twelve years later the Massachusetts Commission on Immigration reported that "so-called 'Black Hand' crimes are practically unknown in Massachusetts," although Italians committed criminal violations. As a "very large proportion" of the immigrants came from the "South," the Commission considered the absence of Black Hand crime to be proof that "local American conditions are responsible for those criminal organizations elsewhere."[18]

Even more striking in indicating the impact of local conditions on crime were differences between Italian colonies in the United States and those in Latin America, specifically Brazil and Argentina, the countries of major Italian immigration. Radicalism, rather than crime in the Chicago style, constituted the most serious social problem involving Italian immigrants in South America. Argentines, for example, viewed socialism and anarchism among Italians as grave difficulties, in contrast to the more limited appeal which these doc-

[15] Italian "White Hand" Society, *Studies, Actions and Results*, p. 24. Also *Chicago Record-Herald*, March 20, 1911; *La Parola dei Socialisti*, March 28, 1914. American lack of support rested on the belief that the entire situation was the problem of immigrants alone, so long as the criminals victimized only other "Southerners." The 1920's brought a change, both in victims and in attitudes.

[16] Local Community Research Committee, *Chicago Communities*, Vol. VI, "East Side," Document 8. See also Vol. VI, "West Englewood," Document 1*a*; *La Tribuna Italiana Transatlantica*, November 9, 1907.

[17] Landesco, *Organized Crime*, pp. 947–48, 953–54; Kendall, "Who Killa de Chief?" p. 504.

[18] Robert A. Woods, ed., *Americans in Process* (Boston: Houghton Mifflin Co., 1902), pp. 207–9; Abbott, *Immigration Problems in Massachusetts*, pp. 105–6.

trines exerted upon "Southerners" in the United States. Efforts to prevent the immigration of undesirables or to extradite those already in the country by passing laws like the "Residence of Strangers Law" (1902), "Social Defense Law" (1910), and "Italo-Argentine Sanitary Convention" (1913) had little effect.[19]

While southern Italians committed violent crimes in Latin America, neither Brazil nor Argentina exhibited any indication of the Black Hand or any equivalent of Anthony D'Andrea or Al Capone. The principal reason for this difference (as the White Hand society accurately observed in 1908) was that in the United States, and Chicago in particular, "social and political conditions are so favorable to the rise and spread of crime."[20]

Chicago displayed, and continued into the twentieth century to possess, many characteristics of a frontier town, some commendable and others execrable. It was vigorous, brash, lusty, optimistic, energetic. It also contained labor violence, corruption in civic and business affairs, apathy toward poverty, inadequate housing, unsanitary living conditions, vice, and organized crime. According to English journalist William T. Stead, who observed conditions in the city during a five months' stay (October, 1893, to March, 1894), "The first impression which a stranger receives on arriving in Chicago is that of the dirt, the danger, and the inconvenience of the streets." He found Chicagoans equally indifferent to the plight of the poor, corruption in business and politics, and to Christian precepts. He concluded that Chicagoans recognized one common bond: money. "The quest of the almighty dollar is their Holy Grail." Lincoln Steffens also characterized Chicago: "First in violence, deepest in dirt, loud, lawless, unlovely, ill-smelling, irreverent, new; an overgrown gawk of a village, the 'tough' among cities, a spectacle for the nation." Into this environment, with its unparalleled economic opportunities, optimism, mobility, corruption, filth, and violence, arrived increasing numbers of southern Italian and Sicilian immigrants in the years after 1880.[21]

Many Chicago policemen worked illegally and in close harmony with criminals and politicians. Models of success in the city's immigrant communities often turned out to be (as Chicago's few but vociferous socialists claimed and as the White Hand society found to its distress) corrupt politicians and lawbreakers who operated under the protection of the guardians of the law. They displayed all the outward signs of economic achievement: expensive clothes and cars, quantities of ready money, food and drink of excellent quality. They also commanded attention, often respect, from the American community and the Italian-language newspapers. Socialists blamed the Italian-language press, "with few exceptions," for permitting itself to be "the main lever for the elevation of these rascals" and "an accessory to all the disgraceful things befalling our colonies."[22]

Socialists might well have had Anthony D'Andrea in mind, for he exhibited char-

[19] Robert J. Alexander, *Labor Relations in Argentina, Brazil and Chile* (New York: McGraw-Hill Book Co., 1962), pp. 147–49, 162–65, and *Communism in Latin America* (New Brunswick, N.J.: Rutgers University Press, 1957), pp. 155; Robert D. Ochs, "A History of Argentine Immigration, 1853–1924," unpublished Ph.D. dissertation, University of Illinois, 1939, pp. 149–57; James Bryce, *South America, Observations and Impressions* (new ed.; New York: Macmillan Co., 1914), p. 320.

[20] Charles Wagley, *An Introduction to Brazil* (New York: Columbia University Press, 1963), pp. 181–83; Thomas F. McGann, *Argentina, the United States and the Inter-American System* (Cambridge, Mass.: Harvard University Press, 1957), p. 193; Robert E. Foerster, *The Italian Emigration of Our Times* (Cambridge, Mass.: Harvard University Press, 1919), pp. 439–40.

[21] William T. Stead, *If Christ Came to Chicago* (New York: Living Books, Inc., 1964), pp. 187, 24–25, 51, 74, 110, 123, 172, 175–76; Lincoln Steffens, *The Shame of the Cities* (New York: McClure, Phillips & Co., 1904), p. 163.

[22] John Landesco, "Crime and the Failure of Institutions in Chicago's Immigrant Areas," *Journal of the American Institute of Criminal Law and Criminology*, XXIII (July–August, 1932), 224; *La Parola dei Socialisti*, March 28, 1914.

acteristics decried by the socialist press as typical of Italian-community leaders. He (and, during the 1920's, other presidents of the Unione Siciliana) commonly associated with criminal friends and businessmen, involved himself in gangland activities and rivalries, and held a criminal record. Before his death, D'Andrea had been not only president of the Unione, but also president of a labor union and candidate for political office. He and those like him gained leadership of the Unione and other societies through their own personal power and connections. They aspired to such offices because of the prestige and influence that accompanied these positions, including some measure of political power through control of members' votes on election days. On the other hand, members of societies willingly or knowingly elevated men of questionable reputation to positions of authority, possibly through hope of financial reward or fear of personal injury.[23]

For many Chicagoans, however, poverty was the most degrading and deplorable crime. Dishonesty, bribery, and misuse of power simply provided ready methods of overcoming lack of money. Native-born as well as immigrant residents of the city held this view, as concerned Chicagoan Samuel Paynter Wilson observed in 1910. Conditions in Chicago favored many types of lawbreaking, and the Black Hand flourished in the city for a number of years.[24]

Black Hand activities in Chicago virtually disappeared in the 1920's. At least three factors caused this decline. First, the supply of simple, pliable victims dwindled soon after the termination of immigration in the years following 1914. Action by federal authorities made up a second factor. Enforcement of laws prohibiting use of the mails to defraud forced personal delivery of threatening Black Hand notes, potentially a dangerous activity because of possibilities of being recognized.[25]

While the preceding two factors limited opportunities for criminals in the ethnic colony, a vast new field of endeavor had presented itself in the American community because of new federal laws prohibiting the manufacture and sale of alcoholic beverages. American drinking tastes and capacities did not adjust to the new regulations, and enterprising young men found themselves in a position to reap immense profits. Many American robbers, murderers, blackmailers, burglars, and thieves left their former fields of labor for the more lucrative work offered through Prohibition and for which their former professions had provided good training. It is likely that many well-qualified Blackhanders also forsook the less profitable extortion rackets of the Italian quarter in order to join the liquor-traffic scramble. Thus immigration restriction, along with Prohibition, marked the end of the Black Hand era and introduced many who had been involved in crime within the Italian community into the mainstream of American gangsterdom.[26]

IV

Black Hand activities—mainly extortion and terrorism—received most publicity inside and outside the immigrant community, but many other types of crimes existed within the colony. Over the years Chicago's Italian-language newspapers described a wide range of wrongdoings not limited to the Black Hand. These included padrone (or labor agent) abuses and the illegalities of immigrant bankers, among others.

Labor agents prospered in Chicago because of the city's prominence as a trans-

[23] *L'Italia*, October 12, 1913; November 2, 1919.

[24] Samuel Paynter Wilson, *Chicago and Its Cess-pools of Infamy* (11th ed.; Chicago: N.P. [1910?]), p. 29.

[25] Landesco, *Organized Crime*, pp. 946–47. Harvey Warren Zorbaugh, *The Gold Coast and the Slum: A Sociological Study of Chicago's Near North Side* (Chicago: University of Chicago Press, 1929, pp. 170–74), described Black Hand activities in the "Little Sicily" colony on the Near North Side. Zorbaugh does not indicate dates, but the sources he cited described the period before 1920.

[26] John Landesco, "Prohibition and Crime," *Annals of the American Academy of Political and Social Sciences*, CLXIII (September, 1932), 125.

portation center. While they fulfilled a vital function for newly arrived immigrants as well as for American businessmen, padroni generally did so by cheating and shortchanging their less knowledgeable, more gullible compatriots until effective safeguards frustrated or hampered their efforts.[27]

Immigrant bankers flourished in every part of the country where newcomers from southern and eastern Europe had gathered in any considerable numbers. The principal financial transaction of these bankers consisted of receiving deposits and sending money abroad. Most also served as steamship ticket agents, and often conducted some other business as well. Hence some "banks" could be found in grocery stores, saloons, or other natural gathering places. Investigators of the Immigrants' Protective League visited fifty-five Chicago bankers of Italian birth or extraction in the spring of 1912. Of these, thirty-five were also steamship ticket agents, fifteen were labor agents, eleven operated grocery stores, four ran drugstores, two owned saloons, one owned a barber shop, and one owned a real estate office.[28]

The typical banker had little experience in business methods, operated with a minimal accumulation of capital, and for the most part worked freely outside legal controls. Immigrants bankers were not, for example, restricted as to the kinds of investments they could make with money deposited (restrictions which applied to state and national banks). As a result, speculative ventures of private bankers often ended in disaster, particularly for immigrant depositors; since they had no adequate legal safeguards, they lost their savings. Immigrants who transmitted money to Italy provided bankers with lush opportunities to defraud, either by using the money for investment purposes with the intention of sending some of it overseas when investments paid off, or else by simply accepting the money and keeping it. Because of the conditions under which they operated, many immigrant bankers went out of business, some through inefficient business methods, others because they had absconded with customers' savings.[29]

The last two decades of the nineteenth century provided Italian bankers with almost unlimited opportunities for exploiting their customers. Conditions for depositors improved somewhat after the turn of the century. By the Italian Emigration Law of 1901, the Bank of Naples became designated as the financial institution entrusted with transmission of savings from all parts of the world, including the United States. The Bank of Naples, therefore, exerted a strong influence in forcing immigrant bankers to improve services for their clients, modify rates of exchange, and lower charges for transmission of money.[30]

In addition, after 1900 various states which contained large immigrant populations enacted legislation intended to regulate immigrant banking operations. Massachusetts, New York, New Jersey, Pennsylvania, and Ohio passed such laws, which were far less stringent than regulations governing state and national banks. In Illinois, rural private American bankers effectively opposed regulations which might have limited their own free and easy activities. As a result, adequate state legislation controlling immigrant banks and American private banking operations was not enacted or enforced until the 1930's, following the failure of numerous banks, both American and immigrant. Consequently, Italians (and other immigrant bankers) continued

[27] For a detailed examination of padrone methods as well as the factors responsible for the system's decline, see Humbert S. Nelli, "The Italian Padrone System in the United States," *Labor History,* V, No. 2 (Spring, 1964), 153–67.

[28] Immigrants' Protective League, *Annual Report for the Year Ending January 1, 1913,* p. 18.

[29] U.S. Senate, *Immigration Commission,* XXXVII, 203–25; Immigrants' Protective League, *Annual Report, 1910–1911,* pp. 20–21, and *Annual Report for the Year Ending January 1, 1913,* pp. 18–20.

[30] Gino C. Speranza, "What Italians Send Back," *New York Evening Post,* reprinted in *Immigration,* II, No. 10 (March, 1911), 269.

to operate in Illinois with few restrictions throughout the period of large-scale immigration.[31] Legislation on the books did not mean, of course, that police enforced the laws, that illegal activities ceased, or that the native community supported the new statutes. During Prohibition, for example, Americans ignored existing legislation and patronized illegal bars ("speakeasies") operated by Italians and others, and city police did little to halt the liquor business.

Other businessmen in the immigrant colony supplied needed services and products and lined their own pockets by overcharging or otherwise cheating their compatriots. Among these were quack dcotors, shyster lawyers, and merchants who adulterated their wares—storekeepers, for example, who mixed linseed oil with olive oil and sold the mixture as pure Sicilian or Luccan oil. Italian-language journals reported these and many other crimes, inluding robbery, embezzlement and misappropriation of funds, arson, counterfeiting, election fraud, and a number of rackets and sharp practices executed by peddlers, restaurant owners, labor agents, bankers, and community businessmen, all contriving to relieve inexperienced newcomers of their money. In addition, corruption and violence often marked the organization and operation of Italian labor union locals.[32]

The neighborhood gang filled an important social void for ethnic-colony youth. It also served to introduce many boys and young men to lives of crime. Although Italians lived in areas which had high delinquency rates, not every child turned to, or was forced into, a criminal career. Even for youngsters inhabiting slum areas, there existed a number of alternatives to crime.[33] Nevertheless, young Italian-Americans, as Alberto Pecorini noted in 1916, "furnished an alarmingly high percentage of young criminals." He maintained that all immigrant groups, from Irish and German to Italian and Jewish, had passed through the same experience. This juvenile lawbreaking stemmed in large part from the fact that criminal activity offered opportunities for quick and substantial monetary gain and therefore success. Without perhaps understanding the significance of his observation, Durante lamented that "today, not many crimes are committed to avenge the honor of wives, sisters, or children as in the past; most of them are for money."[34]

Despite the wide range of immigrant community illegalities, most "Southerners" led law-abiding lives. The bulk of arrests and convictions involving Italians in Chicago resulted from minor offenses (misdemeanors), particularly violations of city

[31] U.S. Senate, *Immigration Commission*, XXXVII; 280; Immigrants' Protective League, *Annual Report, 1913*, pp. 19–20; Grace Abbott, *The Immigrant and the Community* (New York: Century Co., 1917), chap. iv; *L'Italia*, April 13, 1912; May 5, 1915.

[32] *La Tribuna Italiana Transatlantica*, May 23, 1903; August 16, 1914; *La Parola dei Socialisti*, April 18, 1908; January 4, May 31, 1913; *La Parola Proletaria* (Chicago), August 26, 1916; *L'Avanti!* (Chicago), November 15, 1918; *Il Messaggiere Italo-Americano* (Chicago), July 5, 1888; *L'Italia*, January 28, 1899; February 11, November 5, 1905; December 4, 1909; March 23, April 13, 1912; March 21, 1913; April 26, 1914; May 5, 1915; June 3, 1917; August 4, 1918; November 7 and 23, 1919.

[33] Clifford R. Shaw *et al.*, *Delinquency Areas: A Study of the Geographic Distribution of School Truants, Juvenile Delinquents and Adult Offenders in Chicago* (Chicago: University of Chicago Press, 1929); Clifford R. Shaw and Henry McKay, *Juvenile Delinquency and Urban Areas* (Chicago: University of Chicago Press, 1942); David Matza, *Delinquency and Drift* (New York; John Wiley & Sons, 1964), p. 65; Albert J. Reiss, Jr., and Albert Lewis Rhodes, "The Distribution of Juvenile Delinquency in the Social Class Structure," *American Sociological Review*, XXVI, No. 5 (October, 1961), 729. Also Thorsten Sellin, *Culture Conflict and Crime*, Bulletin 41 (New York: Social Science Research Council, 1938); Robert K. Merton, "Social Structure and Anomie," *American Sociological Review*, III (October, 1938), 672–82; Robert E. Park and Herbert A. Miller, *Old World Traits Transplanted* (New York: Harper & Brothers, 1921), esp. pp. 238–58.

[34] Alberto Pecorini, "The Child of the Immigrant," *La Fiaccola* (New York), August 31, 1916, reprinted from *Il Cittadino;* William Foote Whyte, "Social Organization in the Slums," *American Sociological Review*, VIII (February, 1943), 38; *L'Italia*, April 1, 1911.

ordinances, rather than Black Hand crimes. In 1908, the last year in which the Department of Police *Annual Report* listed city ordinance violations separately from state misdemeanors, Italians committed a total of 1,935 criminal offenses. Felonies accounted for 294, while the other 1,631 offenses were misdemeanors, of which 1,276 involved city ordinances.[35]

The Chicago City Council Committee on Crime, under the chairmanship of Alderman (and University of Chicago political science professor) Charles E. Merriam, presented in March, 1915, a comparison of arrest and conviction statistics for native-born (white and black) and foreign-born groups. The Committee found a considerably greater percentage of arrests and convictions among native groups than their ratio of the population. Foreign-born elements, on the other hand, showed "almost uniformly" smaller percentages than their proportion of the population. Italians exhibited an excess of one-tenth of one per cent in convictions (misdemeanors and felonies combined); the Committee remarked, "This is surely so small as to be negligible!"[36]

The professional criminal knew, or had ready access to lawyers who knew, the nuances of the law and how to avoid retribution for antisocial actions. He had money and connections with politicians, as well as with corrupt policemen, who would delay or circumvent efforts to bring him to justice. He also could arrange intimidation of witnesses and victims so that they kept silent.[37] The average immigrant Italian, on the other hand, often faced arrest and severe punishment for violating laws and ordinances about which he, and most newcomers, knew nothing. If the immigrant spoke no English, he labored under an additional disadvantage because most court interpreters lacked knowledge of court proceedings, were unacquainted with the various dialects used by Italian residents of the city, and often had a limited familiarity with the English language. In some courts police officers served as interpreters, in others court officials (usually clerks) translated, while in still others the officials simply "got along the best they could" by picking a bystander who claimed to speak the language. Court decisions based on the work of such interpreters often showed little justice or impartiality.[38]

V

Within the immigrant colony, bankers and padroni, blackhanders and other lawbreakers realized small but important profits by swindling or terrorizing compatriots. Another level of illegal activity involved Italians. Organized crime, business operations reaping vast profits in and from the American community, offered almost limitless opportunities for promotion within its hierarchy.

Chicago's organized crime had its origins in the 1870's with the activities of Michael Cassius McDonald. Except for a brief eclipse during the mayoralty of John A. Roche (1887–89), McDonald controlled virtually all city politics from 1879 until the mid-1890's. Recalling the McDonald era on August 18, 1907 (a few days after his death), the *Chicago Record-Herald* stated that "he never held office, but he ruled the city with an iron hand. He named the men who were to be candidates for election, he elected them, and after they were in office they were merely his slaves." Chi-

[35] City of Chicago, Department of Police, *Report of the General Superintendent of Police of the City of Chicago, to the City Council, for the Fiscal Year Ending December 31, 1908* (Chicago, 1909), p. 34.

[36] Chicago City Council, *Report of the Committee on Crime* (Chicago, 1915), pp. 52–56.

[37] Edith Abbott, "Recent Statistics Relating to Crime in Chicago," *Journal of the American Institute of Criminal Law and Criminology*, XII, No. 3 (November, 1922), 330.

[38] Gino Speranza, "The Relation of the Alien to the Administration of the Civil and Criminal Law," *Journal of the American Institute of Criminal Law and Criminology*, I, No. 4 (November, 1910), 563–72; Kate Holladay Claghorn, *The Immigrant's Day in Court* (New York: Harper, 1923), pp. 205–6, 209–10, 222–23; Grace Abbott, *The Immigrant and the Community*, chap. v.

TABLE 1

NATIVITY OF MALE PERSONS ARRESTED AND CONVICTED FOR FELONIES, 1913

NATIVITY	ARRESTS		CONVICTIONS		PERCENTAGE DISTRIBUTION*
	No.	%	No.	%	
American:					
White..........	5,756	56.3	2,241	56.9	43.1
Colored.........	882	8.6	354	9.0	2.6
Foreign:					
Austrian........	401	3.9	158	4.0	11.2
English.........	166	1.6	79	2.0	5.2
French.........	22	0.2	10	0.3	....
German	815	8.0	366	9.3	12.6
Greek..........	139	1.4	29	0.7	0.6
Hollanders......	19	0.2	8	0.2	0.7
Irish	186	1.8	98	2.5	4.4
Italian	392	3.8	108	2.7	3.2
Russain	1,027	10.0	331	8.4	8.5
Scandinavian....	214	2.1	93	2.4	6.7
Other	218	2.1	64	1.6	1.2
Total foreign..	3,599	35.1	1,344	34.1	54.3
Total......	10,237	100.0	3,939	100.0	100.0

* Of male population of Chicago 21 years of age and older.

SOURCE.—Chicago City Council, *Report of the Committee on Crime* (Chicago, 1915), p. 56.

TABLE 2

NATIVITY OF MALE PERSONS ARRESTED AND CONVICTED FOR MISDEMEANORS, 1913

NATIVITY	ARRESTS		CONVICTIONS		PERCENTAGE DISTRIBUTION*
	No.	%	No.	%	
American:					
White........	50,999	58.5	23,656	59.6	43.1
Colored.......	4,741	5.4	2.179	5.5	2.6
Foreign:					
Austrian......	3,282	3.8	1,492	3.8	11.2
English.....	1,240	1.4	537	1.3	5.2
French........	181	0.2	90	0.2	....
German......	6,942	8.0	2,977	7.5	12.6
Greek........	1,592	1.8	947	2.4	0.6
Hollanders....	209	0.3	115	0.3	0.7
Irish.........	2,354	2.7	901	2.3	4.4
Italian........	2,972	3.4	1,333	3.4	3.2
Russian......	7,519	8.6	3,314	8.3	8.5
Scandinavian..	2,857	3.3	1,330	3.3	6.7
Other........	2,268	2.6	819	2.1	1.2
Total foreign	31,416	36.1	13,855	34.9	54.3
Total....	87,156	100.0	39,690	100.0	100.0

* Of male population of Chicago 21 years of age and older.

SOURCE.—Chicago City Council, *Report of the Committee on Crime* (Chicago, 1915), p. 56.

cago historian Bessie Louise Pierce described him as "virtually the dictator of the City Hall." With his political power established following Carter Henry Harrison's mayoral success in 1879, "Mike" McDonald turned to organizing the city's first "syndicate," which centered on gambling. During his reign McDonald, as political party boss and head of the "syndicate," forged the powerful and highly effective coalition of criminals, politicians, and compliant policemen which the Torrio-Capone organization utilized so successfully during the 1920's.[39]

Following the assassination of Mayor Harrison in October, 1893, McDonald's star began to fade, and a number of new independent organizations or "syndicates" formed. By 1907, when "Mike" died, various spheres of interest controlled gambling in Chicago. Mont Tennes gave orders on the North Side, James O'Leary on the South Side, and Alderman Johnnie Rogers on the West Side. First ward political bosses "Hinky Dink" Kenna and "Bathhouse John" Coughlin, early patrons of "Diamond Jim" Colosimo, presided over gambling in the Loop.[40]

After years of gang warfare most of McDonald's former gambling empire centered in the hands of Mont Tennes, who ruled in turn until the 1920's, when the Torrio-Capone group pushed him aside and seized control of gambling as well as other illegal activities in the Chicago area. Although the real entry of Italians into big-time crime, and most of their successes therein, came after 1920—that is, during Prohibition—they did make a strong beginning under Colosimo in the first two decades of the twentieth century.

While still a boy, Colosimo left Calabria for the United States in the 1890's. His first employment in this country, and the work that took him to Chicago, was that of waterboy for a railroad section gang, at the age of sixteen. Before the turn of the century Colosimo became a street sweeper and established his residence in the first ward. Colosimo organized his fellow street sweepers (or "white wings") who resided in the first ward into a political bloc which he led as a unit to the polling station at election time in order to vote for candidates supported by the Democratic machine. This action quickly brought him to the attention of Aldermen Kenna and Coughlin, who had dominated first ward politics since 1893. They made "Big Jim" a precinct captain, and he delivered the vote so successfully for his patrons that before his death he rivaled Kenna and Coughlin as a political power in the ward.[41]

In the meantime, Colosimo and his wife, Victoria Moresco, acquired a string of brothels and saloons as well as a legitimate business, a nationally known restaurant called "Colosimo's Cafe." (Socially prominent Chicagoans patronized it along with theater and opera personalities, including George M. Cohan and Enrico Caruso.) By 1914 "Big Jim" and associates had built a "syndicate," the first one organized by Chicago Italians, based on vice operations, especially prostitution and gambling. In that year State's Attorney Maclay Hoyne launched a grand jury investigation of vice in Chicago's first ward and found the existence of three vice rings or "syndicates." The first and largest consisted of the Colosimo-Torrio outfit, headed by "Big Jim" and his chief lieutenant, Johnny Torrio.[42]

Black Hand extortion threats had

[39] Bessie Louise Pierce, *A History of Chicago* (New York: Alfred A. Knopf, 1957), III, 302–5; Herbert Asbury, *Gem of the Prairie: An Informal History of the Chicago Underworld* (New York: Alfred A. Knopf, 1940), pp. 142 ff.; Alson J. Smith, *Syndicate City: The Chicago Crime Cartel and What To Do About It* (Chicago: Henry Regnery Co., 1954), pp. 25–30.

[40] Landesco, *Organized Crime*, chap. xix, "Tennes as Vice Chief"; Virgil W. Peterson, *Barbarians in Our Midst, A History of Chicago Crime and Politics* (Boston: Little, Brown & Co., 1952), pp. 84–91.

[41] Fred D. Pasley, *Al Capone: The Biography of a Self-Made Man* (Garden City, New York: Garden City Publishing Co., 1939), pp. 9–14; Emmett Dedmon, *Fabulous Chicago* (New York: Random House, Inc., 1953), pp. 289–90.

[42] Landesco, *Organized Crime*, pp. 850–57.

prompted Colosimo to bring Torrio from New York in 1909 for personal protection. Within five years, effective and ruthless methods had advanced Torrio to second-in-command and a power in his own right through his organization of gambling in the city and some nearby suburbs. (At this time Torrio resided in the same building as "Big Jim," 101 West 21st Street). In 1919 Henry Barrett Chamberlain, Operating Director of the Chicago Crime Commission, described increasingly efficient "syndicate" crime: "Modern crime, like modern business, is tending toward centralization, organization, and commercialization. Ours is a business nation. Our criminals apply business methods. . . . The men and women of evil have formed trusts."[43]

When the National Prohibition Enforcement Act (the Volstead Act) went into effect on January 16, 1920, the Colosimo-Torrio organization expanded from prostitution and gambling into an even more lucrative business, the manufacture and distribution of liquor. Colosimo did not live long enough to reap the full financial harvest of his bootlegging ventures, for he was murdered on May 11, 1920.[44]

After Colosimo's death, Johnny Torrio succeeded to the first ward based Italian "syndicate" throne, which he occupied until his retirement in 1924. An able and effective leader, Torrio excelled as a master strategist and organizer and quickly built an empire which far exceeded that of his predecessor in wealth, power, and influence. He received invaluable aid from his chief lieutenant, Alphonse Capone, who in turn ruled the empire. Italian domination of Chicago's organized crime had made a strong beginning.[45]

VI

Several factors facilitated Italian successes in the highly competitive (and deadly) business of organized crime. Some related to old world backgrounds and characteristics, others to conditions and opportunities in the host society. Mafia expert Michele Pantaleone has presented a brilliant exposé of the Sicilian Mafia in a recent book. His comments on Sicilians in American crime, however, raise more questions than they answer. According to Pantaleone, men schooled in the Sicilian Mafia "founded and organized gangsterdom in America fifty years ago"—that is, in the decade following 1910. These criminals operated in "St. Louis, Chicago, Kansas City, Detroit, New Orleans, New Jersey [*sic*] and other cities." Boston and Milwaukee are significantly absent from this list. Also pointedly missing are, first, any indication that "organized gangsterdoms" existed in Chicago, New York, and other cities long before 1910, and, second, the fact that when an Italian "syndicate" developed after 1910 in competition with organizations composed of members of other ethnic groups, it was not staffed solely by Sicilians or even entirely by Italians, any more than Irish, Jewish, or Polish criminal organizations were made up only of members of these groups. Thus many alleged *capo mafiosi* (leaders) of Chicago's Italian "syndicate" had no Sicilian ancestry, while others born in Sicily emigrated as children and grew to maturity in urban America. James Colosimo, for example, was born in mainland Italy (Calabria) and arrived in Chicago as a boy; Naples-born John Torrio grew up in Brooklyn; Al Capone was born in Brooklyn of Neapolitan parents; while Jake Gusick (or Jack Cusick), the "so-called brains of the Capone organization," was only one of many non-Italian members of the "American Mafia hierarchy."[46]

[43] Henry Barrett Chamberlain, "Crime as a Business in Chicago," *Bulletin of the Chicago Crime Commission*, No. 6 (October 1, 1919), p. 1.

[44] *Chicago Daily News*, May 12, 1920.

[45] Lloyd Wendt and Herman Kogan, *Lords of the Levee: The Story of Bathhouse John and Hinky Dink* (Indianapolis: Bobbs-Merrill, 1943), pp. 430–42; Virgil W. Peterson, "Chicago: Shades of Capone," *Annals of the American Academy of Political and Social Sciences*, CCCXVII (May, 1963), 31–32.

[46] Michele Pantaleone, *The Mafia and Politics* (London: Chatto & Windus, 1966), pp. 38–39 (also see the Preface to the English edition written by

More significant than the Sicilian origin of some criminals were the ethnic cohesion and group loyalty exhibited by the Italian criminal element in its struggle to dominate Chicago's organized crime. The Sicilian or southern Italian origin of the bulk of the city's Italians included a background of clan traditions held to be sacred from "outside" interference. Doubtless these traditions and connections provided a ready-made nucleus for criminal gangs in the new world and insured a fierce loyalty to the group's laws and rituals rather than to "outside" rules and regulations, as some writers on Italian-American criminality have pointed out.[47]

To a southern Italian or Sicilian living in the Kingdom of Italy during the period from 1890 to 1920, however, the concept of *paesi* ("homeland") meant village of birth, outside of which existed nothing but strangers. Residents of other towns and provinces, regarded as foreigners, became objects of suspicion or contempt. Political scientist Edward C. Banfield, in a recent study widely accepted by immigration scholars and students of Italian history, described a Sicilian-southern Italian society dominated by amoral familism. In the homeland Banfield found peasants and gentry alike unable to act "for any end transcending the immediate, material interests of the nuclear family." If this study is accurate, one can reasonably conclude that community and group consciousness among "Southerners" in the United States did not cross the Atlantic, but developed in the new world.[48]

In America this expanded sense of loyalty and patriotism manifested itself in celebrations supporting the Italian Kingdom, membership in mutual aid and fraternal societies based on regional or national groupings, and support of an "Italian" press and Italian Catholic parishes. In the new homeland, therefore, village loyalties grew to become national loyalties, and members of the ethnic group came to look upon non-Italians, rather than people from outside a particular village in Italy, as "outsiders." "American" Italians (those born or raised in the United States) who established and operated the Chicago Italian "syndicate" excluded members of other ethnic groups until they had proven themselves worthy of acceptance. Conditions in Chicago offered fertile ground for the activities of outlaws (a similar situation existed also in New York City). Long before "Southerners" arrived, gangs composed predominantly of other ethnic groups—Irish, German, Scandinavian—operated openly and often under the protection of the law and elected officials. This "favorable" situation could not be found in all urban areas to which Italians migrated, with the significant result that members of the second generation were prominently involved in organized crime in some cities and not in others. Hence local conditions permitting or even encouraging lawlessness clearly occupied a place of basic importance in Italian successes within Chicago's organized crime.[49]

Chicago attracted professional Italian crooks, generally from Sicily, because of opportunities available to extort money from fellow Sicilians, unimpeded by police,

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Denis Mack Smith, pp. 17–18); Peterson, *Barbarians in Our Midst*, p. 135; Frederic Sondern, Jr., *Brotherhood of Evil* (New York: Farrar, Strauss, & Cudahy, 1959), p. 69.

[47] Among others, Sondern, *Brotherhood of Evil*, p. 66; Ed Reid, *Mafia* (rev. ed., New York: New American Library, 1964), chap. ii; Rudolph J. Vecoli, "Contadini in Chicago: A Critique of *The Uprooted*," *Journal of American History*, LI (December, 1964), 409.

[48] Edward C. Banfield, *The Moral Basis of a Backward Society* (Glencoe, Ill.; Free Press, 1958), p. 10. Among others who accept the Banfield position are Herbert J. Gans, *The Urban Villagers: Group and Class in the Life of Italian-Americans* (New York: Free Press, 1962), p. 203; and Norman Kogan, *The Politics of Italian Foreign Policy* (New York: Frederick A. Praeger, 1963), pp. 4–5.

[49] Harold Ross, "Crime and the Native Born Sons of European Immigrants," *Journal of the American Institute of Criminal Law and Criminology*, XXVIII (March–April, 1938), 202–9. Significantly, Italian residents of Latin American countries apparently indicated no propensity to indulge in crime for profit.

under the magic name "Black Hand."[50] In the Illinois Crime Survey, John Landesco reported the results of a study of more than 300 crimes attributed to the Black Hand. He found that these crimes were "limited almost entirely to the Sicilian neighborhoods" of Chicago. Writing in 1909, Arthur Woods, Deputy Police Commissioner of New York City, reported a similar situation. New York police found that "in almost every case" Black Hand crimes were committed by men who had been involved in criminal activities in Italy and who had immigrated to New York in order to continue "fattening off the main body of their fellow-countrymen." In this period, such criminals were not, however, members of a tightly organized, highly centralized structure. "The Black Hand is not a cohesive, comprehensive society, working with mysterious signs and passwords," maintained Woods. "Given a number of Italians with money, and two or three ex-convicts, you have all the elements necessary for a first-rate Black Hand campaign." Landesco reached a similar conclusion concerning the Black Hand in Chicago but went a step further to conclude, "It is the purest banality to excuse the nefarious, bloody practices and wide-spread tribute paid by the victims, that the historical explanation that blackmail and the conspiracy of silence are old-world traits transplanted." Rather, the Black Hand's success in Chicago grew out of local conditions which "have favored the rise, spread and persistence of extortion by violence" even among groups "which did not import the pattern" from the old world. Undoubtedly many Sicilian Blackhanders moved into the "syndicate," but "American" Italians provided the leadership.[51]

Chicago's Italian-community youngsters sought economic mobility through many channels, including crime. Children raised in "delinquency areas" could and did turn to work in legitimate lines and found success in commercial, trade, and professional occupations. Nevertheless, illegal activities appeared to offer the quickest means available for monetary gain. Many immigrant-community youngsters early realized that in the highly competitive, mobile, and rootless society in which they lived, achievement, measured in financial terms, had high value, while the methods used to realize success received little attention or condemnation.[52]

Chicago's early Italian community, composed largely of northern Italians, took little interest in, and avoided contact with, "Southerners." The bulk of Chicago's southern Italians, especially those from Sicily, arrived in the city in the years between 1900 and 1914.

Children of "Southern" immigrants arrived at maturity at a time when educational requirements were becoming ever more important in order to qualify for jobs in public as well as in private employment. The fluid political, social, and financial patterns which had aided (and in fact had speeded) the assimilation of Germans, Irish, Bohemians, Poles, Swedes, and Norwegians in the three decades following 1870, were in the process of growing more rigid and unyielding as Chicago slowly grew out of its frontier-town stage and as earlier immigrant elements became entrenched. These earlier arrivals sought to establish barriers to prevent later arrivals

[50] The Italian Kingdom encouraged criminals to leave the country. Many eventually settled in American cities. This was not a process unique to Italians; it had existed with other, and earlier, groups. In an article published in 1909, entitled "Immigrants and Crime," p. 118, William S. Bennet, Congressman from New York and a member of the U.S. Immigration Commission, pointed out that "thirty years ago societies were actually organized for the purpose of sending to this country criminals, paupers, old people, and the class that we call unfortunate women. . . . That was all before the South European immigration had started, and from countries from which the very best of our immigrants, according to the universal acceptation, have come."

[51] Landesco, *Organized Crime*, pp. 937, 947–48; Arthur Woods, "The Problem of the Black Hand," *McClure's Magazine*, XXXIII (May, 1909), 40.

[52] Landesco, "Prohibition and Crime," p. 124, and "Crime and the Failure of Institutions," p. 240.

from other ethnic backgrounds from challenging their positions.[53]

"Southerners" realized some success in gaining patronage jobs, but they were, as one early Italian politician bitterly complained, "the dollar-and-a-half-a-day 'jobs' of sweeping the streets" while Irish aldermen gave "the-four-dollar-a-day 'jobs' of sitting in an office to Irishmen." If at all possible, ethnic-group members tried to "look after their own," whether in politics, crime, organized labor, or the professions. (Early-arriving northern Italians did not consider "Southerners" to be "of their own.") Thus Anton J. Cermak, whether in his capacity as alderman, president of the Cook County Board of Commissioners, or mayor, made sure that his fellow Bohemians (Czechs) received patronage jobs and also support when they tried for elective office. Lacking established "patrons," southern Italians did not move up the political ladder of success in Chicago—to appointive and elective offices, patronage jobs, and exemptions from the law—until the 1920's. Then, under the leadership and guidance of Johnny Torrio and Al Capone, "Southerners" found politics and its urban handmaiden, crime, to offer an increasingly important source of money, as well as a means of social mobility.[54]

At the time when "American" Italians were arriving at maturity only to find economic advancement made difficult (but not impossible) by inadequate education, social and ethnic background, and lack of political connections, a new field of endeavor appeared, requiring as qualifications only ambition, ruthlessness, and loyalty. Prohibition provided a powerful and attractive force with fast, lush financial opportunities which appealed to many qualified young men—and which they regarded as nothing more than a new form of business enterprise. "Prohibition is a business," Al Capone stoutly maintained. "All I do is to supply a public demand. I do it in the best and least harmful way I can." He and others like him undoubtedly convinced themselves that they were simply labor and distribution organizers in a strongly competitive field. Criminals could persuade themselves of their value to the business world because the liquor trade seemed to provide a service which rich as well as poor urban dwellers desperately wanted and

TABLE 3

FOREIGN-BORN POPULATION OF CHICAGO, BY COUNTRY OF ORIGIN

Nativity	1884*	1898†	1914‡
Germany........	209,631	207,310	191,168
Ireland..........	114,005	104,354	68,305
Norway.........	18,292	22,248	27,562
Sweden..........	23,755	56,862	66,287
Bohemia.........	28,281	40,516	49,074
Poland..........	23,509	44,325	124,543
Italy............	4,091	12,585	58,782

* City of Chicago, Board of Education, *School Census of the City of Chicago. Taken May, 1884. Total Population of the City. Over 21 Years and under 21 Years of Age. By Wards and By Divisions of the City* (Chicago, 1884), p. 31.

† "School Census of 1898," printed in City of Chicago, Board of Education, *Proceedings, July 13, 1898 to June 28, 1899* (Chicago, 1899), pp. 254–58.

‡ City of Chicago, Board of Education, *School Census of 1914 and Annual Report of the Superintendent of Compulsory Education* (Chicago, 1914), p. 6.

[53] Richard G. Ford, "Population Succession in Chicago," *American Journal of Sociology*, LVI (September, 1950), 160.

[54] Jane Addams, *Democracy and Social Ethics* (New York: Macmillan Co., 1902), p. 259; Alex Gottfried, *Boss Cermak of Chicago: A Study of Political Leadership* (Seattle: University of Washington Press, 1962), pp. 68–70, 86, 268–69.

willingly paid for. Prohibition "opened up a new criminal occupation, with less risk of punishment, with more certainty of gain, and with less social stigma than the usual forms of crime like robbery, burglary, and larceny."[55]

During the 1920's, the Torrio-Capone "syndicate" fought other organizations for control of the liquor business in the Chicago area. The composition of gangland organizations was based largely, but not entirely, on ethnic origin. Thus, not only did the Capone group contain Slavic, Jewish, Irish, and other non-Italian elements; but Italians, foremost among them "Schemer" Drucci and the nine Aiello brothers, worked for Capone's major rival, the North Side gang led by Barney Bertoche, Dion O'Banion, Hymie Weiss, and George "Bugs" Moran. More than 500 gangland murders took place during the decade as competing organizations attempted to eliminate rivals, not always by superior management techniques. By 1930, according to a careful study of 108 kingpins of organized crime in Chicago prepared by William F. Ogburn and Clark Tibbits, "American" Italians constituted 31 per cent of the leadership group, while 29 per cent came of Irish background and 20 per cent were Jewish.[56]

Each group reaped financial returns, and found itself pushed aside by a new, ambitious ethnic element, eager in its turn to gain economic benefits to be realized from crime and corrupt politics. In his collection of readings about organized crime in America, Gus Tyler maintained that Italians at first operated "almost exclusively" within the Italian colony. "Then they entered into warfare against the other ethnic gangs. Then they merged. And then they became the dominant element in the merger." The last of the large masses of "white" ethnic groups, Italians, however, remained in power at the top of the crime syndicate because no new ethnic mass came along to force them out of their positions of leadership. Perhaps the logical successors, Negroes, have been prevented from exerting pressure in large part because of segregation and color. Not only do Negroes lack a tradition of co-operative group action, but residential segregation and color render attempts to move illegal activities out of the ghetto community and into the larger white society extremely hazardous. Perhaps group awareness growing out of civil rights movements or the Black Power movement, along with society's increasing emphasis on equality regardless of color, will encourage Negroes to demand a place in the hierarchy of organized crime in positions other than "soldiers," as they are now employed.[57]

VII

Chicago's Italians enjoyed great success in criminal activities, whether within the immigrant community through Black Hand and other extortion methods or by "syndicate" techniques in the larger American environment. The Black Hand existed, but as a method of criminal action, a modus operandi, and not a formal organization. As the White Hand society pointed out in 1908, many other Italian community residents who did not see themselves as Black Hand types, among them bankers, interpreters, labor agents, and small businessmen, worked diligently to extort fellow immigrants at every opportunity. They em-

[55] Edward Dean Sullivan, *Chicago Surrenders* (New York: Vanguard Press, 1930), p. 205; Landesco, "Prohibition and Crime," p. 125; Andrew Sinclair, *Era of Excess: A Social History of the Prohibition Movement* (New York: Atlantic Monthly Press, 1964), p. 220.

[56] Edward Dean Sullivan, *Rattling the Cup on Chicago Crime* (New York: Vanguard Press, 1929), especially chaps. iv, v, ix, and xviii; Denis Tilden Lynch, *Criminals and Politicians* (New York: Macmillan Co., 1932), chaps. vi and x; William F. Ogburn and Clark Tibbitts, "A Memorandum on the Nativity of Certain Criminal Classes Engaged in Organized Crime, and of Certain Related Criminal and Non-Criminal Groups in Chicago," p. 9, unpublished manuscript dated July 30, 1930, in the Charles E. Merriam Papers, University of Chicago Library.

[57] Gus Tyler, ed., *Organized Crime in America: A Book of Readings* (Ann Arbor: University of Michigan Press, 1962), p. 336.

ployed more subtle and refined methods than did Black Hand thugs, "but they too are means of exacting tribute from the sweat of the Italian working man, who pays because of his ignorance, and because of the absolute lack of any assistance and protection in a foreign land."[58]

Profits gained through Black Hand methods in the immigrant community seemed small when compared with the lucrative returns won by "syndicate" gangsters from Prohibition law violations, gambling, prostitution, and dope peddling. Italians born or raised in Chicago quickly learned to adapt American business techniques to crime by organizing compatriots (and acceptable "outsiders"), centralizing the chain of command, and controlling and directing the flow of money. In this process of "Americanizing" Italian criminal activities, "American" Italians established the Italian "syndicate."

Crime even facilitated immigrant adjustment. The "syndicate" required the repression of lingering old-world prejudices against fellow members from other provinces or countries because of the overriding importance of co-operation in the common quest for money, the symbol and substance of success. In the new urban home—cynical, cruel, vulgar, avaricious Chicago—Sicilians and Italians, northern as well as southern, joined together for mutual benefit and profit. Because of its function as a means of economic betterment and social mobility, crime occupied a place in the acculturation of Italian immigrants to the United States, along with community institutions, education, the padrone system, and politics.

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[58] Italian "White Hand" Society, *Studies, Action and Results*, p. 27; Landesco, *Organized Crime*, p. 937.

Working Wives and Marriage Happiness

Susan R. Orden and Norman M. Bradburn

ABSTRACT

A woman's freedom to choose among alternative life styles is an important predictor of happiness in marriage. Both partners are lower in marriage happiness if the wife participates in the labor market out of economic necessity than if she participates by choice. This finding holds across educational levels, stages in the life cycle, and part-time and full-time employment. Among the less educated, the strain comes from an increase in tensions for husbands and a decline in sociability for wives; while among the better educated, husbands and wives both experience an increase in tensions and a decrease in sociability. A woman's choice of the labor market over the home market strains the marriage only when there are preschool children in the family. At other stages in the life cycle, the choice between the labor market and the home market makes little difference in an individual's assessment of his own marriage happiness. However, the labor market choice is generally associated with a higher balance between satisfactions and tensions for both husbands and wives.

There have been dramatic changes in the participation of married women in the labor force in the United States. The rate of participation more than doubled from 1900 to 1940 and then doubled again from 1940 to 1960.[1] "In 1962, there were 23,000,000 women in the labor force and the forecast for 1970 is 30,000,000. Approximately three out of five women workers are married. Among married women one in three is working; among non-white almost one in two. Many of these women, nearly a third, work part time; three-fifths of all part-time work is done by married women."[2]

This paper deals with the effects of a woman's work status on her marriage and suggests that a woman's freedom to choose among alternative life styles is an important predictor of her own and her husband's happiness in marriage.

PREVIOUS RESEARCH

Other investigators have not disclosed any statistically significant relationships between a woman's work status and her adjustment in marriage.[3] They have found only some tendency for employment to increase the conflict a woman encounters in her relationship with her husband. Nye suggests that the net adverse effect of employment on marriage is less in the higher socioeconomic families than in the lower. Blood, on the other hand, suggests that there is some positive evaluation of marriage associated with the wife's employment for lower-income families and some negative evaluation when the husband's income is high. Feld reports no difference in evaluations of either marital problems or marital unhappiness between employed

[1] Glen G. Cain, *Married Women in the Labor Force: An Economic Analysis* (Chicago: University of Chicago Press, 1966).

[2] U.S. President's Commission on the Status of Women, *American Women* (Washington, D.C.: Government Printing Office, 1963), p. 27.

[3] Harvey J. Locke and Muriel MacKeprang, "Marital Adjustment of the Employed Wife," *American Journal of Sociology*, LIV (May, 1949), 536–39; Robert O. Blood, Jr., and Donald M. Wolfe, *Husbands and Wives: The Dynamics of Married Living* (Glencoe, Ill.: Free Press, 1960); F. Ivan Nye, "Marital Interaction," in F. Ivan Nye and Lois W. Hoffman (eds.), *The Employed Mother in America* (Chicago: Rand McNally & Co., 1963); Robert O. Blood, Jr., "The Husband-Wife Relationship," in Nye and Hoffman, *op. cit.*; and Sheila Feld, "Feelings of Adjustment," in Nye and Hoffman, *op. cit.*

and non-employed mothers and concludes that the woman's employment status is not an important variable in accounting for adjustment in marriage. Blood recommends that a more fruitful approach for future research would be to specify more precisely the conditions under which employment has positive or negative effects.

There are several important gaps in previous research. First, the husband has been completely neglected. Earlier studies have been based exclusively on interviews with women. The impact of a woman's participation in the labor market may be significantly different for the husband than it is for the wife.

Second, the concept of economic necessity used in previous studies is, at best, as Blood describes it, a "slippery term in an advertising-saturated culture."[4] While it is undoubtedly true, in general, that the need to work is related to income, previous research measuring need by husband's income alone ignores qualitative differences in the perception of need at different class levels. The middle-class woman's "need" to supplement her husband's income in order to acquire the numerous accoutrements of the good life may be as real to her as the lower-class woman's need to meet her family's basic requirements for food, clothing, and shelter.

Finally, the measures of marital adjustment in previous investigations have ignored the positive side of the marriage—the companionship and sociability that husbands and wives enjoy together. The measures of adjustment in these studies have dealt primarily with over-all evaluation and with the negative side of the marriage—the conflict or tension encountered in the relationship. Blood points out that no information exists on what he and Wolfe consider the purest form of companionship: "going out together just to have a good time."[5] It is not at all clear whether a woman's contribution to the family income increases the amount of money which can be budgeted for social activities or whether employment drains the woman's physical and psychic energies, making her content to join her husband with the proverbial pipe and slippers.

[4] Blood, *op. cit.*, p. 284.

[5] *Ibid.*, p. 296.

PLAN OF ANALYSIS

This paper proposes to fill in some of the gaps in our knowledge of the relation between a wife's employment and marriage happiness. First, our sample includes husbands. There are 781 husbands and 957 wives, or a total of 1,738 married respondents. This analysis is concerned with 1,651 of these respondents who report the husband as the chief wage earner.[6] The men and women are not couples; but, since they both were selected on a probability basis, it seems reasonable to assume that, on the average, the distribution of responses is the same as it would have been if they had been couples.

Second, we distinguish between women who are impelled into the labor force by perceived economic necessity and those who enter the labor force by choice. This distinction is based on responses to the following question: "Would you [or your wife, if the respondent is a man] work if you [or she] didn't need the money?" As we shall see, the distinction between wives who are reluctant recruits to the labor force and those who enter the labor market as a preferred alternative has important consequences for the analysis of marriage happiness.

Ideally, it would be desirable to make a comparable distinction between women who choose to center their activities in the home and those who are there reluctantly. However, in the absence of data to make this distinction, we consider these women as a single group. We assume that, in general, women who are in the home are there by choice. In periods of high levels of eco-

[6] Out of the original sample of 1,738 married respondents, we have excluded eighty-seven deviant cases: wives who are chief wage earners, men who are not chief wage earners, retired respondents, persons on pension or on relief, and respondents for whom we have incomplete data.

nomic activity and relatively low unemployment, it seems reasonable to assume that women who choose to work will generally find employment opportunities and make the necessary arrangements. The year 1963, when our study was in the field, was a year of high economic activity. The gross national product was $583.9 billion, an increase of $27.7 billion over the 1962 level; the unemployment rate for women was 6.5 per cent compared with 6.2 per cent in 1962; factory sales of passenger cars totaled 7,638,000, an increase of 10 per cent over 1962 levels.[7]

We recognize that the assumption that married women generally have free access to the labor force in a full-employment economy is an oversimplification. On the supply side, there are undoubtedly restraints to freedom of entry imposed by the woman's education, her stage in the life cycle, her own and her husband's attitudes toward "working wives," the availability of adequate substitute help, and her own personality disposition, needs, and desires. On the demand side, there may be restraints to free entry imposed by employers as well as by male incumbents in the labor force.[8] To the extent that wives in the home include reluctant recruits who would prefer to be in the labor market, they would tend to understate the marriage adjustment of the group as a whole.

The third addition this study makes to earlier research is to correct for omission of the positive side of the marriage. The analysis will use a conceptual framework that views marriage happiness as a two-dimensional model composed of a dimension of satisfactions and a dimension of tensions.[9] By means of a cluster analysis of responses to two checklists—one of recent pleasurable experiences in the marriage and another of disagreements—two clusters of marital experience were differentiated. One cluster reflects satisfactions—the companionship and sociability that husbands and wives enjoy together. The other cluster reflects tensions—the disagreements that husbands and wives encounter in the relationship. The items in the satisfactions cluster do not correlate, or correlate only moderately, with the items in the tensions cluster. Thus, satisfactions and tensions are not merely opposite ends of a single continuum of experiences but are separate and independent dimensions. Two indexes of satisfactions (companionship and sociability) and a single index of tensions were derived from responses to the batteries of items. These indexes correlate in the expected directions with the individual's own rating of his happiness in marriage: both indexes of satisfactions are positively related to marriage happiness, and tensions are negatively related to marriage happiness, while the two dimensions are virtually independent of each other. These characteristics of satisfactions and tensions suggest that the difference between an individual's scores on these separate dimensions, which we call the Marriage Adjustment Balance Scale (MABS), is a good indicator of happiness in marriage.

Finally, we propose that a study of married women's participation in the labor force cannot rely on the simple distinction made in studies of the male labor supply which view market work and leisure as the principal alternatives. For married women, there are three basic alternatives: work in

[7] U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.; Washington, D.C.: Government Printing Office, 1965): gross national product, p. 325; unemployment, p. 216; auto factory sales, p. 569.

[8] See Lee Rainwater, Richard P. Coleman, and Gerald Handel, *Workingman's Wife* (New York: Oceana Publications, 1959); Mirra Komarovsky, *Blue-Collar Marriage* (New York: Random House, 1964); Joseph D. Mooney, "Urban Poverty and Labor Force Participation," *American Economic Review*, LVII (March, 1967), 104–19; and Robert W. Hodge and Patricia Hodge, "Occupational Assimilation as a Competitive Process," *American Journal of Sociology*, LXXI (November, 1965), 249–64.

[9] Susan R. Orden and Norman M. Bradburn, "Dimensions of Marriage Happiness," *American Journal of Sociology*, LXXIII (May, 1968), 715–31.

the home, work in the labor market, and leisure activities.

When we speak of work for women, we must consider the woman who centers her life in the home and specializes in the production of home goods—principally child care, food preparation, and housekeeping. Adapting a distinction made by Cain,[10] we have designated women in the sample as either labor market workers or home market workers. Leisure can then be viewed as an alternative open to both groups of women.

This designation of women as labor market workers or home market workers should help to clarify the concept of work for women and eliminate some of the ambiguity in discussions of role conflicts. The designation, however, should not obscure the fact that home market work differs from labor market work in several important respects. First, there are no monetary rewards for tasks performed in the home; second, there are no job descriptions or universal standards of achievement for the production of home goods; third, the value of goods and services produced in the home is not included in the national income if the woman performs them herself but is included if they are performed by a substitute and then only at the lowest wage level; fourth, the home market worker reduces her responsibility if she performs well, while the labor market measures success by increase in responsibility. These differences undoubtedly create complex problems of prestige and achievement which cannot be resolved simply by designating all women as workers and then differentiating between women who work in the labor market and women who work in the home market. We do expect, however, that this distinction will prove useful as an operational concept.

In this study, the dependent variable—marriage happiness—is described by five measures. Satisfactions are measured by two indexes, one of companionship and the other of sociability; tensions by a single index; and over-all happiness in marriage by two measures, the individual's own assessment of his marriage and the MABS. The analysis will be done separately for husbands and wives in terms of three subgroups of the sample—marriages where the woman is in the labor market by necessity, those where the woman is in the labor market by choice, and those where the woman is in the home market by choice.

The data are from personal interviews by the NORC interviewing staff on the third wave of a longitudinal study of psychological well-being. Respondents were drawn from a probability sample of four communities, two within the Detroit metropolitan area and one each from the Chicago and the Washington metropolitan areas, as well as a fifth sample from the ten largest metropolitan areas in the United States.[11]

LABOR MARKET PARTICIPATION

In the aggregate, the women in the sample follow the national pattern of labor market participation. Of the 34 per cent of the married women in the labor market, 25 per cent work full time and 9 per cent work part time.

A woman's education, her husband's education, the size of her family, and her stage in the life cycle are important predictors of her probability of entering the labor market. Table 1, which deals with educational attainment, shows how participation in the labor market increases from 35 per cent among married women who have an eighth-grade education or less to 45 per cent for college-educated women.

Although women who are themselves

[10] Cain, *op. cit.*

[11] The research design, the over-all research objectives, and the sampling techniques are described in detail in Norman M. Bradburn, *The Structure of Psychological Well-Being* (Chicago: Aldine Publishing Co., in press); and David Caplovitz and Norman M. Bradburn, *Social Class and Psychological Adjustment: A Portrait of the Communities in the "Happiness" Study—a Preliminary Report* (Chicago: National Opinion Research Center, January, 1964).

college graduates are most likely to enter the labor market, the wives of college-educated men are least likely to do so. Only 29 per cent of these women are in the labor market. This probably reflects the fact that wives of many college graduates have less education than their husbands. In the 1960 Census, for example, there were 3,843,000 married men who had completed four or more years of college compared with only 2,020,000 married women who had done so.[12] Blood and Wolfe suggest, "If a wife has less education than her husband, the chances are she will be unusually satisfied with his income and less apt to go to work herself."[13] Another possible explanation is that the less educated woman can find employment only in low-status jobs which would be inconsistent with her husband's status and might prove an embarrassment both to herself and to her husband.

As one would expect, children are an important factor in determining the labor market participation of married women. The proportion of women who are employed drops from 65 per cent for those who have no children to 40 per cent for those with one child. The proportion continues to decline as the number of children increases: 30 per cent of the women who have two or more children and only 22 per cent of those who have four or more children are in the labor market.

A married woman's life is less likely than her husband's to follow a continuous pattern. As she moves through the life cycle, the nature of her responsibilities changes, and she has the opportunity to choose among a new set of alternative ways of organizing her life. At the points of change, a woman re-examines her responsibilities and commitments to her husband, children, home, and self. The choice she makes when her children are infants is likely to be different from the choice she makes when her children move on to

TABLE 1

WIFE'S WORK STATUS BY EDUCATION OF HUSBANDS AND WIVES

Education	Wife's Work Status: Labor Market: Full Time	Wife's Work Status: Labor Market: Part Time	Wife's Work Status: Home Market	Total: Percentage	Total: N-NA
Wife's education:					
8th grade or less	23	12	64	99	146
Some high school	21	9	70	100	240
High school graduate	23	8	69	100	342
Some college	27	13	60	100	116
College graduate	40	5	55	100	58
Husband's education:					
8th grade or less	24	13	63	100	151
Some high school	26	7	67	100	175
High school graduate	26	6	67	99	199
Some college	38	7	55	100	85
College graduate	21	8	71	100	137

N–NA	1,649
NA, education	2
Total *N*	1,651

[12] U.S. Bureau of the Census, *U.S. Census of Population: 1960, Subject Reports Educational Attainment,* Final Report PC (2)-5B (Washington, D.C.: Government Printing Office, 1963). Data for married men are from Table 4, p. 54; for married women, from Table 5, p. 71.

[13] Blood and Wolfe, *op. cit.,* p. 99.

school, or when they finally leave the home to pursue independent lives. Participation in the labor force—movement both into and out of paid employment—is one way that women respond to changes in their life patterns. Table 2 shows that, during their children's infancy and early childhood, only 18 per cent of the wives are employed. As children grow, there is a steady increase in the proportion of women entering the labor market. When the youngest child in the family is in the upper half of grade school, the proportion of women in the labor market reaches a peak of 49 per cent and remains near this level as the youngest child moves on to high school and from there to independent activities.

The changing pattern of part-time and full-time employment shown in Table 2 illustrates how women respond to changes in their responsibilities. After the period of infancy and early childhood when employment is minimal, women begin to move into both part-time and full-time employment at each successive stage of child rearing. This trend continues until the youngest child is in the upper half of grade school. At this stage in a woman's life cycle, part-time employment is at a peak of 18 per cent. After the youngest child is in high school, mothers begin to move out of part-time and into full-time employment. At the stage when there are no longer any children under twenty-one in the home, only 6 per cent of the mothers are employed part time, while 44 per cent are employed full time.

If the wife was employed, we asked the respondent (husband or wife) the main reasons for her presence in the labor market. The responses were 69 per cent in order to earn money, 6 per cent in order to pursue a career, 15 per cent in order to get out of the house, 10 per cent for other reasons. We found little difference in the responses given by women and by men to this question. We did find, however, that the opportunity to earn money was more likely to be given as the major motive for full-time employment than it was for part-time employment. Three-quarters of the women employed full time compared with only one-half of the women employed part time reported that the chance to earn money was their major objective.

Even though the opportunity to earn money is a major motive for employment for most women, it is essential to distinguish those women who are in the labor market out of economic necessity from those who are there by choice. Over half

TABLE 2

WIFE'S WORK STATUS BY AGE OF YOUNGEST CHILD

Age of Youngest Child	Wife's Work Status: Labor Market, Full Time	Labor Market, Part Time	Home Market	Total: Percentage	Total: N–NA
Under 3 years	12	6	81	99	506
3–5 years (preschool)	18	9	73	100	267
6–10 years (grade school–lower)	22	14	63	99	235
11–14 years (grade school–upper)	31	18	50	99	125
15–18 years (high school)	36	13	51	100	106
19–20 years	30	7	63	100	27
None under 21	44	6	50	100	385
Total N	....	....	....	....	1,651

of the women in the labor market (55 per cent) reported that they would work even if they did not need the money. This group was designated as women who have a commitment to their employment and are in the labor market by choice rather than by economic necessity.

Table 3 shows, as one would expect, that the proportion of employed women

TABLE 3

MARRIED WOMEN IN THE LABOR FORCE
"Would You Work If You Didn't Need the Money?"
(Percentage Responding "Yes," by Education)

RESPONDENT'S EDUCATION	"YES" RESPONSES	
	Wives	Husbands
8th grade or less	34 (50)	41 (54)
Some high school	49 (72)	56 (57)
High school graduates	57 (110)	52 (67)
Some college	71 (48)	55 (38)
College graduates	77 (26)	69 (39)

N–*NA*, wife in labor market	561
NA, education	2
N, wife in labor market	563
N, wife in home market	1,088
Total *N*	1,651

who are in the labor market by choice increases with education from 34 per cent among those with an eighth-grade education or less to 77 per cent among college graduates. In terms of the husband's education, a somewhat smaller increase occurs, but it is in the same direction.

In summary, the data show that, while on the average one-third of married women participate in the labor market, this proportion increases with the woman's own education but declines with the husband's education. Participation in the labor market is responsive to the size of the family and to the woman's changing responsibilities as she moves through the life cycle.

LABOR MARKET PARTICIPATION AND MARRIAGE HAPPINESS

The crucial question to which this paper is directed is how a woman's labor market participation affects both her marriage happiness and that of her husband. The relative standing of each subgroup in the sample on the five measures of marriage adjustment will be expressed in terms of ridits.[14] This statistic measures the probability that a person chosen at random from a particular subgroup of the sample will be better or worse off on a measure than an individual chosen at random from an identified reference distribution. The ten metropolitan areas were the natural selection as the identified reference distribution, since they were included in the study to provide a basis for comparison.

Table 4 shows that both partners in a marriage are lower in marriage happiness when the wife is denied a choice and is in the labor market only because she needs the money than when the wife participates in the labor market by choice. When a married woman is in the labor market only out of economic necessity, there is a significant reduction in her happiness, measured both in terms of her perception of the balance of recent positive and negative experiences and in terms of her long-range evaluation of her marriage. The husband is also lower on both indicators of over-all happiness, but the difference is significant only on the MABS.

The particularly interesting finding in Table 4 is that the strain in the marriage stems from different sources for men and for women. When the wife is in the labor market only out of necessity, the husband perceives a significant increase in the negative side of the marriage in the tensions

[14] Irwin D. J. Bross and Rivkah Feldman, *Ridit Analysis of Automotive Crash Injuries* (New York: Cornell University Medical College, October, 1965); Irwin D. J. Bross, "How To Use Ridit Analysis," *Biometrics*, XIV (March, 1958), 18–38; and Thomas S. Langner and Stanley T. Michael, *Life Stress in Mental Health* (New York: Free Press, 1963), pp. 87–101.

he experiences in the relationship. In the same situation, the wife perceives a significant reduction in the positive side of the marriage in the sociability she enjoys with her husband.

The questions which served as the bases for the indexes of tensions and satisfactions were phrased in terms of disagreements or enjoyable activities with the spouse during the past few weeks. Even though the men and women in the sample are not couples, we would expect, on the average, little difference between them in activities which involve them both. It seems safe to assume, therefore, that the differences observed here are largely perceptual in character. We shall see later, when we control for education, that these perceptual differences occur mainly among the less educated segments of society. Lower-class men are higher in tensions and lower-class women are lower in sociability if the woman is in the labor market out of necessity than if she is there by choice. Among the better educated, both husbands and wives are higher in tensions and lower in sociability.

The difference in the way men and women at lower socioeconomic levels perceive the level of sociability they enjoy in marriage may reflect differences in their psychological needs and expectations. In this group, wives more than husbands may look to the marriage partner to satisfy their need for sociability. The wife's lower perception of the sociability she enjoys may reflect a discrepancy between her needs and expectations, on the one hand, and her actual social experiences, on the other.

The perceptual difference on the tensions index probably reflects a basic difference in the way lower-SES husbands and wives conceptualize their roles. The man whose wife is in the labor force out of necessity may feel derelict in the performance of his

TABLE 4

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES FOR HUSBANDS AND WIVES, CONTROLLING FOR WIFE'S WORK STATUS

Wife's Work Status	Marriage Adjustment Measures					N-NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Husbands:						
Wife's work status:						
Labor market:						
Necessity	.44	.64*a	.48	.40	.35	118
		*			*	
Choice	.50	.46	.51	.44	.49	137
		*			*	
Home market	.47	.54*a	.50	.38	.40	487
Wives:						
Work status:						
Labor market:						
Necessity	.37*b	.56	.40	.34	.33	139
	*			*	*	
Choice	.47	.52	.48	.45	.46	168
				*	*	
Home market	.47*b	.56	.46	.38	.38	600

N-NA	1,649
NA	2
Total N	1,651

NOTE.—In this and subsequent tables, an asterisk identifies significant difference at 5 per cent level of confidence between adjacent ridits. An asterisk followed by the same letter identifies differences between ridits that are not adjacent.

economic role as the income provider for his wife and children. A man interprets his marriage role in terms of his ability to provide for the economic needs of his family. The fact that he cannot support his family without his wife's help is a threat to his perception of himself as a husband and father. He appears to be more sensitive to disagreements in his marriage that loom as an additional threat to an already shaky ego.

The woman, on the other hand, is probably more capable of integrating her need to supplement her husband's income into her role of caring for her children. The woman who is in the labor market out of economic necessity is apparently, either intentionally or unintentionally, overlooking some disagreements in her marriage. They may not weigh as heavily on her, or she may not perceive them as disagreements at all.

Among women who are free to choose between the labor market and the home market, there is no evidence that the labor market choice creates a strain in the marriage either for the wife or for the husband. On the contrary, Table 4 shows that, if the wife chooses the labor market, husbands and wives both attain a higher balance in their perceived levels of tensions and satisfactions than they do if the wife chooses the home market. Both husbands and wives are lower in tensions and higher in sociability if the wife chooses the labor market than if she chooses the home market, but the difference for men is significant only on the tensions index and for women only on the sociability index.

TABLE 5

AVERAGE RIDITS ON SOCIABILITY INDEX WHEN WIVES ARE FREE TO CHOOSE BETWEEN HOME MARKET AND LABOR MARKET, CONTROLLING FOR TOTAL FAMILY INCOME

WIFE'S WORK CHOICE	TOTAL FAMILY INCOME		
	Less than $5,000	$5,000–$7,999	$8,000 or More
Husbands:			
Wife's work status:			
Labor market by choice	[.24] (3)	.36 (27)	.48 (99)
Home market by choice	.34 (54)	.35 (208)	.42 (212)
Wives:			
Work status:			
Labor market by choice	[.27] (9)	.38 (40)	.49 (105)
Home market by choice	.24 (85)	.38 (246)	.43 (236)

N–*NA*, choice: labor market and home market	1,324
NA, income	74
NA, sociability	11
N, choice	1,409
N, necessity	242
Total *N*	1,651

The lower tension level for men whose wives choose the labor market over the home market may reflect something of the personality, outlook, and attitude of the husband as well as his relationship with his wife and children. It seems likely that the wife's choice to participate in the labor market indicates that both partners recognize, enjoy, and perhaps commit themselves to a marriage in which both will be relatively free and independent. Also, in

a relationship as intimate and complex as marriage, the man whose wife chooses to participate in the labor market may experience some relief from the pressures upon him as a person in the relationship. Our data are not sensitive enough to test these hypotheses directly.

The higher level of sociability in marriages in which the wife chooses the labor market over the home market may simply reflect the fact that the wife's income allows the husband and wife the means to enjoy a higher level of sociability together. The sociability index is composed of items which presuppose a certain level of discretionary income—going out to a sporting event together, eating out in a restaurant together, entertaining, and visiting friends. The wife's income may be an important factor in allowing the couple to fit these activities into the family budget.

This hypothesis can be tested by controlling for different income levels. Income is positively related both to labor market choice and to sociability. Among those women who have a choice, the proportion who choose the labor market increases from 10 per cent to 14 per cent to 31 per cent as total family income increases in three categories: less than $5,000, $5,000–$7,999, and $8,000 or more. Sociability at these three income levels increases from an average ridit of .24 to .38 to .46. Thus, if income explains the difference on the sociability index between the labor market and the home market choice, we would expect this difference to disappear when we control for income.

Table 5 shows that income explains all of the difference on the sociability index when total family income is under $8,000 and some of the difference when family income is over $8,000. At the higher income levels, the wife's income may have an "extra" tag which frees the couple psychologically to enjoy higher levels of sociability than are enjoyed by the home market wife and her husband at the same income level.

We can make another comparison across income levels. A woman at one income level who chooses the labor market can be compared with a woman at the next lower level who chooses the home market, on the assumption that the employed woman would be at the lower income level had she not chosen to participate in the labor market. Even though the woman is in the labor market by choice, the fact that she is gainfully employed places her in a higher income bracket than if she had not made this choice. We expect that her contribution to the total family income gives her an advantage on the sociability index compared with the woman who does not make the labor market choice.

When we compare women across income levels, we find that the woman who chooses the labor market is higher on marriage sociability than the woman who chooses the home market at the next lower income level. In comparing women in the $8,000 or higher group who choose the labor market with women in the $5,000–$7,999 income group who choose the home market, the average ridits on the sociability index are .49 against .38; and in comparing labor market wives in the $5,000–$7,999 income group with home market wives in the less than $5,000 income group, the average ridits are .38 and .24. In the first comparison the difference is significant, and in the second it is just short of significance. The same general argument holds for men whose wives are in either the labor market or the home market by choice.

It seems quite clear, then, that the woman's contribution to the family income is an important factor in explaining differences on the sociability index between marriages in which the wife chooses the labor market and those in which she chooses the home market. In addition, the woman who chooses the labor market may be involved with a new network of friends from among her work associates with whom then both she and her husband socialize.

Even though husbands and wives are both significantly higher on the MABS if the wife chooses to participate in the labor

market than if she chooses the home market, there is no difference between these two groups in their own assessment of their marriage happiness. On this measure, there is only a slight tendency for husbands whose wives choose the labor market to report that they are happier than husbands whose wives choose the home market. There is no difference for the wives themselves. A woman who chooses the home market is just as likely to say her marriage is happy as the woman who chooses the labor market.

This suggests to us that home market workers entertain certain commitments and responsibilities that are as meaningful to them as the employment commitment is for women who choose the labor market, at least at certain stages of the life cycle. In the aggregate, there is no difference in over-all marriage happiness ratings associated with the choice between the home and the labor market. However, we shall see later, when we control for stages in the life cycle, that certain differences tend to emerge. When there are preschool children in the home, most women choose to focus their identity in the home as the center of the family. These women are more likely to assess their marriage as happy than are women who choose the labor market. As children enter school, the proportion of women who enter the labor market increases. At this stage, women in the labor market seem to have some advantage over those in the home market. Later, when children are of high school age or older, there appears to be no difference in the way women evaluate their marriage. Women who remain in the home apparently take on other commitments as a way of adapting to changes in their life patterns. They may commit themselves to volunteer, civic, or church activities, to artistic or intellectual pursuits, or simply to social activities. The woman who chooses the home market appears to find alternative ways to adapt to her life pattern which are as satisfying to her as employment is to the woman who chooses the labor market.

It should be noted, however, that there are undoubtedly important differences in the alternatives which are open to women in the home market at different levels of the social structure. The lower-class woman, for example, is probably limited by her own and her husband's traditional orientation toward the woman's role, by her educational attainment which affects the level of her expectations and aspirations, and by her reluctance and lack of aggression in moving out from the family clan to confront the problems of a larger society and to share in the pleasures of this society.[15]

CONTROL FOR PART-TIME VERSUS FULL-TIME EMPLOYMENT

When a woman enjoys the freedom to choose among alternative life styles, there is some tendency for marriage adjustment to be more favorable for both partners if she chooses part-time employment over either full-time employment or the home market.

Table 6 shows that, for one thing, women making the part-time choice are more likely to report that they are happy in marriage than are women making either the full-time or the home market choice. For another, the husband whose wife chooses part-time employment is higher in companionship than are other husbands. This is striking because, generally, companionship remains remarkably stable in response to changes in a woman's work status. This case is one of the few times we observe any change on the companionship index. Also, the husband whose wife chooses part-time employment is significantly higher on the MABS than is the man whose wife chooses the home market. And finally, in all of the comparisons on the five marriage adjustment measures, part-time employment is more favorable than the home market choice; and, in eight of the ten comparisons, part-time is more favorable than full-time employment.

[15] See Rainwater *et al.*, *op. cit.*; and Komarovsky, *op. cit.*

These tendencies suggest that part-time employment may indeed be the way for a woman to combine the labor market and the home market to achieve optimum adjustment in the marriage relationship both for herself and for her husband. However, only a small proportion of women—just 6 per cent of those who are free to make a choice—choose part-time participation in the labor market. This fact may reflect limited opportunities for part-time employment as well as social pressures to put in a "full day's work."

Table 6 further shows that, in marriages where the wife is in the labor market out of economic necessity, there is also some tendency for adjustment to be more favorable if the woman is employed part time than if she is employed full time. Seven of the ten comparisons on the marriage adjustment measures favor part-time over full-time employment.

The control for part-time and full-time employment does not explain the difference between marriages where the wife participates in the labor market by necessity and those where she participates by choice. Participation in the labor market out of necessity, whether it be part time or full time, creates a strain in the marriage for both the husband and the wife.

TABLE 6

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES CONTROLLING FOR WIFE'S WORK STATUS AND FULL-TIME VS. PART-TIME EMPLOYMENT

Wife's Work Status	Marriage Adjustment Measure					N-NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Husbands:						
Wife's work status:						
Labor market by necessity:						
Full time	.43	.63	.47	.39	.34*a	90
Part time	.44	.65	.52	.44	.36*b	27
Labor market by choice:						
Full time	.49	.47	.47	.45	.48*a	99
Part time	.51	.46	.60	.43	.54*b	36
					*	
Home market	.47	.54	.50	.38	.40	487
Wives:						
Work status:						
Labor market by necessity:						
Full time	.36	.56	.40	.33	.33*c	102
Part time	.41	.59	.41	.36	.33	36
Labor market by choice:						
Full time	.46	.51	.46	.42	.45*c	110
Part time	.52	.51	.50	48	.47	51
Home market	.47	.56	.46	.38	.38	600

N-NA	1,638
NA	13
Total N	1,651

EDUCATIONAL ATTAINMENT

We noted earlier that, as one would expect, women are most likely to participate in the labor market out of economic necessity at lower socioeconomic levels. The proportion declines with steps up in education from 66 per cent for women who have an eighth-grade education or less to 23 per cent for women who are college graduates.

The interesting finding in Tables 7 and 8 is that the strain holds at all levels of education for both husbands and wives if

market than if she chooses the home market, there is no difference between these two groups in their own assessment of their marriage happiness. On this measure, there is only a slight tendency for husbands whose wives choose the labor market to report that they are happier than husbands whose wives choose the home market. There is no difference for the wives themselves. A woman who chooses the home market is just as likely to say her marriage is happy as the woman who chooses the labor market.

This suggests to us that home market workers entertain certain commitments and responsibilities that are as meaningful to them as the employment commitment is for women who choose the labor market, at least at certain stages of the life cycle. In the aggregate, there is no difference in over-all marriage happiness ratings associated with the choice between the home and the labor market. However, we shall see later, when we control for stages in the life cycle, that certain differences tend to emerge. When there are preschool children in the home, most women choose to focus their identity in the home as the center of the family. These women are more likely to assess their marriage as happy than are women who choose the labor market. As children enter school, the proportion of women who enter the labor market increases. At this stage, women in the labor market seem to have some advantage over those in the home market. Later, when children are of high school age or older, there appears to be no difference in the way women evaluate their marriage. Women who remain in the home apparently take on other commitments as a way of adapting to changes in their life patterns. They may commit themselves to volunteer, civic, or church activities, to artistic or intellectual pursuits, or simply to social activities. The woman who chooses the home market appears to find alternative ways to adapt to her life pattern which are as satisfying to her as employment is to the woman who chooses the labor market.

It should be noted, however, that there are undoubtedly important differences in the alternatives which are open to women in the home market at different levels of the social structure. The lower-class woman, for example, is probably limited by her own and her husband's traditional orientation toward the woman's role, by her educational attainment which affects the level of her expectations and aspirations, and by her reluctance and lack of aggression in moving out from the family clan to confront the problems of a larger society and to share in the pleasures of this society.[15]

CONTROL FOR PART-TIME VERSUS FULL-TIME EMPLOYMENT

When a woman enjoys the freedom to choose among alternative life styles, there is some tendency for marriage adjustment to be more favorable for both partners if she chooses part-time employment over either full-time employment or the home market.

Table 6 shows that, for one thing, women making the part-time choice are more likely to report that they are happy in marriage than are women making either the full-time or the home market choice. For another, the husband whose wife chooses part-time employment is higher in companionship than are other husbands. This is striking because, generally, companionship remains remarkably stable in response to changes in a woman's work status. This case is one of the few times we observe any change on the companionship index. Also, the husband whose wife chooses part-time employment is significantly higher on the MABS than is the man whose wife chooses the home market. And finally, in all of the comparisons on the five marriage adjustment measures, part-time employment is more favorable than the home market choice; and, in eight of the ten comparisons, part-time is more favorable than full-time employment.

[15] See Rainwater *et al.*, *op. cit.*; and Komarovsky, *op. cit.*

These tendencies suggest that part-time employment may indeed be the way for a woman to combine the labor market and the home market to achieve optimum adjustment in the marriage relationship both for herself and for her husband. However, only a small proportion of women—just 6 per cent of those who are free to make a choice—choose part-time participation in the labor market. This fact may reflect limited opportunities for part-time employment as well as social pressures to put in a "full day's work."

The control for part-time and full-time employment does not explain the difference between marriages where the wife participates in the labor market by necessity and those where she participates by choice. Participation in the labor market out of necessity, whether it be part time or full time, creates a strain in the marriage for both the husband and the wife.

TABLE 6

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES CONTROLLING FOR WIFE'S WORK STATUS AND FULL-TIME VS. PART-TIME EMPLOYMENT

Wife's Work Status	Marriage Adjustment Measure					N–NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Husbands:						
Wife's work status:						
Labor market by necessity:						
Full time	.43	.63	.47	.39	.34*a	90
Part time	.44	.65	.52	.44	.36*b	27
Labor market by choice:						
Full time	.49	.47	.47	.45	.48*a	99
Part time	.51	.46	.60	.43	.54*b	36
					*	
Home market	.47	.54	.50	.38	.40	487
Wives:						
Work status:						
Labor market by necessity:						
Full time	.36	.56	.40	.33	.33*c	102
Part time	.41	.59	.41	.36	.33	36
Labor market by choice:						
Full time	.46	.51	.46	.42	.45*c	110
Part time	.52	.51	.50	48	.47	51
Home market	.47	.56	.46	.38	.38	600

N–NA	1,638
NA	13
Total N	1,651

Table 6 further shows that, in marriages where the wife is in the labor market out of economic necessity, there is also some tendency for adjustment to be more favorable if the woman is employed part time than if she is employed full time. Seven of the ten comparisons on the marriage adjustment measures favor part-time over full-time employment.

EDUCATIONAL ATTAINMENT

We noted earlier that, as one would expect, women are most likely to participate in the labor market out of economic necessity at lower socioeconomic levels. The proportion declines with steps up in education from 66 per cent for women who have an eighth-grade education or less to 23 per cent for women who are college graduates.

The interesting finding in Tables 7 and 8 is that the strain holds at all levels of education for both husbands and wives if

TABLE 7

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES FOR WIVES, CONTROLLING FOR WIFE'S EDUCATION AND WORK STATUS

WIFE'S EDUCATION AND WORK STATUS	MARRIAGE ADJUSTMENT MEASURE					N-NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Less than high school education:						
Labor market:						
Necessity	.36	.55	.39	.25	.29	69
Choice	.44	.52	.52	.39	.44	52
Home market	.45	.54	.44	.34	.37	262
High school graduate:						
Labor market:						
Necessity	.44	.58	.42	.43	.38	46
Choice	.52	.50	.44	.44	.45	58
Home market	.49	.57	.48	.40	.39	236
Some college or higher:						
Labor market:						
Necessity	.30	.62	.35	.38	.29	20
Choice	.47	.50	.46	.51	49	52
Home market	.50	.59	.47	.43	.39	102

N-NA for wives	897
NA	7
N for wives	904
N for husbands	747
Total N	1,651

TABLE 8

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES FOR HUSBANDS, CONTROLLING FOR HUSBAND'S EDUCATION AND WIFE'S WORK STATUS

HUSBAND'S EDUCATION AND WIFE'S WORK STATUS	MARRIAGE ADJUSTMENT MEASURE					N-NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Less than high school education:						
Labor market:						
Necessity	.43	.60	.46	.41	.37	57
Choice	.49	.46	.49	.40	.47	54
Home market	.44	.54	.48	.33	.38	210
High school graduate:						
Labor market:						
Necessity	.43	.72*a *	.53	.44	.30	31
Choice	.47	.47	.53	.39	.47	33
Home market	.51	.55*a	.51	.42	.42	134
Some college or higher:						
Labor market:						
Necessity	.44	.62	.47	.36	.34	29
Choice	.53	.46	.51	.53	.54	48
Home market	.48	.53	.51	.42	.43	143

N-NA for husbands	739
NA	8
N for husbands	747
N for wives	904
Total N	1,651

the woman is in the labor market out of necessity. There are undoubtedly qualitative differences in the interpretation of need at different class levels. Yet, despite differences in standards of evaluation and in probabilities, marriages at all levels of the social structure are affected adversely when the woman is in the labor market only out of necessity. At each of three levels of education, husbands and wives are lower on both measures of over-all happiness—the individual's own assessment of his or her happiness in marriage and the MABS—if the woman is in the labor market by necessity rather than by choice.

The source of the strain on the marriage varies with the three educational groups. As we noted earlier, in the lowest educational group the strain in the marriage comes from different sources for men and for women. Here the women are lower in sociability and the men higher in tensions if the wife is in the labor market by necessity than if she is there by choice. In the high school graduate group, husbands and wives are both higher in tensions, but the impact is greater on the husband than on the wife. Even considering the small case base, this difference is significant for the husband. In the highest of the three educational groups—those who have at least some college education—husbands and wives are both lower in sociability and higher in tensions if the wife is in the labor market by necessity than if she is there by choice.

Differences in the way men and women perceive the tensions and sociability they experience in marriage occur mainly among the less educated and tend to disappear at higher levels of education. We have already commented on the possibility that, at lower socioeconomic levels, differential perception of sociability enjoyed in marriage may reflect differences in psychological needs and expectations between men and women. Differences in their perception of tensions may indicate a basic difference in the way lower-class husbands and wives conceptualize their roles. The man whose wife is in the labor market only out of necessity may feel threatened in his primary role as the income provider for his family, while the woman may be better able to integrate her more diverse roles. At higher socioeconomic levels, the congruence between the man's and the woman's perception of the tensions and sociability they enjoy in marriage suggests that these marriages are more egalitarian than are marriages at lower levels of the social structure.

When we compare marriages in which the woman is free to choose between the labor market and the home market, Tables 7 and 8 show that at the three educational levels there is little difference in the individual's own assessment of the marriage either for husbands or for wives. On the MABS, however, husbands and wives both achieve a higher positive balance of recent experiences if the wife chooses the labor market than they do if the wife chooses the home market. Even with the small case base, this difference is significant for college-educated men. With only one exception, there are less tensions and more sociability at every educational level for both men and women if the wife chooses the labor market over the home market.

Thus, we conclude that, in general, the relationship between a woman's work status and marriage happiness holds for different levels of the social structure, but the introduction of a control for education specifies that differences between men and women on the tensions and sociability which they perceive in marriage occur in the less educated segments of society.

STAGE IN THE LIFE CYCLE

At every stage of the life cycle, differences in marriage adjustment persist for husbands and wives when we compare marriages where the woman is in the labor market out of economic necessity with those where she is free to choose between the labor market and the home market. In twenty-one out of twenty-four comparisons in Tables 9 and 10, participation in the labor market by necessity is less favorable

TABLE 7

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES FOR WIVES, CONTROLLING FOR WIFE'S EDUCATION AND WORK STATUS

WIFE'S EDUCATION AND WORK STATUS	MARRIAGE ADJUSTMENT MEASURE					N-NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Less than high school education:						
Labor market:						
Necessity	.36	.55	.39	.25	.29	69
Choice	.44	.52	.52	.39	.44	52
Home market	.45	.54	.44	.34	.37	262
High school graduate:						
Labor market:						
Necessity	.44	.58	.42	.43	.38	46
Choice	.52	.50	.44	.44	.45	58
Home market	.49	.57	.48	.40	.39	236
Some college or higher:						
Labor market:						
Necessity	.30	.62	.35	.38	.29	20
Choice	.47	.50	.46	.51	49	52
Home market	.50	.59	.47	.43	.39	102

N-NA for wives	897
NA	7
N for wives	904
N for husbands	747
Total N	1,651

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AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES FOR HUSBANDS, CONTROLLING FOR HUSBAND'S EDUCATION AND WIFE'S WORK STATUS

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	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
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Labor market:						
Necessity	.43	.60	.46	.41	.37	57
Choice	.49	.46	.49	.40	.47	54
Home market	.44	.54	.48	.33	.38	210
High school graduate:						
Labor market:						
Necessity	.43	.72*a *	.53	.44	.30	31
Choice	.47	.47	.53	.39	.47	33
Home market	.51	.55*a	.51	.42	.42	134
Some college or higher:						
Labor market:						
Necessity	.44	.62	.47	.36	.34	29
Choice	.53	.46	.51	.53	.54	48
Home market	.48	.53	.51	.42	.43	143

N-NA for husbands	739
NA	8
N for husbands	747
N for wives	904
Total N	1,651

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TABLE 9

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES FOR WIVES, CONTROLLING FOR STAGE IN LIFE CYCLE AND WIFE'S WORK STATUS

STAGE IN LIFE CYCLE AND WIFE'S WORK STATUS	MARRIAGE ADJUSTMENT MEASURE					N-NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Preschool children:						
Labor market:						
Necessity	.38	.54	.41	.37	.37	52
Choice	.44	.60	.49	.43	.40	40
Home market	.47	.59	.45	.38	.36	337
Grade school children:						
Labor market:						
Necessity	.36	.62	.36	.30	.27	37
Choice	.48	.51	.50	.57	.53	36
Home market	.45	.53	.46	.37	.40	112
High school children or older:						
Labor market:						
Necessity	.38	.53	.43	.33	.35	45
Choice	.48	.48	.44	.36	.42	73
Home market	.50	.51	.48	.36	.41	141

N-NA for wives	873
Honeymooners	26
NA	5
N for wives	904
N for husbands	747
Total N	1,651

NOTE.—Excludes "honeymooners"—women under thirty who have no children (26 respondents: 16 in labor market and 10 in home market). Too few cases to calculate meaningful ridits. Women over thirty without children are included in the last category: 'High school children or older."

TABLE 10

AVERAGE RIDITS ON MARRIAGE ADJUSTMENT MEASURES FOR HUSBANDS, CONTROLLING FOR STAGE IN LIFE CYCLE AND WIFE'S WORK STATUS

STAGE IN LIFE CYCLE AND WIFE'S WORK STATUS	MARRIAGE ADJUSTMENT MEASURES					N-NA
	Marriage Happiness	Tensions	Companionship	Sociability	MABS	
Preschool children:						
Labor market:						
Necessity	.40	.69	.45	.38	.27	38
Choice	.40	.59	.44	.34	.34	26
Home market	.47	.57	.49	.38	.38	274
Grade school children:						
Labor market:						
Necessity	.45	.61	.46	.41	.36	38
Choice	.46	.41	.50	.48	.55	35
Home market	.44	.51	.45	.38	.41	100
High school children or older:						
Labor market:						
Necessity	.49	.60	.52	.38	.39	29
Choice	.53	.41	.56	.42	.53	61
Home market	.52	.50	.57	.39	.46	107

N-NA for husbands	708
Honeymooners	31
NA	8
N for husbands	747
N for wives	904
Total N	1,651

NOTE.—Excludes "honeymooners"—men under thirty who have no children (31 respondents: 12 whose wives are in labor market

for the marriage adjustment of both the wife and the husband than is participation in either the labor market or the home market by choice.

Among those who are free to choose between market and the labor market, there are some differences at varying stages of the life cycle. When there are preschool children in the home, husbands and wives are both happier in marriage if the wife chooses the home market than if she chooses the labor market. Seven out of ten comparisons favor the home market choice. When there are grade school children in the family, all of the comparisons favor the labor market choice. When the youngest child in the family is of high school age or older, there is little or no difference in marriage adjustment between the labor market and the home market choice.

CONCLUSIONS

Since the freedom to choose among alternative life styles is clearly an important variable is predicting happiness in marriage, efforts to extend this freedom should have positive effects on the marriage happiness of both husbands and wives. A general upgrading and expansion of educational and employment opportunities for lower-SES men would allow them to fulfil their role as income providers for their families and, at the same time, would extend to their wives the freedom to choose between the home and the labor market.

However, even when the husband's income is adequate to meet his family's needs, there are undoubtedly other restraints on a woman's freedom of choice. On the supply side, there are restraints to freedom of entry into the labor market imposed by the woman's education, her stage in the life cycle, the availability of adequate substitute help, and her own personality disposition, needs, and desires. On the demand side, there may be restraints to free entry in the labor market imposed by employers and by male incumbents in the labor force. Any efforts that individuals and private and public institutions direct toward removing these restraints would extend women's freedom to choose between the home market and the labor market.[16] In the private sector, for example, an expansion of employment opportunities commensurate with a women's talents and capabilities, on both a full-time and a part-time basis, would insure that the choice was a meaningful one. In the public sector, if society moves toward its national goal of better educational opportunities for all its members, there should be an increase in the proportion of women who choose to enter the labor force in prestigious occupations to which they have a real sense of commitment. We predict that this phenomenon will not be detrimental to the institution of marriage. On the contrary, there is evidence to support the contention that there might well be a strengthening of the marriage relationship both for the husband and for the wife.

National Opinion Research Center

[16] For a discussion of policy proposals, see U.S. President's Commission on Status of Women, *op. cit.*; Eli Ginsberg and Associates, *Life Styles of Educated Women* (New York: Columbia University Press, 1966), chap. xii, pp. 179–95; and Hannah Gavion, *The Captive Wife* (London: Routledge & Kegan Paul, 1966), chap. xvi, pp. 141–48.

The Redress of Distributive Injustice[1]

Philip W. Blumstein and Eugene A. Weinstein

ABSTRACT

Distributive injustice was experimentally induced in a dyad where a stooge claimed a share of credit incommensurate with his contribution to a joint task. Redressive behavior on part of subjects was of two kinds—involving the allocation of rewards in a subsequent work session, and the granting of esteem to the other person. With some exception, the tendency was for beneficiaries of injustice to be more extreme in redress than were its victims. Females, individuals high in Machiavellianism, and those high in Need for Approval did not redress in reward allocation when victimized. Those low in Machiavellianism redressed more when they were victims.

INTRODUCTION

In one form or another, the concept of distributive justice undergirds most contemporary models of social relations.[2] As the starting point of the present study we take the formulation presented by Homans: "A man in an exchange relation with another will expect that the rewards of each man be proportional to his costs . . . and that the net rewards, or profits, of each man be proportional to his investment—the greater the investment, the greater the profits."[3] Homans' proposition implies that exchange relations tend toward balance. But what of distributive injustice? What happens when the individual's rewards and investments are out of balance?

The tendency to redress injustice has considerable empirical support, ranging from studies of children to studies of workers in a variety of industrial and commercial settings.[4] Such studies point to redressive responses of two kinds. First, there are those involving affective components. Depending on the direction of injustice, one may feel anger or disesteem for the other, or tension and guilt for oneself. Second, redress may occur through behavioral responses directed at correcting imbalance between inputs and rewards. In this investigation, we are concerned with aspects of both of these. Our research

[1] Paper read at the annual meetings of the American Sociological Association, San Francisco, August, 1967. This research was supported by a grant from the National Science Foundation, GS-1093. It is reported on more fully in the Master's thesis of the first author, "When Justice Fails: An Experimental Study of Personality Factors in Responses to Distributive Injustice" (Vanderbilt University, 1967). The authors wish to thank Lawrence Beckhouse and Jack Dailey for their thespian talents used in behalf of this project.

[2] For an excellent discussion of the historical antecedents of the concept of distributive justice, see J. Stacy Adams, "Inequity in Social Exchange," in Leonard Berkowitz (ed.), *Advances in Experimental Social Psychology* (New York: Academic Press, 1965), II, 267–99.

[3] George Caspar Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, Brace & World, 1961), p. 232.

[4] See, for example, Elliott Jaques, *Equitable Payment* (New York: John Wiley & Sons, 1961); J. V. Clark, "A Preliminary Investigation of Some Unconscious Assumptions Affecting Labor Efficiency in Eight Supermarkets" (unpublished Ph.D. dissertation, Harvard University, 1958); John W. Thibaut, "An Experimental Study of the Cohesiveness of Underprivileged Groups," *Human Relations*, III (August, 1950), 251–78; J. Stacy Adams and William B. Rosenbaum, "The Relationship of Worker Productivity to Cognitive Dissonance about Wage Inequities," *Journal of Applied Psychology*, XLVI (June, 1962), 161–64; J. Stacy Adams and Patricia R. Jacobsen, "Effects of Wage Inequities on Work Quality," *Journal of Abnormal and Social Psychology*, LXIX (July, 1964), 19–25; and Homans, *op. cit.*

strategy is to create experimentally a series of situations characterized by either justice or injustice and to observe both affective and behavioral responses in them. On the affective side, we are concerned with the effects of injustice on the ways the source of injustice is evaluated. On the behavioral side, we are concerned with the allocation of rewards after an episode of injustice has occurred.

Redress of injustice.—Why is injustice redressed? The answer usually given is that individuals are motivated to re-establish justice and return the exchange relation to a balanced state. There are two basic sources of this motivation appearing in the literature. Aronson is typical of those who see the motivational source in the reduction of cognitive dissonance: "If a person . . . expends effort in an attempt to reach a goal . . . and fails, he experiences dissonance. His cognition that he has tried hard to reach the goal is dissonant with his cognition that he did not reach it."[5] Through a similar model, Heider argues that a situation is balanced when the outcomes of an individual are in accord with what he deserves.[6] When imbalance occurs, the "ought force" dictates that events have to be corrected to restore balance. Taking off from Heider's balance model is Alexander and Simpson's treatment. They hold that injustice creates lack of balance by threatening the assumption of orderly and predictable relations of the individual with the environment. "When these definitions are disrupted or threatened, [the individual] finds this stressful; for even a reliably cruel world may be preferable to a chaotic one. . . ."[7]

Both Heider's reference to an "ought force" and Alexander and Simpson's orientation to the notion of predictability and order in social relations lead into the second source of motivation, the normative aspect. In its most clear-cut form, we have Gouldner's statement of the norm of reciprocity (of which distributive justice can be seen as a special case).[8] The norm of reciprocity creates an obligation to maintain equity in exchange.[9] Blau argues that the obligation will be honored because of the operation of two factors. First, to do otherwise makes the actors feel guilty, presumably because they have internalized reciprocity norms; and, second, because in any exchange situation a second type of exchange is present—an exchange of fairness for esteem.[10] As Homans discusses this problem, first justice becomes a value; then fair exchange becomes rewarding in itself.[11]

Both theoretical arguments and the available empirical findings, therefore, lead to the following predictions concerning the effects of distributive injustice:

Hypothesis I: In an exchange situation a person will try to redress injustice through affecting the distribution of rewards between himself and the other(s).

Victims versus beneficiaries.—As stated above, Hypothesis I is vague in that it fails to specify anything about the direction of injustice. But there are two types of injustice situations—one, where the person is the *victim* (i.e., his inputs exceed his rewards), and another where the

[5] Elliot Aronson, "The Effects of Effort on the Attractiveness of Rewarded and Unrewarded Stimuli," *Journal of Abnormal and Social Psychology*, LXIII (September, 1961), 375.

[6] Fritz Heider, *The Psychology of Interpersonal Relations* (New York: John Wiley & Sons, 1958), pp. 283–84.

[7] C. Norman Alexander, Jr., and Richard L. Simpson, "Balance Theory and Distributive Justice," *Sociological Inquiry*, XXXIV (Spring, 1964), 183–84.

[8] Alvin W. Gouldner, "The Norm of Reciprocity: A Preliminary Statement," *American Sociological Review*, XXV (April, 1960), 161–78.

[9] Muir and Weinstein provide evidence for the existence of such obligations and practices used to account them (see Donal E. Muir and Eugene A. Weinstein, "Norms of Social Obligation," *American Sociological Review*, XXVII [August, 1962], 532–39).

[10] Peter M. Blau, "Justice in Social Exchange," *Sociological Inquiry*, XXXIV (Spring, 1964), 193–206.

[11] Homans, *op. cit.*, p. 233.

person is the *beneficiary* (i.e., his rewards exceed his inputs). It is clear enough why a person will be upset and will show anger if he receives less than he should according to the rule of distributive justice and that he will be motivated to redress the injustice. But it is not at all self-evident why the beneficiary of an injustice situation will try to correct it and thereby contribute to the lowering of his own reward-cost ratio. But Jaques and Adams and Jacobsen have shown that the beneficiary does indeed redress imbalance.[12] Alexander and Simpson apply their cognitive balance model to an explanation of the beneficiary position.[13] They argue that people are motivated toward balance in their social comparison processes and that for balance to occur, a person's evaluation of himself, of his reward-to-cost ratio, and his perception of the reward-to-cost ratio of the other person must all be commensurate. Therefore the beneficiary will try to redress injustice and bring these back into line. From this argument we may derive modifications for Hypothesis I as follows:

Hypothesis IA: The victim will undertake to lower the rewards of the other person.

Hypothesis IB: The beneficiary will attempt to raise the rewards of the other person.

There are also evaluative consequences of injustice situations. It has been noted that esteem is one of the "goods" exchanged. Failure to provide a fair return of obligations implies social inferiority as evidenced by the evaluative content of the adjective "fair." As Homans points out, "Anyone who accepts from another a service he cannot repay in kind incurs inferiority as a cost of receiving the service. The esteem he gives the other he foregoes himself."[14] These considerations lead to another set of predictions:

Hypothesis IIA: In an exchange situation the beneficiary will grant esteem to (positively evaluate) the other person.

Hypothesis IIB: The victim will deny esteem to (negatively evaluate) the other person.

These hypotheses suggest that there will be attempts to redress imbalance in both directions; Homans raises the issue of whether both types of redress will be of equal magnitude.[15] While the victim will feel anger and the beneficiary guilt, Homans holds that the former will be given stronger expression than the latter. Simple hedonism would create some recalcitrance on the part of the beneficiary to lower his own reward level in order to give to the other. These arguments are summarized:

Hypothesis IIIA: The victim will claim greater rewards from the other to restore justice than the beneficiary will give. The affective parallel to this hypothesis is:

Hypothesis IIIB: The victim will withhold more esteem from the other than the beneficiary will grant.

Individual differences in redress.—Norms are differentially internalized by individuals: Distributive justice may be more of a demand on the self for some than for others. Further, perceptions of what constitutes costs and rewards, and the commensurability of each, can vary from individual to individual. In this study, we are concerned with accounting for such individual variation in redress of distributive injustice. To do so, we incorporate two personality variables, *Machiavellianism* and *Need for Approval,* into our analysis.

Machiavellianism, as used by Christie, is the orientation toward manipulation of one's fellow man as a means of achieving one's personal goals.[16] Those high in the trait (high Mach's) use strategies of duplicity, guile, and other opportunistic means, and, in general, they are not constrained in their behavior to the traditional canons of morality defined by the

[12] Jaques, *op. cit.*; and Adams and Jacobsen, *op. cit.*

[13] Alexander and Simpson, *op. cit.*, p. 189.

[14] Homans, *op. cit.*, p. 320.

[15] *Ibid.*, p. 76.

[16] Richard Christie and Florence Geis, *Studies in Machiavellianism* (forthcoming).

norms of the community.[17] From this we would expect that being the beneficiary of injustice would be less repugnant to the high Mach than to the low Mach, since the high Mach is more apt to ignore the norm of repayment which operates against his hedonic welfare. In an obverse manner we would expect him to be particularly distressed in the role of victim. Stated in terms of our variables of interest we have:

Hypothesis IVA: High Mach's will claim greater rewards from the other when they are victims than they will give when they are beneficiaries. They will also withhold more esteem when they are victims than they will grant when they are beneficiaries.

Hypothesis IVB: Low Mach's will give greater rewards to the other when they are beneficiaries than they will claim when they are victims. They will also grant more esteem when they are beneficiaries than they will withhold when they are victims.

Need for Approval, or the Social Desirability Response Tendency, has had a long history in the social-psychological literature. Especially noteworthy among the contributions are those of Edwards and of Crowne and Marlowe.[18] The latter refer to the relevance of their construct to individual behavior: "Social desirability . . . refers to a need for social approval and acceptance and the belief that this can be attained by means of culturally acceptable and appropriate behaviors. . . . A low need for social approval implies a degree of independence of cultural definitions of acceptable behavior."[19]

Since giving rewards and refraining from punishing are both means for achieving social approval, we may expect differences in style of redress depending upon the individual's Need for Approval. Those high in Need for Approval would be more inclined to reward others as a means of gathering favor and more hesitant to punish for fear of incurring the other's displeasure. On the other hand, since the approval of others is not as important to one low in Need for Approval, he would be less willing to trade his own benefits for such approval and have fewer qualms about punishing those who have taken from his just rewards. In terms of reward allocation this leads to:

Hypothesis VA: Persons high in Need for Approval will claim fewer rewards from the other when they are victims than they will give when they are beneficiaries.

Hypothesis VB: Persons low in Need for Approval will give fewer rewards to the other when they are beneficiaries than they will claim when they are victims.

For somewhat different reasons, we would expect the evaluations of the source of distributive injustice to parallel allocation of rewards in relation to Need for Approval. Those high in Need for Approval tend to be reluctant to express negative evaluations of others to a third party.[20] This is consistent with the social norm characterized in, "If you can't say something good about a person, don't say anything," and would interfere with their redress of injustice when they have received less than their fair share. Thus, we can state:

Hypothesis VIA: Persons high in Need for Approval will withhold less esteem from the other when they are the victims than they will grant when they are the beneficiaries.

Hypothesis VIB: Persons low in Need for Approval will grant less esteem to the other when they are the beneficiaries than they will withhold when they are the victims.

[17] Richard Christie, "Impersonal Interpersonal Orientations and Behavior" (unpublished manuscript, Columbia University, 1962); and Florence Geis, "Machiavellianism and the Manipulation of One's Fellow Man" (unpublished manuscript, New York University, 1965).

[18] Allen L. Edwards, *The Social Desirability Variable in Personality Assessment and Research* (New York: Dryden Press, 1957); and Douglas P. Crowne and David Marlowe, *The Approval Motive* (New York: John Wiley & Sons, 1964).

[19] David Marlowe and Douglas P. Crowne, "Social Desirability and Response to Perceived Situational Demands," *Journal of Consulting Psychology*, XXV (April, 1961), 109–10.

[20] See Jack B. Williams, "Nonverbal Feedback in the Dyad" (unpublished Master's thesis, Vanderbilt University, 1968).

METHOD

Sample.—The sample consisted of thirty-two students enrolled in a course in Introductory Sociology. Students in the course were administered a battery of personality inventories, including Machiavellianism and Need for Approval, prior to the experiment.[21] The available students were first divided according to sex. Then each sex group was divided at the median for Machiavellianism.[22] For each sex, the eight highest and eight lowest Mach students were chosen for participation in the study. This procedure provided four sets of eight students each: high Mach males, low Mach males, high Mach females, and low Mach females. After the data were collected it was found that the design was also balanced with respect to Need for Approval and the experimental conditions, making possible separate analyses incorporating each personality variable.

Experimental procedure.—The students who were chosen to participate in the experiment were told that they were to appear at arranged times to engage in a joint task with another student from a different section of Introductory Sociology and that the task would count toward their final grade in the course. When the student arrived at the appointed place he met another student, the first of two confederates of the investigator (Stooge 1).[23] The pair was told that their assignment was to help develop items for a questionnaire of student attitudes. For the first assignment, they were jointly to make up at least five items on fraternity and sorority life. The instructor left and Stooge 1 introduced the first experimental condition. He was programmed to do either a disproportionately large portion of their joint task, making up a majority of the items, or a disproportionately small portion of the work, displaying a cavalier attitude toward the endeavor.

After about ten minutes the instructor returned. He indicated that he needed some way to tell how much credit to give each member of the pair and asked that each student write down independently what percentage of the work he contributed. At this point the second experimental condition was introduced. The stooge was programmed to claim either two-thirds of the work as his, or one-third. The instructor read the percentages aloud *so that each knew what the other had claimed.* The instructor then assigned them to make up five more items on campus social life apart from fraternities and sororities. He left, giving them ten minutes to complete their second joint task. The stooge's work behavior was consistent over both sessions. At the end of the second work session the instructor returned and again asked the students to record what percentage of the joint work was theirs. He first indicated that these percentages would be averaged between the parties over the two work sessions to arrive at their individual grades.

The first stooge left and Stooge 2 was brought in from a waiting room and introduced to the student as his new work partner. The joint work sessions were then repeated with the new partner, but dealing with different attitude content areas. In this second set the student experienced levels of stooge work and claim opposite to those of the previous set. After both sets of work sessions had been completed, students were asked to fill out an "impression formation questionnaire" on each of their work partners.

The experimental design provided that each student receive each level of the two manipulated conditions, stooge's claim and work contribution, but only two of the four possible combinations. The study thus contained what can be regarded as one experimental and one control group. Stu-

[21] The Christie Mach IV instrument and the M-C Social Desirabiltiy Scale were used (see Christie, *op. cit.*; and Crowne and Marlowe, *op. cit.*).

[22] It has been frequently found that females tend to be less Machiavellian than males, as measured by the Mach IV instrument. Realizing this, it was considered wise to compare only within sexes. Therefore the groups were dichotomized about the individual sex medians.

[23] Both stooges were male.

dents in the experimental group each received two conditions of distributive injustice created by the claim of their partner, which was incommensurate with his level of contribution to the joint task. In one they were the *victim* (low work, high claim), in the other the *beneficiary* (high work, low claim) of injustice. Students in the control group received two conditions of distributive justice, one in which claims and work contribution level were both low, one in which they were both high. The four conditions are represented in Figure 1. Each of these groups was counterbalanced with respect to sex, Machiavellianism or Need for Approval, order of experimental conditions, and identity of stooges.

Dependent measures.—Our hypotheses are framed in terms of reward allocation

Stooge's Contribution	Stooge's Claim	
	⅓	⅔
High..........	Beneficiary	Justice-high
Low	Justice-low	Victim

Fig. 1

and esteem granted. Reward allocation was indexed by the percentage of credit the student claimed for himself in each work session. Esteem was measured on the basis of responses on the impression formation questionnaire. Items from the questionnaire were factor analyzed.[24] The first factor consisted of six pairs of polar adjectives, all highly evaluative in connotation.[25] Total scores on the six items constituted a measure of esteem granted, based on general positive evaluation of the other.

FINDINGS

Prior to investigating the major experimental effects, a series of "housekeeping" analyses were performed. Using t-tests, it was found that identity of stooge had no effect on the dependent measures. Analyses of variance were performed incorporating order of experimental conditions and order of stooge. These, too, fortunately, revealed that order had no material effects. Claim and evaluation data were correlated. The result, $r = .56$, justified separate analyses. Accordingly, analyses of variance were performed in order to test the hypotheses.

According to Hypotheses IA and IB, we would expect that in the second session the student would claim more percentage points for his own effort in the situation where he was the victim and claim fewer

TABLE 1

MEAN SUBJECT'S CLAIM IN SESSION TWO BY STOOGE'S CLAIM IN SESSION ONE AND STOOGE'S CONTRIBUTION LEVEL

Stooge's Contribution	Stooge's Claim: One-Third	Stooge's Claim: Two-Thirds	Group Mean
High.........	49.06	53.75	51.41
Low..........	70.94	69.25	70.10
Group mean	60.00	61.50	....

percentage points in the situation where he was the beneficiary. The means bearing on this hypothesis are shown in Table 1, where it can be observed that the results are not quite as predicted.[26] It is true that the beneficiary does claim significantly fewer points for himself (49.06). The victim, however, claims about the same as the student who received justice from the low contribution stooge (69.25 versus 70.94). Comparing the difference between stooge's

[24] A principle axis solution was obtained with a varimax rotation.

[25] The six pairs were: industrious-lazy, smart-stupid, tactful-untactful, stud-nub, likable-unlikable, and generous-selfish.

[26] For the main effect of stooge's contribution, $F (1, 72) = 294.81$; $p < .001$. For the interaction of session and stooge's claim, $F (1, 72) = 3.81$; $p < .05$. For purposes of clarity, only the second session data are being presented, since the effects of the stooge's claim took place after the end of the first session. In only one case was there a significant first session interaction effect of claim. This was with Machiavellianism. While likely to be spurious, we can speculate whether high Mach students peeked at what their partners were claiming and adjusted their claims accordingly.

claim group means with that for stooge's contribution shows the latter to be much the stronger effect.[27]

Machiavellianism tends to specify the relationship between distributive injustice and reward allocation. Table 2 contains the means for the experimental conditions by level of Machiavellianism.[28] An interesting pattern emerges. The predictions in Hypotheses IA and IB are confirmed for the low Mach's; both victims and beneficiaries redress injustice by allocating rewards to restore balance between effort and claim. But for a slight exception among beneficiaries, high Mach's do not redress imbalance at all. Indeed, the high Mach will penalize just claims when they come from the low contributor, taking nearly three-fourths of the credit for himself.

TABLE 2

MEAN SUBJECT'S CLAIM IN SESSION TWO BY STOOGE'S CLAIM IN SESSION ONE, STOOGE'S CONTRIBUTION LEVEL, AND MACHIAVELLIANISM

Stooge's Contribution	Stooge's Claim		Group Mean
	One-Third	Two-Thirds	
High Mach:			
High	54.00	56.88	55.44
Low	73.88	62.50	68.19
Group mean	63.94	59.69	....
Low Mach:			
High	44.13	50.63	47.38
Low	68.00	76.00	72.00
Group mean	56.06	63.31	....

Three significant relationships are added when Need for Approval is incorporated in the analysis of reward allocation data. Need for Approval has a main effect which stems from the much greater reluctance of those high in Need for Approval to claim the lion's share of the credit for themselves, the respective means being 56.53 and 64.75.[29] The significant relationships of Need for Approval jointly with each of the experimental conditions on reward allocation are found in Table 3.[30] It can be seen from the table that the high and low Need for Approval groups differ in their propensity to redress distributive imbalance in relation to their partner's claims. Hypotheses IA and IB work for those low in Need for Approval, but the opposite pattern appears for those high in Need for Approval.

TABLE 3

MEAN SUBJECT'S CLAIM IN SESSION TWO BY STOOGE'S CLAIM IN SESSION ONE, STOOGE'S CONTRIBUTION LEVEL, AND NEED FOR APPROVAL

Stooge's Contribution	Stooge's Claim		Group Mean
	One-Third	Two-Thirds	
High Need for Approval:			
High	50.25	47.50	48.88
Low	68.75	63.13	65.94
Group mean	59.50	55.31	....
Low Need for Approval:			
High	47.88	60.00	53.94
Low	73.13	75.38	74.25
Group mean	60.50	67.69	....

We turn now to the redress of imbalance through the granting of esteem. For evaluation of work partner, sharp differences are found for contribution level and for claim of stooge as shown in significant main effects.[31] The pattern of means found in Table 4 supports Hypotheses IIA and

[27] As compared to .005 for stooge's claim, ω^2 for contribution level is .514.

[28] For the interaction of Machiavellianism and the stooge's claim, $F(1, 72) = 18.59$; $p < .001$.

[29] $F(1, 24) = 14.36$; $p < .001$.

[30] For the interaction of Need for Approval and stooge's contribution, $F(1, 72) = 4.01$; $p < .05$. For the interaction with stooge's claim, $F(1, 72) = 4.29$; $p < .05$.

[31] For the contribution main effect, $F(1, 24) = 42.68$; $p < .001$. For the claim main effect, $F(1, 24) = 5.96$; $p < .02$.

IIB. Partners who victimize are rated more negatively, and altruistic partners are rated more positively than their justice counterparts. Again, stooge's contribution level accounts for more variance than his claim.[32] Exploration of the influence of personality variables yielded little of interest. Machiavellianism did not interact with either of the experimental conditions. Need for Approval interacted significantly with stooge's contribution level to produce the pattern found in Table 5.[33] Although the group averages for high and low Need for Approval are comparable, those low in the trait make much sharper distinctions in their evaluations than those high in the trait, especially in rewarding or punishing stooge's contribution level.

TABLE 4

MEAN EVALUATIVE RATING BY STOOGE'S CLAIM IN SESSION ONE AND STOOGE'S CONTRIBUTION LEVEL[a]

Stooge's Contribution	Stooge's Claim		Group Mean
	One-Third	Two-Thirds	
High	28.06	23.94	26.00
Low	18.13	15.31	16.72
Group mean	23.09	19.63	....

[a] The higher the rating the more positive the evaluation.

In the present experimental design, it was not possible to isolate the interaction of stooge's contribution and stooge's claim and equate this with the effects of distributive injustice. If, for example, the tendency to redress was equal for both victims and beneficiaries, there would be no interaction but only additive effects. Thus the components of this interaction must be explored by means of simple effects. In this exploration, given Hypotheses III, IV, V, and VI, we were particularly interested in whether there was a differential tendency to redress on the part of the victims when compared with beneficiaries. Accordingly, t-tests were computed comparing the absolute value of the difference between justice and injustice for beneficiaries with the analogous difference for victims. For example, reference to Table 4 shows that beneficiaries redress more than victims, the differences in means being

$$|23.94 - 28.06| = 4.12 \text{ and}$$
$$|18.13 - 15.31| = 2.82,$$

respectively. Table 6 summarizes the results. The data treat Hypothesis III rather roughly. It was predicted that the tendency to redress injustice would be greater for the victim than the beneficiary. For the total sample the opposite is true for both the reward allocation and the evaluation data. Nor does specification by personality variables clarify matters. Only in the evaluation data were directional hypotheses for personality variables confirmed, and then only if they were consistent with the general trend for beneficiaries to redress more than victims (low Mach's and high Need for Approval).

Sex-linked patterns in the redress of distributive injustice were found to interact

TABLE 5

MEAN EVALUATIVE RATING BY STOOGE'S CLAIM IN SESSION ONE, STOOGE'S CONTRIBUTION LEVEL, AND NEED FOR APPROVAL

Stooge's Contribution	Stooge's Claim		Group Mean
	One-Third	Two-Thirds	
High Need for Approval:			
High	25.63	22.25	23.94
Low	18.63	18.13	18.38
Group mean	22.13	20.19	....
Low Need for Approval:			
High	30.50	25.63	28.06
Low	17.63	12.50	15.06
Group mean	24.06	19.06	....

[32] As compared with .053 for his claim, ω^2 for stooge's contribution level is .443.

[33] In the analysis of variance, $F\ (1, 24) = 13.05$; $p < .002$.

significantly with the experimental conditions. In reward allocation, partner's contribution level was significantly more important as a criterion for females than for males.[34] Males gave more weight than females to their partner's previous claim when deciding how much credit to take for themselves.[35] The two sexes took their partner's level of work into account about equally in their evaluations of them. However, again, males were more sensitive to the claims their partners made. This meant that males were more active in redressing injustice (see Table 6). In fact, victimized females actually rewarded their work partner.

Those high in Need for Approval are hesitant to risk disapprobation. Our findings can be interpreted as reflecting this tendency. The individual high in Need for Approval claimed less credit for himself (and hence awarded more to the other), thereby avoiding being seen by his partner as overclaiming. It is only when the person high in the trait is certain of his partner's willingness to reward him (i.e., when he has been the beneficiary) that he is willing to increase his claim. Thus his patterns of reward allocation run counter to distributive justice, while the individual low in Need for Approval redresses imbalance when both victim and beneficiary.

TABLE 6

COMPARATIVE TENDENCY TO REDRESS INJUSTICE OF VICTIMS AND BENEFICIARIES

Group	Reward Allocation			Evaluation		
	Victim	Beneficiary	t	Victim	Beneficiary	t
Total	....	4.69	3.57***	2.82	4.12	2.06*
Male	2.00	8.75	4.56***	6.62	5.13	1.33
Female	....	0.63	0.26	....	3.12	2.23*
High Mach	....	2.88	1.08	2.37	3.00	0.44
Low Mach	8.00	6.50	0.66	3.25	5.25	1.62
High Need for Approval	....	....	....	0.50	3.38	2.15*
Low Need for Approval	2.25	12.12	3.51**	5.13	4.87	0.19

NOTE.—Blank cells indicate non-redress and are assigned value of zero in t-test.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

DISCUSSION

Consider, first, our results for individual difference variables. Certain factors, to be discussed later, operated to mask some of the predicted effects. There were, however, a number of strong relationships which fell outside the scope of our hypotheses. In most cases, the patterns which emerged made good interpretive sense.

Among our more intriguing findings is the tendency for males to redress distributive injustice and for females not to. This occurs because males differentiate their responses according to both their partner's contribution level and his claim; for the most part, females tend to ignore claim. In fact, in the case where they are victimized, females tend to give credit and positively evaluate the partner as compared to the justice counterpart. Several explanations may be offered. The female is likely to be accustomed to male exploitation in this type of encounter and therefore

[34] For the interaction of sex and stooge's contribution, $F(1, 72) = 4.76$; $p < .03$.

[35] For sex by stooge's claim, $F(1, 72) = 5.82$; $p < .02$.

pays little heed to his claim. Or she may perceive the victimizing partner as dependent on her, feel sorry, and reward him a bit. One wonders how these findings would have turned out if the stooge had been another female.

To be low in Machiavellianism means one is more prone to subordinate self-interest to abstract moral principles. This universalism expressed itself in the conformity of responses to the principle of distributive justice. Both in terms of reward allocation and evaluation, the low Mach's reward the worthy and punish the undeserving about equally. High Mach's are more particularistic in their responses to the situation. They opportunistically take points from partners reluctant to claim them for themselves and temper their demands when dealing with a partner who had previously requested the lion's share of the credit. In other words, high Mach's "play the game," while low Mach's apply justice norms.

Homans' central thesis, as tested in this paper, is reflected in Hypotheses IA and IB and IIA and IIB: When distributive injustice occurs, both in the case of the victim and of the beneficiary, it will be redressed in both reward allocation and in evaluation. The data provide strong support for three of these four hypotheses. Only in the case of the victim did the hypothesis for reward allocation fail to be supported. This brings up two related problems: Why was justice not redressed in the one case of reward allocation, and, since it was not, why *was* it redressed in the case of evaluation of partner?

One clue to our first question is the huge effect that partner's level of work has in the first session. Since the student took so many points from the low contribution stooge in the beginning, he really had very few more points to take in the second session. He would have to claim everything for himself to "even out." Therefore there was very little effect of the stooge's claim in the low contribution case. That the slight difference is in a direction counter to hypothesis is attributed to chance. The large effect of stooge's contribution that so thoroughly swamped the effect of stooge's claim can be seen as a result of redress of distributive injustice. In this case, behavior consistent with the rule of justice would mean contributing at least half the work in a joint task. By the nature of the jointness of the task and the rewards to both parties in simply getting the job done, we could expect that not putting in "one's fair share" would be a serious breach of the rule of justice. The findings bear this out by showing the severe punishment (in terms of reward allocation and evaluation) of those who slacked off, ***regardless of their claim.***

The question remains of why the injustice was redressed in the case of the victim in the student's evaluation. Although so many points had been taken from the low contributor that his claim could not cause further punishment, it is reasonable to expect the claim to have an "insult to injury" effect in the evaluation of him; injustice was compounded. Since the amount of esteem one can withhold is not limited in the same way as percentage points claimed, injustice was redressed in almost all cases for both victim and beneficiary.

A second notion of Homans' we put to the test was that of differential redress of injustice by victim and beneficiary. Our general expectation was that those who were victims of injustice would be more active in their attempts at restoring balance between rewards and costs than those who were its beneficiaries. But just the opposite was found for redress through the allocation of rewards. This appears to have been caused by the tendency, in the first session before partners' claims were known, to punish the slack-off stooge by claiming the bulk of the credit. Victimizing stooges thus got their just deserts at the time of first claim; there was no room to differentiate them further at the time of the second claim. It was possible, however, to compensate stooges whose generosity had previously been to one's own benefit.

There remains the question of why the beneficiary's greater liking of his partner than the victim's dislike of his. One possibility is the relative rarity of altruism of the kind confronted in our study. Self-interest can account for the behavior of the high claiming slack-off and might excuse it in part. Greater attention was focused on the altruistic behavior of the self-made victim. In its unusualness, altruism opens the possibility that it is an offer of friendship, or at least the expression of a desire to be seen in a favorable light. Such overtures were well rewarded in the perception of those who experienced them.

One may raise issues with the external validity of our research design. While the study may be defined as "laboratory research," it was not so from the perspective of the student participants. Post-experimental interviews with the students revealed a high level of veridicality for the experimental situation. That is, students *did* believe themselves to be in a real situation with real (and meaningful) rewards at stake. A second issue is that of the generality of findings from a fairly homogeneous group of university students. Have we merely characterized an aspect of the student subculture, particularly with respect to the tendency for beneficiaries to redress more than victims? We think not. Homans' reasoning regarding the beneficiary may have been too simplistic in relation to potential types of rewards and costs. The reasoning we advanced in support of the observed pattern is not logically restricted to student populations. Of course, such reasoning is very much *post hoc* and requires direct testing across new populations.

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LETTERS TO THE EDITOR

Comment on a Review of Schutz's *Phenomenology of the Social World*

In your issue of May, 1968, you published a review of the book by the late Alfred Schutz, *The Phenomenology of the Social World,* written by Herbert Lamm.[1] As a student of Schutz, I am impressed by the reviewer's grasp of the basic intentions of the author. As to his critical remarks, I grant him the privilege of his opinion. He writes that "the phenomenologists" should "bring about genuine communication" with representatives of Anglo-Saxon thought, such as John Dewey, instead of doing what he thinks they are doing, namely, indulging in sectarianism. The suggestion, of course, is laudable, should it apply. However, insofar as this criticism is directed against Schutz, it is grossly unfair. Lamm creates very misleading impressions on two counts. First, he does not inform his readers that the book is the translation of a work which was published more than thirty-five years ago in Vienna, written by a young Austrian who was steeped in the German *Geisteswissenschaften* but may have known not much more about Dewey than Dewey knew about Husserl. Second, he leaves his readers completely in the dark about the fact that Schutz, after his emigration to the United States in 1939, published more than thirty essays in various American journals, two of them in the *American Journal of Sociology.* In a number of these papers, he displayed a serious and respectful concern for the theories of such American thinkers as William James, George H. Mead, and—yes—John R. Dewey. If a thinker is to be judged, he should at least be judged by the total body of his writings and not merely by his first publication; and if he is charged on the basis of this publication only, the readers should at least be informed that the whole bulk of his mature work has not been brought into consideration.

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[1] Alfred Schutz, *The Phenomenology of the Social World* (Evanston, Ill.: Northwestern University Press, 1967), reviewed by Herbert Lamm in the *American Journal of Sociology,* LXXIII (1968), 779–80.

Comment on a Review of Glaser and Strauss's *Discovery of Grounded Theory*[1]

This is the first such note I have ever written so you may judge the depth of my irritation, which arises not from images of loss of sales or of readers' being persuaded away but from distress at being so misread. Glaser and I could not begin to tell you how many or basic are the errors in the review (there are two right in the first sentence). The book, as we know from having sent draft copies to many readers, arouses strong feelings pro or con and acts a little like a projective, forcing readers to take strong methodological stands themselves on some of the issues raised in the book; so I have no criticisms of Loubser on those counts. I would just like to have had him actually read the book with about four times as much care so as not entirely to misunderstand it. My chief complaint, however, is against the *Journal* itself for giving this book (its subtitle is *Strategies for Qualitative Research*) to someone who seems not to understand qualitative research and who I would guess, from his general stance and phrasing, does quantitative work primarily or entirely. In any case, he can read less accurately than any previous reader, quantitatively or qualitatively oriented.

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[1] Barney G. Glaser and Anselm Strauss, *The Discovery of Grounded Theory: Strategies for Qualitative Research* (Chicago: Aldine Publishing Co., 1967), reviewed by Jan J. Loubser in the *American Journal of Sociology,* LXXIII, (1967), 773–74.

BOOK REVIEWS

The Uses of Sociology. Edited by PAUL F. LAZARSFELD, WILLIAM H. SEWELL, and HAROLD L. WILENSKY. New York: Basic Books, 1967. Pp. xl+902.

People are starving. Cities are being burned. Violence and death have become organized into a way of life. Communication and compromise between nations falter. Nuclear war casts its shadow over all life on earth. Yet business goes on much as usual—perhaps better. Madison Avenue boys continue to sell soap, whiskey, and cigarettes; used-car salesmen still have their eyes on the fast buck; Hollywood grinds out pap in technicolor; and millions of comic strip readers daily absorb themselves in the inanities of "Dick Tracy." The suffering is hard to take, hard to watch. The widespread indifference to it is intolerable. In such a world, is it any wonder that there are reactions of withdrawal, absurdity, and protest? And is it any wonder that many of those who profess sociology—presumably including the study of the very acts and institutions that cause or alleviate suffering—are beginning to question the worth of much sociological endeavor?

For those who raise such questions, *The Uses of Sociology* is must reading, as it should be, as well, for every graduate student in sociology. This book is dedicated "to Robert S. Lynd, author of *Knowledge for What?*" Forty-four authors—nearly all well-known and highly respected sociologists—in an Introduction and thirty-one chapters, discuss the worth of sociology, its uses in the professions, in establishments, in solving social problems and formal planning, and in rapid social change. It is a timely book, reflecting the theme of the 1962 annual meetings of the American Sociological Association. If widely read, it should help to reduce the tension within the profession between the extreme—and, as they are often stated, irreconcilable—views concerning value-relevant and value-free (or value*less*) research. Paul F. Lazarsfeld, then president of the Association, wrote to all the session chairmen: "Doubt is often raised as to whether the rapidly mounting stream of empirical studies and the increasing number of publications on social theory have contributed to anything the educated citizen would find worthwhile. . . . We do not want to create expectations which we cannot fulfill, nor do we want to be excluded from the present surge of interest in scientific activities in the country. . . . I hope that something like a spectrum of utility will develop that leads from the use of social theory in neighboring fields, such as history and economics, at the one end, to the utilization of particular empirical research by agencies wanting to pursue a concrete goal more effectively, at the other end" (p. ix).

After the meetings, the ASA appointed the editors of this volume to prepare a book on the theme. In a detailed guide, the editors invited their authors to use their knowledge of particular fields to answer the broad question: "Where and how have sociological perspectives and findings penetrated modern society and with what effect?" (p. x). The answers give no solace to complacency as to the extent of sound sociological knowledge or its application. There is much room for improvement, but they do show a depth of competence, a wide range of uses, and a felicitous combination of rigorous research and humane and caring sensitivity. At least some of the doubts should be put to rest.

In their Introduction, the editors construct a paradigm for the understanding of the uses of sociology that focuses on the collaboration or lack of collaboration between client and sociologist. The major themes that they derive from this focus include the various roles of the sociologist in relation to the client, the resources at the disposal of the sociologist, different types of clients, the types of problems that confront the client, translation of a practical issue into a research problem, leaping the gap from knowledge to decision, the nature of the administrative setting of the sociologist-client relationship, and the general social context within which the use of sociological knowledge takes place. As the editors point out, "Most of the chapters in this vol-

ume deal with the *implementation* of goals," and the clients "are more apt to be organizations rather than individuals" (p. xiii).

"Sociology and the Other Social Sciences" leads off the volume. In it, Neil J. Smelser deals with the analytic, rather than applied, uses of sociology for economics, political science, anthropology, history, and psychology. Marvin Bressler in "Sociology and Collegiate General Education" gives some "illustrations of outcomes that might emerge from exposure to idea systems that are presumably transmitted only in sociology courses and by all sociologists" (p. 60). Nathan Glazer makes "a distinction between those who feel that microsociological studies are a social escape, those who wish sociologists were more concerned with revolutionary changes, and those who feel it is too early to be professionally concerned with macrosociological problems. . . ." He shows that "no position is immune from the charge that it conceals unexplicated ideological premises" (p. xxiv).

The uses of sociology in law (Hans Zeisel) in providing expert witnesses in court cases, including some excellent suggestions for lawyers who might wish to use a social scientist as an expert (Arnold M. Rose), in medicine (Martin D. Hyman), and in social work and social welfare, which includes general strategy for tapping social-science knowledge (Henry J. Meyer, Eugene Litwak, Edwin J. Thomas, and Robert D. Vinter), show promising beginnings and fruitful areas for further collaboration between sociology and the professions.

Abraham Zaleznik and Anne Jardin review the many uses of sociology in management. Raymond V. Bowers discusses the uses of sociology in the military establishment, David J. Bordua and Albert J. Reiss, Jr., in law enforcement, Neal Gross and Joshua A. Fishman in educational institutions, Charles Y. Glock and Francesco M. Nicosia in marketing, W. Phillips Davison in foreign policy, and Yehezkel Dror in public administration, with four case studies from Israel and the Netherlands. Mark Abrams discusses political parties and the polls, Herbert J. Gans urban poverty and social planning, Alfred J. Kahn the treatment of delinquency and community development, Elmer Luchterhand research and dilemmas in developing social programs, Eugene Litwak and Henry J. Meyer the school and the family, Harold L. Sheppard unemployment, manpower, and area development, Edward A. Suchman public health, and Gordon F. Streib and Harold L. Orbach discuss aging.

Three chapters on rapid social change deal with sociology in developing areas (Wilbert E. Moore), rural sociology (Charles P. Loomis and Zona Kemp Loomis), and sociology in the desegregation process (Thomas F. Pettigrew and Kurt W. Back). And the final section of the book, which is devoted to institutional problems in applied sociology, takes up ethical problems (Robert C. Angell), evaluating social action programs (Herbert H. Hyman and Charles R. Wright), a case study of resistance to sociological data (Judith R. Kramer), the sociologist in the non-academic setting (John W. Riley, Jr.), non-conventional uses of sociology as illustrated by peace research (Amitai Etzioni), and social accounting (Philip M. Hauser).

Although the different chapters, as one would expect, are not of equal relevance to the goals of the volume and are not equally well executed, the summaries of research and its utilization taken together are impressive. Perhaps the most crying need within the terms of reference of the volume is for research directed toward the evaluation of social action programs. In chapter after chapter, it is clear that the authors often have difficulty assessing just how some relevant research has been used, if at all, and, if it has been used, what its consequences have been. Without carefully planned evaluation research, effects seem mostly tangential, indirect, and elusive. Here, the excellent chapter by Hyman and Wright is of key importance. As they say, "Ours is an age of planned social action directed to the solution of every conceivable type of problem" (p. 741). "Evaluation is the methodological accompaniment to planned social action. By playing the accompaniment, the sociologist replaces the little experiments of his own making by the great experiments in social change that are underway" (p. 743).

Also, it seems clear that sociological knowledge has in most fields reached the stage where specialists in utilization are needed for effective and systematic application of such knowledge to action. Many of the authors show that many uses are more potential than actual and have been quite limited even when relevant knowledge is there to be used, if a "translator" or "communicator" had been available. And the federal government under present arrange-

ments seems to be using relatively little of the social research that it finances.

But even if government systematically supported evaluation research and used sociology more in reaching decisions, there is a danger that the volume (usually implicitly) reveals. It is a tendency for sociology and sociologists to be disproportionately mobilized for status quo and establishment purposes. The organizations and agencies that sponsor social research, serve as its clients, and sometimes use research in their actions have varying missions, but together most of them constitute a vast complex of complementary organizational needs. The needs of government, business, the military, trade unions, school systems, and the like—not the needs of men and women—define most of the problems and shape much of the research. Even where social change is the goal, such as community redevelopment, elimination of poverty, improvement of low-income housing, and increasing equality of educational opportunity, elite origins may incline new community action agencies and their demonstration researchers "toward elitist objectives, strategies, and tactics of change" (p. 507).

The status quo is served by the disuse of sociology as well as its use, as Pettigrew and Back illustrate for the case of desegregation. They cite some of the forces that prevented the sociological potential from being more fully utilized. Segregationist southern congressmen threatened both the tax-free status of private foundations and governmental financing of relevant social research. Some segregationists carried out organized boycotts against products identified with the foundations. And the nature of the problem itself did not seem to offer a way of avoiding the emotionally charged aspects of the issue. Thus, both private foundations and governmental agencies were timid about providing financial support for work in this "controversial" area. Also, in addition to financial restrictions, "the constraining atmosphere created by segregationist resistance has directly influenced sociologists, as it has foundations and governmental agencies" (p. 708). Thus, a stifling climate discouraged sociologists from fully testing the limits of research opportunities. Finally, Pettigrew and Back argue that in this instance there has been a sociological bias favoring static, segregation-maintaining analyses. In an examination of 255 intergroup relations papers published from 1900 through 1958 in twenty sociological journals, they find "a general and pronounced trend toward static description and concern with segregation" (p. 714). This emphasis on restrictive forces in race relations work in sociology and the neglect of dynamic concerns, such as the study of the Negro response, led to the "failure of sociology to foresee the present Negro protest, its form and growth, and its future direction" (p. 714).

The particular example may be overstated, but the general point, that sociology tends to be shaped by and to serve established orders, appears valid. This fact, however, is more complicated than it first seems. No simple guide is available to condone or condemn it, since the values that are served vary so widely. (Putting sociology to work to help cool off workers so as to keep unions out of private concerns is one thing and putting it to work to help achieve equal educational opportunities for Negro Americans is another; yet both may serve establishments equally.) Furthermore, no simple, satisfactory way of avoiding establishment bias seems clear. Certainly, identifying with the "underdog" may be a corrective, when one is needed, in some situations, but it introduces another bias and another distortion Underdogs, like overdogs, have no monopoly either on truth or goodness (see Alvin W. Gouldner, "The Sociologist as Partisan: Sociology and the Welfare State," *American Sociologist,* III [May, 1968], 103–16). To be aware and sophisticated about which side sociology supports is a step in the right direction. At least then a choice is possible. To have a commitment to humanity and world unity in sociological work that transcends petty partisanship is another. But we have few guidelines as yet that are specific enough to help the sociological researcher in his day-to-day activities.

People are starving. Cities are being burned. Nuclear war casts its shadow. And sociology—and I mean the *scientific* study of society—has some role to play in helping to solve the world's problems. This book shows that the record to date is much better than many people believe. It points to ways by which the utilization of sociological knowledge can be increased; it delineates areas of underdevelopment where more research is needed; and it suggests some ways of going about it. Let's roll up our sleeves and get on with it while there is still time.

WENDELL BELL

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Comparative Perspectives on Social Change. Edited by S. N. EISENSTADT. Boston: Little, Brown & Co., 1968. Pp. 279.

I have never understood why it was commonplace to state that either the study or the "theorizing" of social change was neglected. As S. N. Eisenstadt demonstrates again in this collection of essays, there is a rich body of literature on these questions from which to choose. Only very few of the selections included here have appeared in previously published readers devoted to social change.

Eisenstadt's volume also illustrates how lightly academic boundaries are to be treated. The material provided by political scientists, anthropologists, sociologists, economists, and others is of similar merit—disregarding the discipline of the reader.

Probably the most difficult requirement to meet is the "comparative perspectives" the title promises. To be truly comparative, a selection must not only deal with more than one social or cultural setting but also have at least something to offer to enhance our awareness and understanding of the differences and similarities between two or more such contexts. Surprisingly, many of the sixteen selections which make up this subreader answer this requirement. Eisenstadt's own article (reprinted from *American Anthropology*) moves freely back and forth, comparing the data about China, Japan, and India, and the theoretical conceptions which have assisted their modernization. Similarly, S. P. Huntington compares the modernization of America to that of Europe. W. J. Goode's and K. Davis' comparisons are truly global.

Eisenstadt provides the volume with an Introduction of his own, which is very much in line with what one expects to come from his pen: analytic, technically precise, sweeping in implication. Only the usual army of references is omitted. Short notes introduce the sections of the book and help place the articles in the context of other articles, without limiting the commentary to the comparative dimension.

This subreader will be useful to students of social change, especially those interested in modernization and those without access to a fair library.

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Criteria for Scientific Development: Public Policy and National Goals. Edited by EDWARD SHILS. Cambridge, Mass.: M.I.T. Press, 1968. Pp. 207. $8.95.

The Government of Science. By HARVEY BROOKS. Cambridge, Mass.: M.I.T. Press, 1968. Pp. 343. $10.00.

Social scientists should feel challenged by two new books in the science and public policy field. Both contain attempts to formulate rational public policies that would promote the development and utilization of science, and yet neither includes an extensive examination of the social science literature relevant to planning in this area. Any finding of fault with their authors for this omission is really a criticism of the sociologists of science and the students of public administration whose work is so uncompelling that it does not command the attention it deserves in the analysis of public policies toward science.

One book is a collection of articles from the first five years of the international journal of science and education policy, *Minerva,* assembled by its editor, Edward Shils. Among the fifteen articles are contributions by such leading practitioners and philosophers of science as Michael Polanyi, Alvin Weinberg, and Stephen Toulmin. Surprisingly, for a collection of independently prepared essays, there is a great deal of cohesion. In only the last five essays do the dialogue and debate among the contributors begin to falter and drift.

The central theme of the book is the same as that which Shils established for *Minerva*—the search for improved government science policies. Government has come to support science because of its usefulness in furthering a variety of social ends, such as defense, public health, and education. In the process of pursuing these ends, government decisions allocating funds to science affect its progress and its ability to serve society. Since the progress of science is thought to require free inquiry, there is a potential conflict between the needs of science and the needs of government. Reconciling the conflict by selecting criteria to guide government science allocations is the key policy issue.

The contributors approach various aspects of this issue. They establish and refine such important distinctions as that between basic research and technological development and that between big science and little science.

They compare national science allocations. Tentative solutions are offered and dissected. But, as Shils points out in his excellent Introduction, there are as yet no satisfactory principles upon which to base science policies, and the final result is a modification of extreme positions that advocate a single criterion or a universal organizational model for government science programs without an agreement or alternative criteria or models.

The other book is a collection of eleven papers and speeches written since 1960 by Harvey Brooks, dean of the Division of Engineering and Applied Physics at Harvard and, through his many advisory positions, a major architect of current government science policies. Composed of pieces prepared originally for diverse audiences and presented without significant editorial revision, the collection is somewhat repetitious and unfocused. Brooks, however, possesses both an intimate knowledge of government science programs and a unique ability to detach himself from the processes which establish them. Each essay is an insightful and balanced policy analysis.

The main theme again is the need for and the limits of government planning in science. Brooks adds considerably to the discussion available in the Shils collection by including analyses of the effect of government structure on the generation of policy alternatives and by being even more rigorous in his approach to the taxonomy of science policy. Moreover, he provides an appreciation of the virtues of existing policies that are often overlooked in the effort to correct their deficiencies. Particularly useful, for example, is Brooks's examination of the role that mission-oriented agencies have played in the development of American science. The end result, however, is much the same. We learn a great deal about the complexities of scientific choice and the pressing needs of science and government without gaining an unequivocal set of guidelines for government policies in science.

Even if it were possible to achieve an agreement on allocation criteria, there would remain two major obstacles to the formulation of rational science policies. First, as Brooks at least recognizes, we lack sufficient knowledge about the internal dynamics of science to play for the growth of science except on the basis of anecdotal evidence about the research process. Here a sociology of science that examines the development of entire fields of research and the influence of government action on the direction of science would have policy relevance. Second, we lack sufficient knowledge about the internal dynamics agencies assigned to guide the development of science. If the health of science and its contribution to society are to depend upon the behavior of government agencies, we should be as concerned with the influence of the bureaucracy on the actual application of the allocation criteria as we are with the selection of the allocation criteria. Here, studies in public administration that focus on the internal decision-making processes of government agencies would be useful.

Until attention is directed toward these obstacles, we are likely to be left with the kind of wisdom, admirably represented in these books, that clarifies terms and establishes categories but that cannot provide a theoretical basis for judging the impact of their recommendations.

HARVEY M. SAPOLSKY

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The Rise and Fall of Project Camelot: Studies in the Relationship between Social Science and Practical Politics. Edited by IRVING LOUIS HOROWITZ. Cambridge, Mass.: M.I.T. Press, 1967. Pp. 385. $2.95.

"American social science is in a crisis of ethics," Professor Kalman H. Silvert warned in the beginning of his trenchant analysis of the implications of Project Camelot, first published in *American Universities Field Service Reports* in July, 1965 (p. 80 in the work reviewed). Silvert then presented a carefully reasoned argument for scholarly reservations about direct government sponsorship of social science research. Many agreed, but some doubted, that a strong struggle would be raised in behalf of such a cause. Marshall Sahlins, for instance, told the American Anthropological Association in November, 1965, that "what had been for some time a cultural common-law marriage between scientific functionalism and the natural interest of a leading power in the status quo became under the aegis of Project Camelot an explicit and legitimate union" (p. 77).

Contrary to this expectation, sharp debate began to rage, questioning aspects of government-sponsored research, the intensity of

which caused Ithiel de Sola Pool to equate some charges made with academic "neo Mc-Carthyism" (p. 277), and Robert Boguslaw somewhat more modestly warned in the spring of 1966 that "otherwise scrupulously careful and responsible members of the profession are led to engage in recriminations and poorly thought through accusations that can readily become malicious in their effects if not in their intentions" (p. 126). Some early attempts within professional associations to develop hard and fast rules on scholarly relations to government have exposed unexpected pitfalls, which cause many to agree with Marshall Sahlins' conclusion that "we have no sanctions and cannot legislate ethics, and perhaps we should not try. For the moment, I favor the principle of letting each man learn to live with himself" (p. 79). This is exactly what hundreds of scholars are now conscientiously trying to do, and for them this book makes a profound contribution in exploring alternatives and pointing out ways of pursuing them.

The quotes just presented indicate that this work deals with the fundamentals of the current ethical debate within the social science profession and that the abortive, large-scale, army-sponsored, social science Project Camelot is merely the focal point for organizing the book, just as it has been one of the focal points in the professional ethical debate.

Louis Horowitz has generally done a judicious job of editing and provides an informative introduction of about forty pages. Unfortunately, Horowitz does not provide the volume with a concluding chapter which might have summarized and integrated the twenty some pieces by scholars, administrators, politicians, and journalists which comprise the book. Instead, he uses as an end piece a related and interesting paper he presented elsewhere for other purposes entitled "Social Science and Public Policy: Implications of Modern Research" (pp. 339–76).

Nevertheless, the pieces seem to relate to one another without much editorial help, and Horowitz does provide some useful conclusions in his introductory essay based on his wide reading and his interviewing of those connected with the project. Among his other findings is the observation that "in nearly every page of the working papers [the book includes twenty pages of the project's documents], there are assertions that clearly derive from American military policy objectives rather than the scientific methods." Although finding no ideological uniformity among social scientists participating in the project, he finds that the social science methodology employed is strongly conservative and hostile to radical change. He points out that one of the significant long-run effects of the project was that, with its demise, the Department of State, by presidential order, assumed the enormous power of censor over all government-sponsored overseas research. He bluntly accuses the American University of being a mere front for the army in this project in return for several hundred thousand dollars for administrative services (p. 25).

Horowitz also provides some suggestions or remedies, among them a prescription of more government grants and less contracted research and more individual research and less large-scale collective research.

Many of the criticisms of the project are spelled out by authors of other pieces in the collection, but one of the most glaring faults receives scant attention. The army's call to scholars to join the project clearly stated that its purpose was "to determine the feasibility of developing a general social systems model which would make it possible *to predict* and influence politically significant aspects of social change in the developing nations" [p. 47; my italics]. Not to admit that the power of prediction lies beyond the powers of social science seems highly questionable on the part of participants. Perhaps a rereading of David Hume would help us all.

Able arguments are also presented here by Robert Boguslaw, Ithiel de Sola Pool, and others favoring close co-operation between scholars and contracting government agencies. They emphasize the benefits to the scholar, government, and society from such co-operation and consultation, and cite instances of useful, independent research completed under government sponsorship. Not all, however, would share the enthusiasm of Ithiel de Sola Pool, who states: "The CIA, as its name implies, should be the central research organization to enable the federal government to understand the societies and cultures of the world. The fact that it uses as little social science as it does is deplorable. We should be demanding that they use us more."

More than one possible conclusion can be drawn from the rich materials in this useful book. But certainly it raises the strong possibility that many American social scientists,

by their own propensities, will be drawn into active participation in government actions which appear to other social scientists to be social work by the sword under the doctrine of assault with intent to uplift.

JAMES T. CROWN

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Children and Politics. By FRED I. GREENSTEIN. New Haven, Conn.: Yale University Press, 1965. Pp. 199. $1.75 (paper).

The Development of Political Attitudes in Children. By ROBERT D. HESS and JUDITH V. TORNEY. Chicago: Aldine Publishing Co., 1967. Pp. 288. $9.95.

The lapse of time between publication and review presents both a burden and an opportunity to an appraising scholar. A flood of "political socialization" research has been undertaken in the few years since Fred Greenstein's study of New Haven schoolchildren and the Chicago-based comparisons of several school populations made by Bob Hess and his colleagues. Where Greenstein found a benevolent political leader judged favorably through the eyes of the young and their allegiant parents, recent studies have revealed the impact of personal and social factors in a cumulative perception of malevolent political leadership. Where Hess saw political socialization as an indirect process of attaching positive political images to personal and institutional objects, more recent evidence indicates that there is considerable issue and political role content in the responses of American youth.

Nevertheless, the opportunity of reappraising these two path-breaking studies far outweighs the fact that their concrete, empirical findings may have been modified or negated. This is so because the contributions of Greenstein and Hess may now be placed within an intellectual context that they have helped to shape. I believe that the neophyte will profit most from reading chapters i, vii, and viii in *Children and Politics* because few scholars match Fred Greenstein's sense of intellectual and research significance within political personality studies. His discussion of "Avenues for Further Inquiry" contains many as yet unminded opportunities, in particular, the necessity of conducting long-term developmental studies from childhood to adulthood if we are ever to specify the political consequences of socialization. A timely use of the wealth of data in the Hess volume is further investigation of the relationships among peers, family, social class, teachers, and the school curriculum—variables that form a core of consistent analysis within the book.

Moreover, the *au courant* status of "political socialization" as a term of scholarly identification and interest requires that we explore two significant problems raised by the work of Greenstein and Hess. First, there is the import of personality development to the development of political learning. Both *Children and Politics* and *The Development of Political Attitudes in Children* are heavily grounded in political psychology, and much effort is spent in drawing political implications from cognitive and evaluative processes among the young. The wisdom of hindsight indicates that the net of personality may have been spread too widely and too haphazardly across the political domain. On this point, the reader would do well to follow *Children and Politics* with two essays by Greenstein, namely, "The Theories of Authoritarian and Democratic Character," in *The Annals of the American Academy of Political and Social Science* (September, 1965), and "The Impact of Personality on Politics," in *The American Political Science Review* (September, 1967). The thrust of these two statements is to modify the scope of personality in political development while sharpening the qualitative depth of its relevance to adult political behavior. The self-conscious effort is to specify the conditions and circumstances in which personal maturation bears political significance.

Second, these two volumes contribute to our understanding of the institutional base of political learning. Here, American youth is portrayed as basically apolitical, and thus the family, school, and political agencies indirectly shape orientations essentially ungrounded in personal political experience. For instance, although the schools are important political socialization agencies, Greenstein and Hess do not tell us that the school system is part of a power structure and that distributive civic education within the schools is an important political element. Thus, when Hess informs us that compliance with the rules of the school, rather than formal civic curriculum, provides the major source of political learning for lower-status pupils, surely that finding ought to be

connected to the political function of the school and its ability to influence the distribution of power, esteem, enlightenment, and participation among the adult population in the next generation. It is difficult to reconcile this latter view with the essentially passive, apolitical thrust of much political socialization literature.

The contributions of Greenstein and Hess can be assimilated by paying more attention to the politicizing of education (and of socialization) as a part of growing up in modern America. This is to say no more than that the passive, cognitive emphasis of apolitical learning by subject citizens needs to be augmented by the realities provided when viable educational and personal experiences influence active citizen roles at an early age. Perhaps, as Freud told us many years ago, the absence of vital functions (in this case political activity, not sex) can be traced to repressive material, including the material provided by society to achieve political socialization conducive to the current distribution of human values and resources.

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Contemporary Political Science: Toward Empirical Theory. Edited by ITHIEL DE SOLA POOL. New York: McGraw-Hill Co., 1967. Pp. xvi+276. $7.50.

In approaching a volume whose authors are the senior figures in the discipline, one is reminded of the return of Louis XVI to Paris from Versailles in 1791. Posters warned that "whoever applauds the King will be beaten. Whoever insults him will be hanged." So it is with reviewing these nine essays originally delivered as plenary session papers of the 1966 convention of the American Political Science Association. As Ithiel Pool, the plenary session organizer and volume editor explains, "The task set for each writer was to assess with reference to some particular field what the relation might be between political theory and what has been found by contemporary political science" (p. vi). But the resultant essays by Gabriel Almond, Robert Dahl, Karl Deutsch, Harry Eckstein, Heinz Eulau, Ithiel Pool, Lucian Pye, Edward Shils, and Herbert Simon give few answers and frequently fail to address themselves to the assignment. Nevertheless, in a curiously satisfying fashion, they raise a host of challenging issues fundamental to contemporary political science. Suffice it to mention but a few of these.

Professor Shils, the only non-political scientist of the contributors, notes that "political scientists are heirs to the oldest traditions of reflection and analyses of man's social life" (p. 266). But if these essays are representative, the traditions have yet to be translated into a viable body of theory which defines and analyzes the peculiarly "political." Rather, the discipline seems strangely parasitical—concepts, theories, and methodologies have been borrowed from the natural sciences and the other social sciences and applied to those aspects of the social (or psychological) process which the individual researcher chooses to identify as "political." Indeed, attempts to define the "essential" concerns of the discipline tend to remain unheeded. Thus, Gabriel Almond's 1966 presidential address to the APSA, included in this volume, urges political scientists not to foresake the "mission . . . passed onto us inescapably by our past and imposed on us by our present division of labor" (p. 18). For him, that mission entails the study of the problems of violence and coercion in human affairs. But none of the other authors, save two, deal with these issues and that version of our mission. (Karl Deutsch argues that the ultimate supremacy of integrative over nationalist trends is impossible without peace and "the renunciation of the national right to initiate or escalate warfare without an international mandate" (p. 221). Ithiel Pool examines the possibilities for stability and order while allowing for mass democratic participation.)

Fundamentally, then, these essays illuminate the essential intellectual anarchy of the discipline. (It may well be that the co-optive style of governance of the discipline organized is a response to this anarchy.)

Moreover, the essays illustrate how the increasing pervasiveness of the scientific ethos is both a symptom and a cause of that anarchy. Eckstein, for example, argues that tentativeness, specialization, conceptual and theoretical abstraction, and complexity are all aspects of the scientific culture. Indeed, the diverse pieces reflect these generalities. In the absence of any overarching theory or theories, the discipline promises to continue to fragment into even more narrowly circumscribed "fields." If some of the authors remain optimistic, equating our

current state with the preparadigmatic stage of Kuhn, this seems more a reflection of their hopes than of their critical observations.

Yet another essential problem of political science is present throughout these essays. The issue is first raised in the editor's Foreword: "Modern political scientists know that they should not confuse their preferences with their descriptions of the world" (p. xi). Consistently, however, many of these authors seem guilty of the opposite sin—confusing their descriptions of the real world with their preferences. Thus, Eulau posits that "because representative institutions did not work as preferred models of representation said they should work, the institutions were blamed rather than the models" (p. 54).

But it is not necessarily the case, as Eulau sees it, that the models should be blamed rather than the institutions. To brand this lack of fit as the "normative fallacy" assumes that all theories are amenable to similar modes of disproof. But strictly normative theories, obviously, are subject to entirely different sorts of logic than are empirical or abstract deductive models. Simon manifests much the same value distortion as does Eulau when he argues that empirical observation about American voters gives us a "realistic view of what the role of a citizen in a democracy can and should be" (p. 101). Aside from the appallingly culture-bound emphasis, the "empirical fallacy" here is only too clear.

Indeed, in tracing the historical fate of the separation of power theory and the unwillingness of political scientists to scuttle it when the evidences of its inadequacies had long been in, Almond reminds us of how non-empirical, culturally normative, and serendipitous our intellectual fathers have been. Are we less guilty of these faults?

The involvement of the political scientist in the solution of political problems is also raised here. Pool suggests that the political scientist consider himself an engineer or problem solver rather than a physicist, so that he can "bring to bear on the solution of a governmental problem whatever techniques are available to him from the bag of tricks of all the social sciences" (p. xii). But the engineer, as is commonly known of that profession, is ultimately non-judgmental, falling prey to Dahl's criticism that political scientists have abnegated their role of evaluating political systems. And yet, while none of the authors echoes Pool's plea for political engineering, virtually everyone has some policy position, at least a latent position, in his essay.

But here again there is cause for concern. Simon makes the point that professionals are not "neutral" or "value-free" but tend to enforce the values of their profession. Therefore, they become more reliable and predictable and can be accorded more responsibility. But just what are the values of the "profession" of political science? What are the normative concerns of the discipline vis-à-vis polity and society? What are its values vis-à-vis its own professional existence? From these essays, one so concerned derives few answers.

On another level, Pye reminds us that political science was virtually irrelevant to the problems of the new states—even less useful than the other social sciences. What, then, do political scientists have to offer to the political process? The answer is not obvious, nor do these essays offer positive insight. (Nor, in fact, do they provide a satisfactory answer to the problem which they were invited to face—the significance of political theory to modern political science, and vice versa.)

Rather, they illustrate that "the discipline" of political science is unified neither in terms of concepts nor in theories or methods; that it has yet to develop any of these which are unique to political science—although it has been successful in dispossessing us of many of our past illusions; and that its relevance to the policy process is questionable. As Eckstein has concluded, given that abstraction is the handmaiden of science and that such abstraction is basically incompatible with policy making, "it seems likely that political scientists least acculturated to the scientific culture get the most attentive political audience" (p. 160). For those political scientists committed to the systematic empirical investigation whose ultimate purpose is some conception of political, social, and economic justice, this thought and these essays offer little encouragement. Plaudits and insults should be dispensed with in the interests of getting on with the job.

Marvin Zonis

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Creativity and Performance in Industrial Organisation. By Andrew Crosby. New York: Barnes & Noble, 1968. Pp. xiii+197. $6.75.

Experiment in Industrial Democracy. By ALLAN FLANDERS, RUTH POMERANZ, and JOAN WOODWARD. London: Faber & Faber, 1968. Pp. 261.

Managing Organizational Innovation. By JEREMIAH J. O'CONNELL. Homewood, Ill.: Richard D. Irwin, Inc., 1968. Pp. x+199. $3.75.

Managerial Attitudes and Performance. By L. PORTER and E. LAWLER. Homewood, Ill.: Richard D. Irwin, Inc., 1968. Pp. viii+209. $5.75.

These four books have practically nothing in common except for their publication date and their use of English. Why this journal believes that one reviewer can grasp their essentials and render (for the price of one) an omnibus review which has at least the semblance of organization is a mystery to me and a disservice to the authors, who deserve more than this. Anyway, I have spent too much of this past hot August trying to design a gazebo capable of exposing the contents of these books and their possible interrelationships to the readers, and I throw in the towel.

Two of the books were written and published in England, though the author of one, Andrew Crosby, has returned to a teaching post in the College of Business Administration at the University of Tennessee. Essentially, he has written a longish essay (with chapter headings) on why organizations should be arranged so that creativity of the individual can be facilitated and maintained. He starts off from the correct notion that complex, modern organizations will be facing more and more "unprogrammed" tasks and that creative types will have to help solve them. A good half of the book has to do with creativity and why IT IS A GOOD THING, and the rest of the book shows how organizations stifle and blunt it. He ends with an unexceptional chapter on how organizations, through the proper implementation of social architecture, can release that creativity which lurks within all of us. The reader may be able to detect from my tone that this tune has been played before, oh so many times, and that the author's autonomous verbalizations sound at times like a McGregorian chant.

Experiment in Industrial Democracy, the other import, is a fascinating book. I was tempted to say "beautiful," but the authors have done their best, in typical British manner, to play down their results, clog their turgid prose with diagrams out of Stephen Potter which thoroughly confuse rather than clarify, and in general put you to sleep. But the high quality of their work and their careful observations and data-collection methods (primarily survey and interviews) coupled with their spectacular example of industrial democracy deserve the highest praise and a wide reading. One of the authors, incidentally, Joan Woodward, for years has been quietly building a solid empirical base plus a reputation for her writings. In this book she and her co-authors discovered a fifty-year-old example of industrial democracy in a chain of successful department stores. The John Lewis Partnership began in 1914 when John Spedan Lewis was determined to share profits (gain), knowledge, and power in his stores and found to his delight that this philosophy led to prosperity. This book describes the origins and vicissitudes of this experiment and evaluates its effects on such matters as the realization of democratic values, morale, understanding, performance and profits, communication, commitments, etc. As I said, it is a fascinating book, but beyond that, it is one of the few empirical evaluation examples of democracy at work.

Jeremiah J. O'Connell's book, *Managing Organizational Innovation,* is the first attempt, to my knowledge, made by a social scientist to observe systematically how a consultant team works with a client system toward implementing major organizational changes. As a non-participant observer, he was able to follow and record the work of a team of consultants from one of the world's largest firms and to reveal the social processes by which organizational roles were transformed from staff to line positions, no mean change, and what this transformation did to the client system in terms of performance and organizational relationships. The last three chapters contribute important ideas to a growing theory of planned change. In fact, I have had opportunities to read this book in manuscript form before it was published, and each successive reading brings out more interesting and subtle relationships between theories of *change* and newer theories of *changing*. In other words, O'Connell, along with others like Argyris, Gouldner, Blake, Shepard, and Schein, is moving toward a theory of applied social science.

The Porter-Lawler book is a lucid and authoritative effort, steeped in data, rich in

theory, and ripe for application. It focuses on managerial compensation, a surprisingly neglected subject in the annals of industrial psychology—witness our ignorance, for example, in the notorious arena of academic recruiting —and one that our authors have brought into shape with this splendid combination of theory and research. Along with the work of Herzberg and Vroom, this book will serve as one of the cornerstones for a theory of executive compensation. Using questionnaires across a wide variety of public service and industrial enterprises, the authors were able to deepen our understanding on a number of issues related to motivation at work: attitudes toward pay and some fascinating results with respect to "secrecy," the importance of pay as an incentive, the relationship between certain "role perceptions" ("other-directed versus inner-directed") and performance, the relationship of incentives other than pay with performance, etc. In all cases, they summarize previous research, account for their findings in the matrix of conflicting data, set forth their findings clearly, and reformulate their "expectancy" theory of motivation in light of their newer findings.

I cannot indorse all of their interpretations or inferences, most especially their refutation of Riesman's hypothesis that modern organization rewards "other-directed" leadership. They found that inner-directed leaders performed better and were more highly rewarded for their efforts. This finding is muddied by a loaded and culturally preferred definition of this character type along with the exclusive use of self-reports leading to the plausible explanation that other-directed leaders will see themselves in terms of the core values of the enterprise, which, in our society, are inner-directed values such as the ones the authors use to define "inner-directed": forceful, imaginative, independent, self-confident, and decisive. Who could resist? Nevertheless, this book integrates and clarifies an important area and in the process sheds new light on the complex problem of motivation at work.

Despite what I said in my opening sentence, I presume that all of these four volumes can be placed gingerly in the roomy and still badly defined field of "organizational theory." For several years now, both critics without and statesmen within this field have been worried about the lack of general theory and the pervasive "fragmentation" of effort. Generally, I suppose that this complaint is valid. For example, aside from Porter and Lawler, who are by training industrial social psychologists, I am not at all certain of the disciplinary origins of the authors. But I am beginning to wonder what difference it makes. Porter and Lawler tested elements and reformulated aspects of expectancy motivation theory; the Flanders *et al.* book on industrial democracy has implications for political theory; and O'Connell links up his findings with theories of planned social change. So, in all cases, the theoretical entry point is unique and feedback to the parent (and diverse) disciplines possible. Human organization becomes, then, simply a convenient crucible for the testing of hypotheses.

It may be that applied social sciences may revitalize their parent disciplines and that the line between "pure" and "applied" social science is illusory. The harsh confrontation between theory and practice may bring about newer models, not only capable of solving problems, but of substantially augmenting social theory.

Warren Bennis

University of Buffalo

Doctors' Strike: Medical Care and Conflict in Saskatchewan. By Robin F. Badgley and Samuel Wolfe. New York: Atherton Press, 1967. Pp. 201. $6.50.

The authors of this book never seemed to decide what would be the main framework for studying the twenty-three-day physicians' strike in Saskatchewan, Canada, in 1962. Consequently, several approaches emerge, resulting in a potpourri. The stupendous event itself —stupendous in that a liberal and proud profession resorted to trade union tactics—is obscured by questions of constitutional law and a sense of outrage. These constitute Part I, really the meat of the book, and should have ended there. Part II deals with "The Wider Setting," with chapter headings such as "Medical Care and Social Welfare," "Doctor and Society," and "Medical Care and Conflict." These profound subjects are dealt with in thirty-nine pages and include comparisons with the American, British, and Canadian health service systems.

The senior author, a sociologist, would, I assume, subscribe to W. I. Thomas' venerable dictum that if events are believed to be real, they are real in their consequences. The authors resort to self-evident explanations for the physicians' strike, such as belief in free enterprise, and a desire to control the market, on the one side, and inept drafting of the legislation so that physicians believed they were not permitted any private practice, on the other. The authors imply that all the actors seemed naïve about the social and political processes in a liberal, democratic society—and thus the fiasco occurred which gave rise to spectacular but true newspaper stories. The observation of the medical profession by the original chairman of the Medical Care Commission, which prepared the report leading to the legislation, is apt: "I have never encountered a group which has so little understanding of the normal social processes operating in our society" (p. 100).

The authors' observation of the government is pertinent: "We believe that the conflict's course could have been altered more decisively in favor of the position of the government, had the latter conducted itself more skillfully" (p. xii). Here would seem to be the polar ingredients of the strike action and of the attitudes of the physicians who emigrated from the British National Health Service, purported to comprise almost one-half of the physicians in practice in Saskatchewan. Physicians everywhere, in the Western countries at least, have accepted government intervention grudgingly. In Saskatchewan it took the most extreme form in its intensity.

So what we have, then, is a reportorial job which needs to go beyond observations such as the following: "During the prosperous 1950's many people in Saskatchewan acquired the trappings and attitudes of the middle class" (p. 107), this in response to the growing conservatism of the Co-operative Commonwealth Federation's economic policies; and, "From the evidence presented, it is apparent that no system for provision of medical services is or can be perfect" (p. 149)—shades of counsels of perfection for social institutions which are focuses of legitimate groups at interest. In Saskatchewan the pluralistic equilibrium broke. Maybe this should have been the analytic approach.

In any case, the physicians came back to work after the government agreed to allow options as to how the patient would pay the physician, but the principle of universal coverage remained intact, an issue which the physicians originally thought was crucial. A range of options to practice within the system was actually more important than the principle of universality. A new equilibrium emerged, but apparently not without scars which may take time to heal. The authors, however, believe that the most important issue was an interest group attempting to overturn the legitimacy of constitutional government. It would seem to me that this never occurred to the medical profession or, for that matter, to the government. The profession fought for its frontier concept of professional freedom—which includes the right to determine method and amount of payment as well as freedom to diagnose and treat—and the government wanted to remain in power after many years in office. The government had run out of popular issues when it stopped short of taking over the means of production and distribution, a policy implicit in CCF philosophy, but it had turned prosperous and "middle class." Hence, the stakes were probably artificially high on both sides, in that this was the last unfinished business of the CCF. It is also of interest that the CCF lost the election two years later, but presumably for reasons not related to the health insurance act. It had been in power for twenty years.

The book should be read by those interested in medical sociology and in the behavior of a profession when it feels threatened to its marrow. All social systems have their ethical breaking points, including the medical profession. The Saskatchewan physicians literally abandoned *their* patients (emergency services in hospitals notwithstanding) because the government would not let them engage in private practice for a fee. The government then apparently used the profession's ethical code to provide service no matter what as a club, and failed. The profession was given no acceptable options, short of practicing for nothing. It would seem that the authors should have dealt with some of these issues, but the constitutional question was uppermost in their minds.

ODIN W. ANDERSON

University of Chicago

The Origins of the National Health Service: The Medical Services of the New Poor Law, 1834–1871. By RUTH G. HODGKINSON. Los Angeles: University of California Press, 1967. Pp. 714. $17.50.

Patients and Their Doctors: A Study of General Practice. By ANN CARTWRIGHT. London: Routledge & Kegan Paul, 1967. 40*s*.

The Clinic Habit. By CAMILLE LAMBERT, HOWARD E. FREEMAN, and collaborators. New Haven, Conn.: College & University Press, 1967. $4.50.

There may be some logic in reviewing these three books as a group, in that they all deal with the problem of delivering health services; that they are produced by an economic historian, sociologist, and health practitioners in collaboration with sociologists; and, last, that they deal primarily with problems of public policy, that is, how to relate health services and people. Beyond these common attributes, however, they have to be reviewed quite separately, although they are individually of interest to students of health service systems and to the medical sociologist.

The book by the economic historian, R. G. Hodgkinson, *The Origins of the National Health Service: The Medical Services of the New Poor Law, 1834–1871*, is one of the most detailed and extensive historical compendiums of a medical era that I have ever read; it has over 714 large pages and is dated precisely 1834–71 to coincide with legislation having great bearing on the development of medical care for the poor in England. There is a fantastic amount of detail, such as charts of menus of food served in the poorhouse infirmaries (no wonder the death rate was 75 per cent) to long tables of rates of pay of health officers and the proportion of people on pauper lists in English counties. The chapters are topical, such as the "Poor Law Medical Officer," "Provision for Lunatics," "Self-help of the Labouring Classes (1) Medical Clubs, and (2) Self-supporting Dispensaries." And these topics are more or less repeated in two periods, 1834–47 and 1847–71. There is a mine of information for the sociologist who is interested in the historical sociology of medical care in England, but this long book is utterly without any redeeming feature as to an analysis of historical situational factors. Hence, the title, *The Origins of the National Health Service*, is a misnomer except in a very crude and thus historically meaningless sense. The last sentence in the book is certainly correct, but it should not take over seven hundred to demonstrate: "The National Health Service has its direct roots in the medical services of the Poor Law." This is, indeed, a clue as to why by common judgment the National Health Service has been underfinanced.

In a remote sort of way, the book by the British sociologist, Ann Cartwright, *Patients and Their Doctors: A Study of General Practice*, is a sequel to the foregoing by jumping over a continuous development in the intervening ninety-five years. The general practitioner in England today is largely the descendant of the Poor Law doctor who gave medical care for an over-all price paid by the Poor Law Guardians. The next stage is the general practitioner with a panel of patients established by the Health Insurance Act of 1911 for the working class, and then the general practitioner today, who is called "the fulcrum of the National Health Service." The National Health Service has attempted to institutionalize the general practitioner in an age of specialization as a gatekeeper for the hospitals and the hospital-based specialists, with the structural strains inherent in this role. Important as the general practitioner is, no systematic nationwide study (England and Wales) had been done until the one under review by Cartwright. Hence, it is a valuable contribution to the understanding of the functioning of the general practitioner in the National Health Service structure.

The study portrays the patterns of relationships between the public and the general practitioners—paraphrasing Cartwright. A random sample of almost one thousand four hundred people were interviewed in their homes with a wide range of questions on how they used and thought of general practitioners. Those interviewed mentioned 552 general practitioners, who were sent questionnaires by mail; 76 per cent responded. In addition to filling out the mailed questionnaire, sixty-nine general practitioners were interviewed personally. These had said at the end of the questionnaire that they would like to discuss conditions in general practice today. Information so gathered was used for illustration.

There is a great deal of information as to number of physician visits in a year, size of

panels, and attitudes toward doctors and patients. Suffice it to use Cartwright's own summary statement in her conclusions:

> If a plebiscite was held on whether patients wished to retain the general practitioner service or to change to a system in which front-line care was based on specialists and hospitals, there is little doubt that the result would be overwhelmingly in favour of the present arrangement. But behind the satisfaction of most patients there lies an uncritical acceptance and lack of discrimination which is conducive to stagnation and apathy. At the same time the data from the general practitioners themselves suggest that general practice has in a number of ways failed to adapt to new scientific and medical techniques and the social conditions of a National Health Service.

This book is highly recommended to students of medical sociology and medical care organization.

The third book, *The Clinic Habit*, by sociologists Camille Lambert and Howard Freeman in collaboration with professional health personnel, is a candid study of the use of a public health dental clinic by a target population generally in the low-income segment. It shows how utterly wrong common-sense predictions can be in public health practice as to the effect of public health programs on people's future behavior. In this regard, sociology may take the place of economics as the "dismal science" when it moves skilfully into health program evaluation.

The setting is an upper- and upper-middle-income area in Boston with a progressive health department. The research problem investigated was, as the authors stated, a comparatively simple one: "Is the early use of a publicly supported dental clinic for children from the ages of four to ten related to their dental condition at high school" (p. 19).

The study group consisted of 298 families with low incomes and with at least one child between five and eight years of age, and was selected by examining the school records of all children in the first four grades in the schools of one-half of the census tracts in the community. Interviews were completed with 265 of the mothers of these families by trained social workers using a precoded questionnaire. In 141 of the families, at least one child between five and eight used the dental clinic for at least twelve months, and in 118 families of the same general occupational status, the children of the same age group did not receive clinic services. After a full description of the methodology and practices of the data, the authors conclude that "we have examined the empirical question whether the use of a public dental clinic by young children results in improved dental health in adolescence. The answer is no" (p. 573).

The authórs generalize that the crucial difference between teen-agers in good dental condition and those in poor dental condition seems to be seeing any dentist regularly and having any necessary treatment as soon as possible. The study group learned, however, that teen-agers accustomed in childhood to receiving dental services from a public clinic are unlikely to turn to a private dentist when they are ineligible for clinic services because of age. The authors continue that the likelihood of a teen-ager being engaged in a preventive program is related to the dental and medical habits of his family, his life-style, in other words. Findings suggest that though many health programs are developed with objectives that are assumed to be congruent with the ethos of independence and so-called rational health behavior (the health man paralleling the economic man again), "the end result is likely to be failure." This is good sociological research bearing on public policy in health.

ODIN W. ANDERSON

University of Chicago

Culture and Aging. By MARGARET CLARK and BARBARA G. ANDERSON. Springfield, Ill.: Charles C Thomas, 1967. Pp. 478. $14.75.

M. Clark and B. G. Anderson's *Culture and Aging* makes evident to its readers the significance and importance of the cultural matrix in the process of aging in the United States. Culture is not regarded as a background issue, but rather as a constellation of active principles and forces in a complex psychocultural process. Two field anthropologists combine their efforts to apply the concepts of their discipline to the study of aging. The authors have framed the book with four types of reader in mind: (1) anthropologists and other social scientists, (2) planners for the aged in the United States, (3) clinicians who work with the elderly, and (4) the aged themselves. Each group will probably find that this book offers important new data and insights in un-

derstanding the phenomenon of growing old in America.

Aging is viewed as a cultural process. "The aged condition in human societies cannot be understood as a simple biological fact or as an accidental by-product of technological developments—it must be understood in terms of human culture" (p. 5). "When people arbitrarily retire from the responsibilities and economic activities of adult life, sometimes two decades before they experience serious functional impairment, a long hiatus [is] created into which we have yet to build new cultural traditions and social institutions" (p. 8). American aged face a normless condition where functional capacities are not congruent with cultural mandates. Four cultural factors are cited as creating a perplexing dilemma for the elderly. They are: (1) weak kinship ties, (2) rapid technological changes, (3) absolute and relative increases in the number of aged in America, and (4) the sacredness of productivity and independence.

This systematic and empiric study reaches its sharpest focus in posing the question (p. 393): "How can I [the senior citizen] grow old and still function, adjudging myself and perceiving how others judge me as a good American?" The investigators outline the strategy they employed in arriving at a set of five *adaptive tasks* which must be accomplished by the senior citizen if he is to find a satisfying answer. These tasks are: (1) *perception of aging and definition of limitations:* arriving at an awareness and acceptance of physical and mental change; (2) *redefinition of physical and social life space:* certain roles and activities must be relinquished in order to maintain optimal control over personal milieu; (3) *substitution of alternative sources of need gratification:* willingness, ability, and *accessibility* of new feasible pursuits; (4) *reassessment of criteria for evaluation of the self:* modify bases for self-judgment; and (5) *reintegration of values of life goals:* finding a new place in the larger scheme of things. *Adaptive tasks* constitute a process and are distinguished from *developmental tasks or adjustment*. Of the sample studied, approximately two-fifths are judged to have successfully accomplished these tasks. Adaptive task (3)—substitution of alternative sources of need gratification—presents the most difficult problem for the elderly. It should be noted that its accomplishment is most dependent upon the social system.

These findings, as well as many others, are based upon a general study of 435 individuals. Data from an intensive study of seventy-nine individuals drawn from the larger group are frequently introduced. The subjects are residents of the San Francisco Bay area. They are the survivors of a group of 1,134 old people selected in 1959 by the Langley Porter Institute Studies of Aging. The data to support inferences made by this study were generated by a *quantified context analysis* and *qualitative content analysis* applied to interview protocols.

The authors divided the general and intensive probability samples into three subgroups: (1) individuals hospitalized in psychiatric facilities, (2) a group recently released from psychiatric hospitalization, and (3) community residents. With the use of simple percentages, the subsample is compared on dimensions related to personal functioning, to the social system, and to the dynamics of the interrelation between the two. The study does not provide a sophisticated analytical statistical model.

The book presents an interesting anthropological view of mental health. "Mental disorder is a social judgement placed on an individual by various institutions in this society" (police officers, relatives, landlords, physicians, hospitals, court officials) (p. 21). The diagnosis of mental illness is described as a cultural operation. It is arrived at by consensus of interested persons.

Other points of interest are the "Social-Status Index" and the "Social-Introduction Index." Both are relatively simple measurement techniques which should be a methodological contribution to related studies. These are representative of methods the investigators employed to operationalize social science concepts for purposes of comparative assessment. It should be noted, also, that the findings of this study are in contrast, at several points, to the work of Cumming and Henry, and to the disengagement concept advanced in that study (Elaine Cumming and W. E. Henry, *Growing Old: The Process of Disengagement* [New York: Basic Books, Inc., 1961]). Instead, Clark and Anderson offer the concept of relaxation. Relaxation is seen as a release from social sanctions—"they (the adaptive elderly) speak gratefully of release from compulsion

to compete, to produce, to succeed, and to hurry. Problems do not loom so large, inconveniences are no longer so important, and one need not be so disturbed by them" (p. 202). The aged who can adapt appreciate certain compensations of old age.

This work offers important contributions to understanding the process of aging and is highly recommended for reading and study.

T. F. ZIMMERMAN

University of Missouri

Social Integration of the Aged. By IRVING ROSOW. New York: Free Press, 1967. xix+354. $8.95.

Rosow tests important propositions about the nature of age grading, friendship, kin, and neighbor relations in this society. He uses a panel design with four distinct residential densities of old persons in working-class, middle-class, and upper-class areas: some 65 per cent of the original sample survived to the third interview, a respectable performance.

Middle-class oldsters have more friends than the working class, and friends of working-class old persons are more often drawn from the neighborhood. Members of both social classes are affected by the density of old persons in their area, but as they age the middle classes undergo most change for they are forced into a local friendship network. Upper-middle-class people in apartment blocks with a high density of life peers, however, seem to respond differently and to maintain their isolation and reliance on purchasing professional help, rather than turning to neighbors, kin, or friends. So the social force of density explains only part of the results. This analysis of the complex world of aging reports that involvement with children and grandchildren, involvement with friends, and involvement with neighbors form independent social systems with distinctive functional roles. In long-term illness, for example, neighbors take care of more old people then do friends or family.

While all of us as we age will face a reduction of roles in all areas of life, this important book shows in detai the patterns of this reduction for different social classes and for differing life styles within each social class. It deserves to be read both for its content and for its elegant use of survey methodology.

JOHN MOGEY

Boston University

The Sociology of Marx. By HENRI LEFEBVRE. Translated by NORBERT GUTERMAN. New York: Pantheon Books, 1968. Pp. 214.

In a country where every intellectual worthy of the distinction has written a book on Marxism, Henri Lefebvre is widely regarded as one of the ruling triumvirate of Marx interpreters. The others are Maximilian Rubel and Louis Althusser. *The Sociology of Marx,* which is the first of Lefebvre's works to be translated into English, is an insightful attempt to construct a sociology from the writings of an admitted non-sociologist.

Marx's subject matter, according to Lefebvre, is "a totality in process of becoming and in its present stage of development, a totality comprising levels and aspects which are now complementary, now distinct, now contradictory. As such, his theory is not history, not sociology, not psychology, etc., but comprehends these approaches, these aspects, these various levels of the whole." Marx's approach to studying this totality proceeds through the complex relations between human activity and its various accomplishments in such a way that none of these parts are out of mind when attention is momentarily focused on the other. The rest of this totality finds its way into Marx's writings as the necessary preconditions and results, however far removed, of these fundamental ties.

Consequently, any attempt to impose academic boundaries on Marx's theories, to parcelize Marxism, distorts the real interaction of what has been included. This error is compounded by limiting the definitions of the concepts used to the same discipline to which one has confined the theory, whereas, in fact, one must pass through many disciplines to uncover the meaning of any of Marx's major concepts. It was the fruits of such partial and one-sided inquiries into society that Marx, himself, attacked as "abstractions." Still, most studies of Marxism, by friends and foes alike, have led to just such abstractions, to attributing to Marx limited and limiting formulas together with concepts that are too narrowly defined.

Aware of these pitfalls, Lefebvre does not try to build a sociology which lies to the side of Marx's economics, psychology, history, etc. His effort is directed, instead, to locating sociological aspects in Marx's theories and to making one of these aspects a new point of departure for examining the same totality with which Marx was concerned. Serving Lefebvre as the touchstone in this attempt is Marx's treatment of reality on the basis of its social forms and structures. Marx—it is often forgotten (even where it was once remembered) —did not grasp capital as the physical means of production, but as a social production relation or "form" of the relation between man's activity and its product in a particular historical period. Labor, commodity, landed property, profit, money, laws, ethical rules and values, etc., are likewise conceived of as forms of this same relation. As the interaction between activity and its product (viewed in their broadest senses) alters, so, too, do the forms in and through which this interaction manifests itself, requiring either the adoption of new concepts or an extension in the meanings of old ones. Lefebvre would have Marxist sociologists study the current interaction of these forms and how the various manifestations of the relation between activity and its product affect each other, with special attention paid to the practices that undermine them. Working with a cross-section of the present, his emphasis remains on change and development. The sociology Lefebvre finds in Marxism is a sociology of forms that has been weighted to obtain a better understanding of the role of revolutionary practice.

What are the relations between capital and labor, between commodities and contracts, between values and juridical principles, between class and ideology, and between all such forms and the daily, often contradictory, activity of men? In replying to such questions, Lefebvre lays great stress on the unity of social forms as elements that exhibit all the general features of a system. Moreover, he presents each form as being what it is not only by virtue of what the others are, but also by virtue of the no-boundary interaction occurring among the other forms. The relations between parts of a system which possesses no truly separate parts can only be internal relations. Capital and labor, for example, are not only forms of the current relation between man and his activity, but also forms of each other as necessarily interacting components of this relation. The fact that productive activity is alienated is part of the social production relation, capital, just as the fact that the product is alienated from the producers is part of the social production relation, labor. The forms, capital and labor, serve as alternative windows for looking out at the same process.

It is the use Lefebvre makes of Marx's dialectic—for this is where we have arrived—that accounts for both the triumphs and failures of his work: triumphs, because it enables him to offer well-balanced analyses of the forms at issue and to avoid dogmatic definitions. In this regard, we would single out his discussion of ideology as especially illuminating. The same dialectical outlook, however, causes Lefebvre to use terms and a manner of reasoning that most readers will find exceedingly obscure. Though Lefebvre elaborated his views on Marx's dialectic in an earlier study, *Logique formelle, logique dialectique*, the present work offers little aid to the uninitiated. For those unfamiliar with the dialectic (and not potted versions of the same), the value of *The Sociology of Marx* will be limited to the occasional, disconnected, though hardly inconsiderable, insights which they can cull. Yet, the chief value of this work is that it does present the sociology of Marx, but as an achievement available only to a few. It is to be hoped that the primers on Marxism with which the Anglo-Saxon world has been deluged has not rendered our intellectuals incapable of studying important though difficult books on this subject. This is one such book.

BERTELL OLLMAN

Progress and Disillusion: The Dialectics of Modern Society. By Raymond Aron. New York: Frederick A. Praeger, 1968. Pp. 230. $5.50.

Raymond Aron strikes American intellectuals as an overwhelming figure, a standing reproach to our departmental parochialisms. We tend to think that he has no equals among us. In truth, however, it is rather that we possess no exact parallel, save perhaps in the young Walter Lippmann. So he is now really quite *sui generis:* a journalist whose erudition graces the pages of *Figaro*, and at the same time a professor, sociologist, philosopher, political scientist, and historian of ideas

But faced with *Progress and Disillusion: The Dialectics of Modern Society,* one wonders why it was ever published in the first place. Well, it is a book written, so to speak, *à clef:* not, that is, for great political or intellectual purposes, but to celebrate on commission the two hundredth anniversary of that unique commercial enterprise known as *Encyclopaedia Britannica.* This was not reason enough! (Apparently, Aron has not seen fit to have it appear in France and in the original French. Moreover, this appalling rendering into English seems also to embarrass its translator, since one is nowhere noted in the text.) In this meandering, unfocused romp, or what one might charitably call this eclectic excursion through virtually all the subjects that have occupied his mind, Aron undertakes the creditable task of dealing with them as a whole, a very special attribute. This effort, then, is ambitious and full of risks, too many of which he falls victim to.

Aron shares the peculiarly intense French national penchant for paradox, a penchant naturally reinforced by the compulsion to evaluate everything. It is there, even in his title, derived, as he tells us (p. 216), from Georges Sorel's *Les illusions du progrès* (Paris, 1911). None of Sorel's animus to progress animates Aron; he is too much the Weberian (or Comptean) to have very much of an animus toward anything that has actually happened. What becomes history falls just a trifle short, in his view, of being validated as inevitability. He is far too modern and cautious to describe the social process as did William Hazlitt: "like the steps of Jacob's ladder in a bright and never-ending succession." But, with hardly less enthusiasm than Condorcet at the threshold of the famous tenth stage, Aron ushers in our historical era.

It is clear, though, that all is not well with Clio, or at least that Clio has been playing tricks on her unabashed lovers. One might despair of progress, then, in terms of angry abuse, as did Sorel, or make the leap, as also paradoxically Sorel was to do, into the enticement of revolution. Judicious temperament, no doubt, is what kept Aron from these obvious options and dubious fates. Aron's optimism, however, encounters its nemesis in the facts of the age. Productivity and the growing demand for justice are not necessarily coterminous. Aron, therefore, driving off hopelessness and embattled against the temptations of excessive hope, is reduced here to what one might call, to coin a barbarism, a limited *possibilist* positivism.

This is a generally welcome position. It partakes little of those secular versions of man's innate sinfulness which reigned triumphant among fashionable academics a decade ago; it is somewhat too contemptuous, but wisely skeptical of the radical infatuations with the third world and the virtuous potency of the poor in which fashionable academics are now caught up. Healthy and sound, one is tempted to say. Unhappily, the carefully buffeted structure of realistic circumspections is built on the condemned site of traditional and hardened Western political perceptions. Aron seems afflicted by a particularly virulent strain of the disease, i.e., enchantment with the U.S. Congress for Cultural Freedom version *circa* 1956. Were there not an occasional gloss of the contingencies, intellectual rigor mortis would not be too unfair a characterization of this synoptic and synthetic view.

This, then, being a conspectus, Aron is singularly blind to the demands of the psychological dimension on and against the political, economic, and social orders. The general French refusal to see the storms of individual and collective *Innerlichkeit* as defining categories in history is here manifest, the emphasis being always on structured institutions, in structured and even predictable relations to one another. In the West at least, where for many more people than ever before the ancient problem of scarcity has been transcended, precisely the stress of psyche and spirit will catalyze conflict, upset consensus, and confound institutional arrangements. The evidence for this looms up all around us, but the rebelliousness of the young in our universities and streets eludes the common understanding. It will not do to assume this to be either pathological or epiphenomenal. Most important, the insurgent assertions beyond materialism and political rights challenge the ingenuity and may defy the capacities of our institutions to adapt to the new requirements. How can a government respond to the demand of its people that it "make love not war"? How can governments really deal with problems of the quality of life? For the next longish period, only some will be beset by such quandaries. But those who are, are not likely to be able to resolve them by parliamentary compromise, and it is not even evident that conventional force could overwhelm these conventionally outclassed rebels.

Aron's projection of the eclipse of distinctively national history is not monolithic in contour,

nor is it anywhere near akin to the technological apocalypse which some serious writers have morbidly drawn from science fiction. But his tentative view of where industrial civilization is bound to take us is based on a dubious reading of its strengths. Aron overestimates the extent to which advanced societies have solved their internal problems of quantity. It is not yet clear that the capitalist side of the great convergence is as successful as we have been prone to think. Certainly that confidence is now being shaken as it has not been since the Great Depression. He, however, too casually assumes that the requisite adjustments can be made.

Aron, then, also underestimates the present and persistent capitalist essence of the American social system. Private property, he rightly argues, is not the state power, save, however, as he does not argue, in the crucial ultimate instance. His reading of corporate power as managerial, independent from property, is also only partly right. That is, the managers are independent of the vast numbers of insignificant stockholdings and holders. But on the major issue of the role of concentrated holdings and the pivotal concentration of these holdings in the over-all economy, he and the ideologists of the new corporation from Berle to Galbraith, on whose work he relies, are simply not borne out by the facts. And that is because they have failed to ask the right questions. In assessing American society, for example, concerned more with income distribution than we ordinarily are, we might well pose the question Aristotle commended to us a long time ago: *Cui bono?* With that as one of the central queries, the dialectics of modern society would emerge rather differently from the way they do in Aron's latest work.

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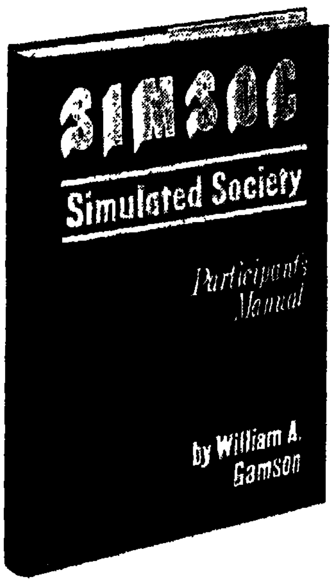
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The Factorial Ecology of Calcutta

Brian J. L. Berry and Philip H. Rees

ABSTRACT

Early traditions in urban ecology and social area analysis have recently converged in the approaches of factorial ecology. Comparison of factorial ecologies undertaken in recent years throughout the world enables a list of the necessary and sufficient conditions for ecological differentiation of urban subpopulations by social rank, stage in life cycle, and ethnic segregation to be specified. At the same time, a more traditional comparative literature suggests widely different ecologies of pre-industrial, industrial, and post-industrial cities. This study attempts to initiate systematic cross-cultural ecological analysis by means of a structured factorial ecology of Calcutta. The investigation reveals an interpenetration of pre-industrial and industrial ecological components, consistent with notions that the city is in some transitional developmental stage. At the same time, the changing significance of caste in ordering Indian society is revealed.

This paper represents a modest attempt to extend cross-cultural research in urban ecology, using a factorial study of the City of Calcutta as an instrument for drawing together diverse but related streams of thought. The extension is but prologue, however, for systematically executed comparative ecological analysis remains an unattained goal.

A VARIETY OF INSIGHTS

Relevant insights and intuitive analogies are many. For example, in his mid-nineteenth-century classic, Kohl devoted an entire chapter to the internal structure of cities. Urban structure, he said, might be viewed vertically as a series of layers as in Figure 1: (1) the ground floor (Rez-de-Chausée) contains the establishments and living quarters of the businessmen; (2) the first floor (Bel Etage) is the "area of wealth and pleasure," the seat of the nobility; whereas (3–5) "upwards" people with lower income reside; the same (a and b) applies to the subterranean levels. These layers, he continued, translate into "arches" of homogeneous social structure, so that the economic and social importance of the population declines as you move out from the center of the city. However, some outer rings might contain a wealthy population because of "a growing sympathy for the countryside."[1]

Parallels with this nineteenth-century European view can be found in studies of Indian cities. For example, Chatterjee concludes, in looking at the ecology of Calcutta's neighbor, Howrah: "The influence of the caste system is reflected in the usual concentration of the higher castes in the central areas of good residential localities, while the lower caste groups usually occupy the fringe. . . . The people still attach more importance to these centrally-situated residential areas. . . . Thus, in spite of the modern development of road transport, the residential decentralization or movement towards the fringe outside the old residential areas is not very marked."[2]

Sjoberg generalizes the pattern to all

[1] Johann Georg Kohl, *Der Verkehr und die Ansiedlung der Menschen in ihrer Abhängigkeit von der Gestaltung der Erdoberfläche* (Leipzig: Arnoldische Buchhandlung, 1st ed. 1841; 2d ed. 1950), pp. 181–85. We are indebted to Thomas G. Peucker for this example.

[2] A. B. Chatterjee, "Howrah: An Urban Study" (unpublished Ph.D. dissertation, University of London, 1960). See also Noel P. Gist, "The Ecology of Bangalore, India," in S. F. Fava (ed.), *Urbanism in World Perspective* (New York: Thomas Y. Crowell Co., 1968), pp. 177–88.

"pre-industrial" cities and contrasts it with "industrial" urbanization:

The feudal city's land use configuration is in many ways the reverse of that in the highly industrialized communities. The latter's advanced technology fosters, and is in turn furthered by, a high degree of social and spatial mobility that is inimical to any rigid social structure assigning persons, socially and ecologically, to special niches. . . .

[There are] three patterns of land use wherein the pre-industrial city contrasts sharply with the industrial type: 1) the pre-eminence of the "central" area over the periphery, especially as portrayed in the distribution of social classes, 2) certain finer spatial differences according to ethnic, occupational, and family ties, and 3) the low incidence of functional differentiation in other land use patterns.[3]

According to the classical papers of the Chicago School,[4] the ecology of the industrial city—Sjoberg's norm—stems from a process of *competition* between various population or interest groups in the city, the *dominance* of particular groups within the *natural areas* of the metropolitan community,[5] with change taking place by the *invasion* of a natural area by a competing group, and the ultimate *succession* to dominance of that group in the area.[6]

The process, the argument continues, derives energy from city growth, such that older population groups move from their original home to the periphery as their economic status improves, to be replaced at the city center by new arrivals. It results in Burgess' concentric zones of successively increasing status, subdivided into small natural areas on the basis of race ("the Black Belt"), or ethnicity ("Little Sicily"), or type of structure ("Residential Hotels"), as depicted in Figure 2.[7] It is this zonal result that Sjoberg contrasts with the pre-industrial form.

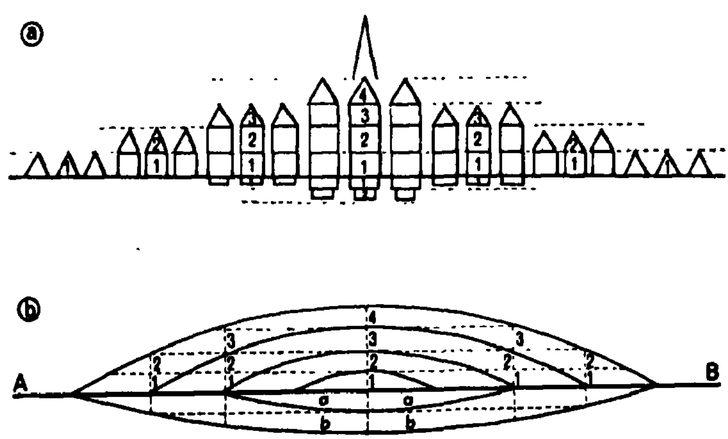


FIG. 1.—Kohl's concept of social space (after Peucker)

There have, of course, been alternative views of the spatial patterns or the determinants of the internal structure of the twentieth-century industrial city. Hoyt, for example, argued that high-rent and low-

[3] Gideon Sjoberg, *The Pre-Industrial City. Past and Present* (Glencoe: Free Press, 1960), esp. pp. 95–103. See Fava, *op. cit.*, for several other examples of this viewpoint.

[4] Robert E. Park, "Human Ecology," *American Journal of Sociology*, XLII (1936), 1–15.

[5] Harvey N. Zorbaugh, "The Natural Areas of the City," *Publications of the American Sociological Society*, XX (1926), 188–97.

[6] Philip H. Rees, "The Factorial Ecology of Metropolitan Chicago," forthcoming in Brian J. L. Berry and Frank E. Horton (eds.), *Geographic Perspectives on Urban Systems* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969). Rees's study was completed as an M.A. thesis at the University of Chicago in 1968.

[7] Rees, M.A. thesis, pp. 3–5. R. E. Park, E. W. Burgess, and R. D. McKenzie (eds.), *The City* (Chicago: University of Chicago Press, 1925).

rent neighborhoods occupied distinct subareas of the city and that these were not aligned concentrically about the city center but were distributed in a sectoral fashion. The spatial pattern of the city's rental areas was determined in Hoyt's view by the choice of those who could afford the highest rents. They pre-empt land along "the best existing transportation lines," "high ground—free from the risk of floods," and "land along lake, bay, river, and ocean fronts where such water fronts are not used for industry."[8] Others have suggested the significance of persistent cultural values attached to areas[9] and of "multiple nuclei."[10] Yet others have tried to identify and characterize new "post-industrial" ecologies characterizing contemporary metropolitan growth.[11]

[8] Homer Hoyt, *One Hundred Years of Land Values in Chicago* (Chicago: University of Chicago Press, 1933), and *The Structure and Growth of Residential Neighborhoods in American Cities* (Washington, D.C.: Federal Housing Administration, 1939), pp. 118–19.

[9] Walter Firey, *Land Use in Central Boston* (Cambridge: Harvard University Press, 1947).

[10] The "multiple-nuclei" concept was suggested by Chauncy D. Harris and Edward L. Ullman, "The Nature of Cities," *Annals of the American Academy of Political and Social Science*, CXLII (1945), 7–17.

[11] See John R. P. Friedmann and John Miller, "The Urban Field," *Journal of the American Institute of Planners*, XXXI (1965), 312–19.

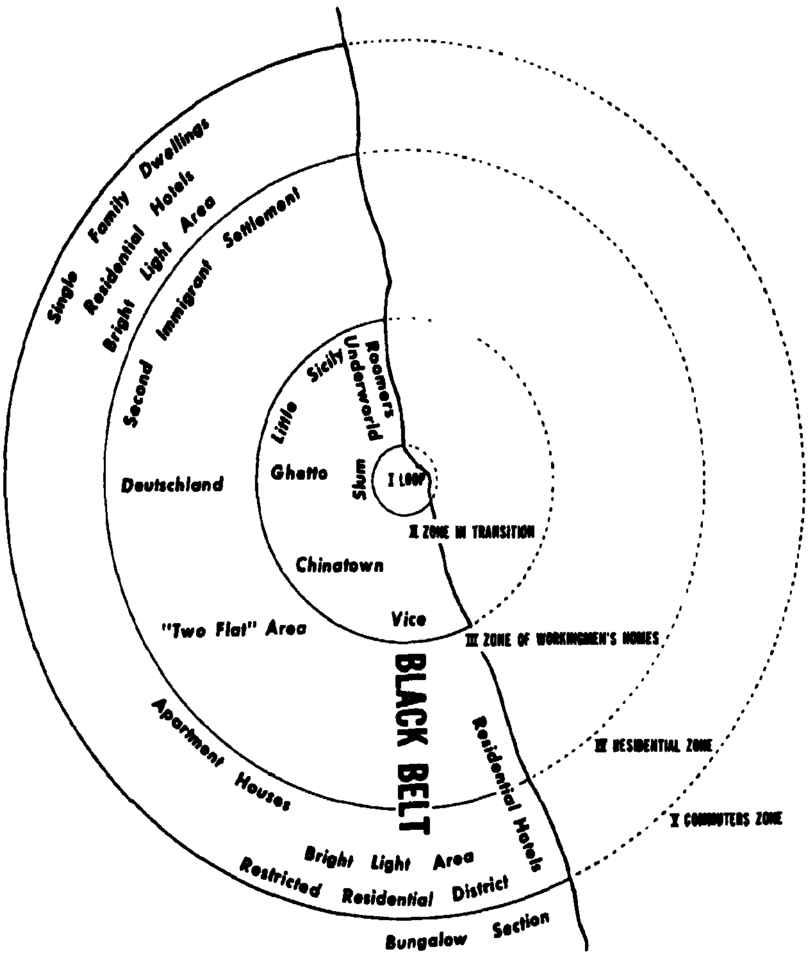


FIG. 2.—Burgess' zones and ethnic communities

BOSE'S ANALYSIS OF CALCUTTA'S ETHNIC DIVERSITY

The most important ecological study of Calcutta available to this time, by Bose, is phrased largely in terms of ethnic variability and cultural differences.[12] Bose depicts Calcutta as "the scene of a major confrontation between the enduring institutions of old India—her caste communities and diversity of ethnic heritages—and the pressures and values arising from the process of urbanization that pressages India's industrial revolution."[13]

Traditionally, Indian society was based upon caste: Brahman priests and teachers; Kshatriya warriors and rulers; Vaisya landowners, merchants, and money-lenders; Sudra cultivators, workers, and artisans; and the "untouchables" (today's "scheduled" castes) were the classical ideals in a complex system of more than 12,000 caste communities. Each was identified with occupation and maintained by intramarriage and spatial monopoly of particular trades in particular locales. Ranking of occupations into a hierarchy which generally coincided with ritual practices and residence of different castes in separate quarters maintained distinctiveness in the system and provided social content for the urban ecology.

It is this pattern that is being modified in Calcutta, India's largest city, according to Bose. The City of Calcutta is a legal unit of more than three million people set within a metropolitan area of seven million (Fig. 3). Created by the British during the eighteenth century, and once the seat of the British raj, it is today a leading industrial center, a major port, a national metropolis, and the focus for eastern India.[14] One quarter of the city's population arrived after 1947 as refugees from East Pakistan. Seventy-five per cent of the population lives in tenements or *bustees* (Fig. 4). More than half the multimember families have only one room to live in. There are three males to every two females in the city, and half the "households" are single men helping to support families remaining in villages elsewhere in India. Within the bustees there is but one water tap for every twenty-five persons and no means for disposing of sewage. Cholera is endemic. Life is often brief and brutal.[15]

Bose points out that Calcutta still houses many traditional communities (Figs. 5-10) for, although the city was a British creation, it was occupied by Indians who brought to it traditional Indian institutions. Among the Bengali-speaking Hindus, the upper castes (Brahman, Kayastha scribes, Vaidya physicians) originally lived in the northern half of the old "native quarter" at the heart of the city (wards 6, 8, 9, 10, 19, 20, 21; see Fig. 11), but today they have also moved southward into former European and Anglo-Indian residential areas. The middle commercial and artisan castes (Subarnabanik bankers, Gandhabanik spice merchants, Kansari brassworkers, Tantubanik weavers) were concentrated in the commercial segment of the native quarter to the north and east of Dalhousie Square (wards 21–27, 41–42). The lower "scheduled" castes (Jaliva fishermen, Hari and Dom bamboo workers and agricultural laborers, Dhopa washermen) lived around the outer eastern and northeastern fringes of the city. These fringes have, since partition, also become the residential areas of many castes of Bengali-speaking Hindu refugees from East Pakistan. The refugees also moved into many

[12] Nirmal Kumar Bose, "Calcutta: A Premature Metropolis," *Scientific American*, CCXIII (1965), 90–102, and *Calcutta, 1964. A Social Survey* (Calcutta: Lalvani Publishing House, 1968). Also S. N. Sen, *The City of Calcutta. A Socio-Economic Survey* (Calcutta: Bookland Private Ltd., 1960).

[13] Bose, "Calcutta: A Premature Metropolis," p. 91.

[14] Brian J. L. Berry, "Essays on Commodity Flows and the Spatial Structure of the Indian Economy" (Department of Geography Research Paper No. 111, University of Chicago, 1966). Also A. Ghosh, *Calcutta. The Primate City* (Monograph Series No. 2; Census of India, 1961); and Asok Mitra, *Calcutta. India's City* (Calcutta: New Age Publishers Private Ltd., 1963).

[15] S. N. De, "Cholera in Calcutta," *Cholera, Its Pathology and Pathogenesis* (London: Oliver and Boyd, 1961), chap. iii.

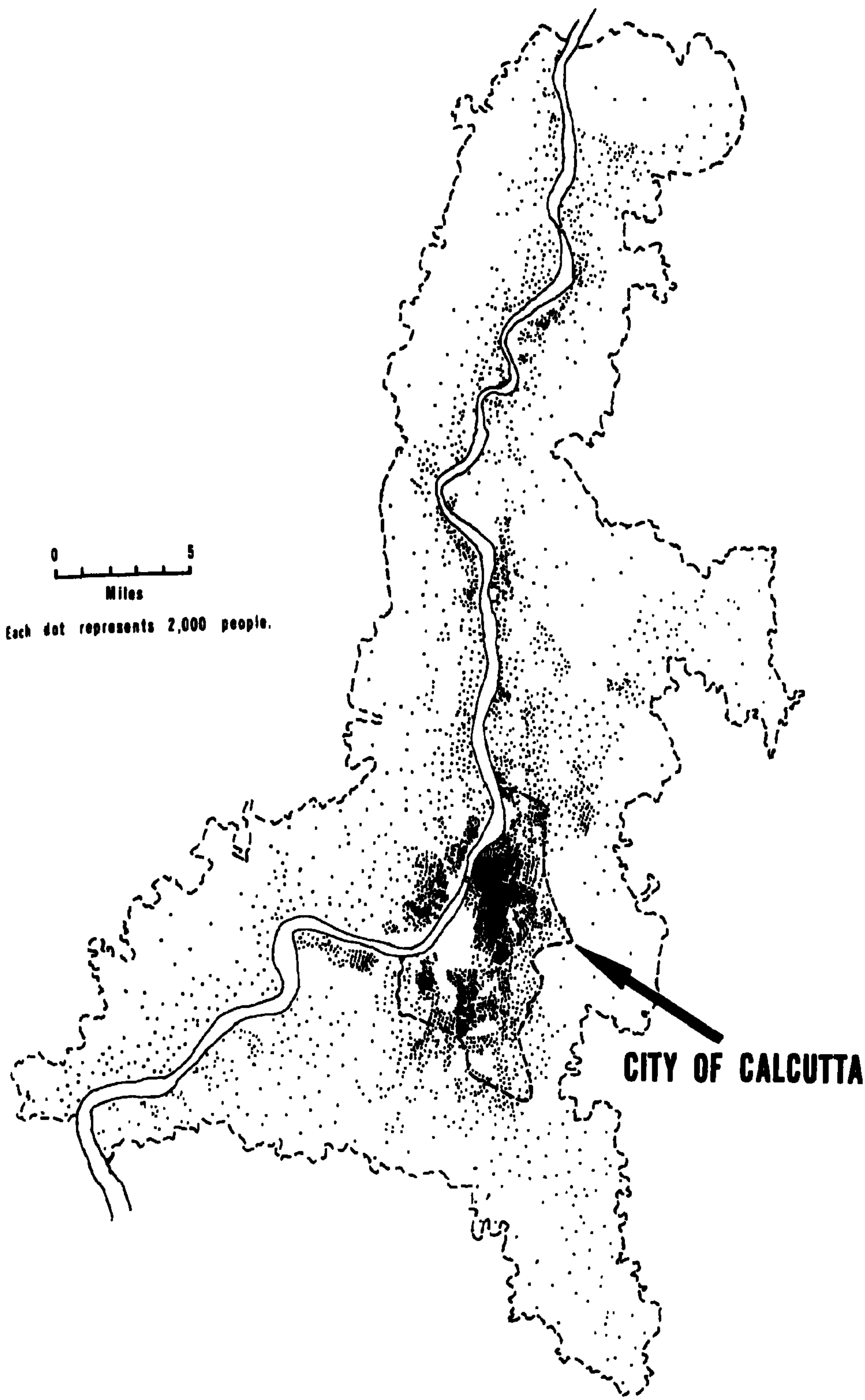


FIG. 3.—Population distribution in the Calcutta metropolitan district, 1961

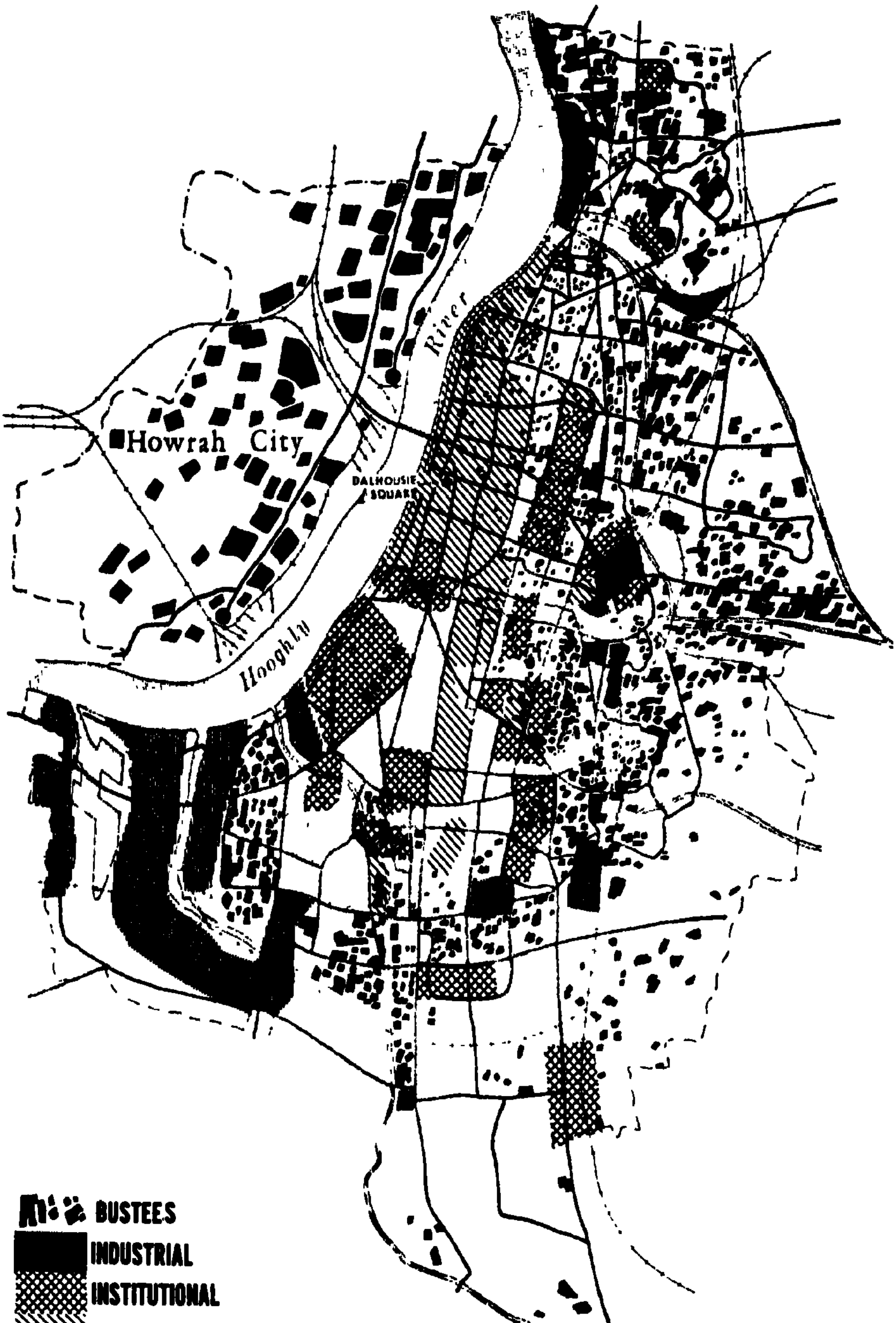


FIG. 4.—Dominant land use

formerly Muslim wards at the northern and southern extremities of the city.

Bose also describes the wide variety of non-Bengalis in the city. Oriya craftsmen can be found scattered in bustees throughout the Bengali wards. Hindi speakers from Uttar Pradesh supply porters for the carrying trade in the commercial wards (19, 22, etc.) and form the bulk of the industrial laborers elsewhere. Kalwar traders live alongside the Bengali commercial castes. The Rajasthanis and Marwaris traditionally occupied the commercial core (wards 24–26, 37, 39). South Indian immigrants are concentrated in wards 62–64.

The former European section was in the contiguous wards immediately east and south of the Maidan (a large park including old Fort William, stretching along the bank of the Hooghly River). The Europeans formerly occupied the uppermost levels of the city's social hierarchy. They lived in palatial houses with large gardens and open spaces, which they owned or rented from Bengali landlords. Their residences are now being bought by Rajasthanis, Punjabis, and other prosperous non-Bengalis. Anglo-Indians and Indian Christians used to be employed, under European patronage, in the railways, docks, and commercial establishments. Today the Indian Christians are occupationally indistinguishable from, say, other Bengalis if they are Bengali-speaking. The Anglo-Indians have been

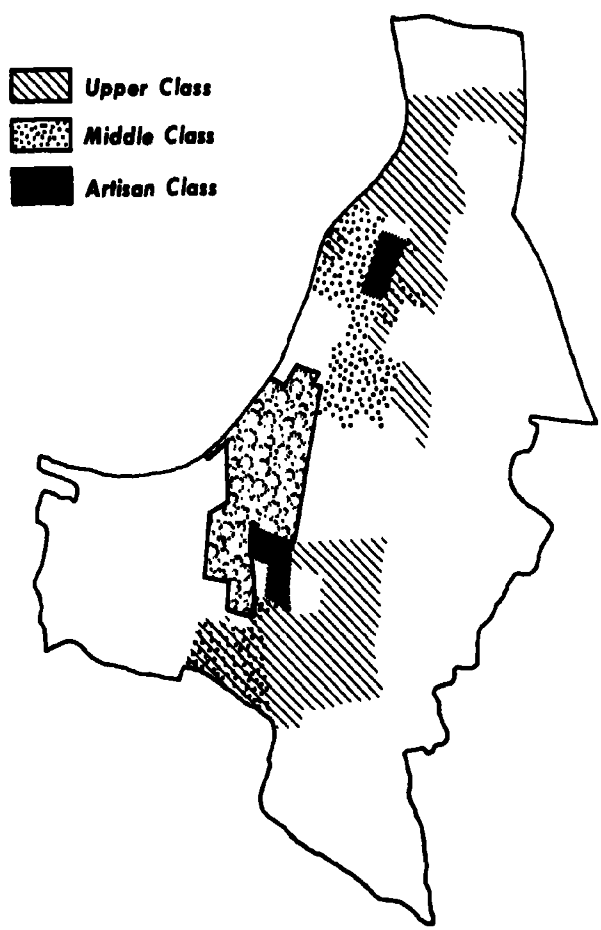


FIG. 5.—Bengali-speaking Hindus of upper, middle, and artisan classes (after Bose)

migrating away from the city and even from India.

The Muslim population, although it is not fractionated by caste, is quite explicitly stratified by class. Two large Muslim quarters surround the places of residence in the southwest and south of the city that were furnished by the East India Company to the Nawab of Oudh and the descendants of the Tippoo Sultan of Mysore. The Muslim commercial middle class lives in wards near the central business district; the lower-class Muslims live in the tenement and slum districts of the east and northeast and in large tracts of the city surrounding the old centers of the Muslim aristocracy in the south and southwest. Many lower-class Muslims used to be employed in the soap and leather industries, regarded among Hindus as "polluting" occupations and reserved to "low"-caste people.

These differences can be seen in myriad and minute ways in the daily life of the city. For example, Dutt shows, in a study of Calcutta's local markets, that

> the nature of commodities sold in a market depends largely on the community composition of the locality. For example, in Burrabazar Market, we find a large number of fruit stalls. Mechua Bazar Market has no fish or meat stall. This can be explained by the fact that the community composition of that particular area is of non-Bengali vegetarians. In the Muslim- and Christian-dominated localities there is a large

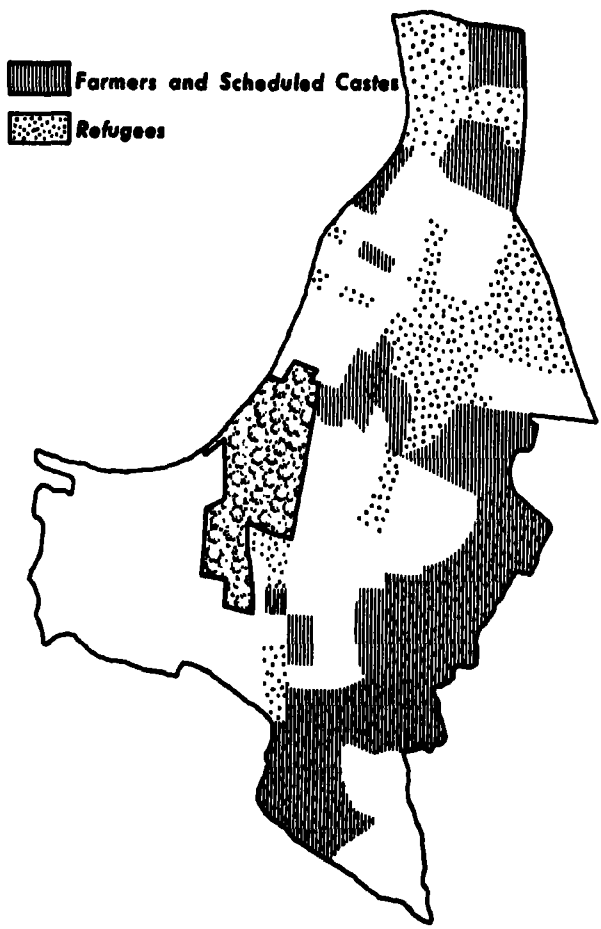


FIG. 6.—Bengali poor (after Bose)

concentration of beef-stalls in the markets. The examples are Rajabazar, Mullik Bazar, Taltola, Park Circus and New Market. In Bengali Hindu-dominated localities, fish stalls predominate. The examples are Shyambazar, Hatibagan, Sovabazar, Gariahat, Kasba, Lake, Bagbazar, Minshi Bazar markets. In the "mixed" localities which consist of Muslims, Christians, and Bengali Hindus, markets have stalls of vegetables and fruits on one side, and fish, meat and beef on the other. Such markets are New Market, College Street, Entally and Park Circus markets.[16]

Bose concludes:

The map of Calcutta thus shows a highly differentiated texture. Ethnic groups tend to cluster together in their own quarters. They are distinguished from one another not only by language and culture but also by broad differences in the way they make their living. Naturally there is a considerable amount of overlap, but this does not obscure the fact that each ethnic group tends to pursue a particular range of occupations.

It can be said, therefore, that the diverse ethnic groups in the population of the city have come to bear the same relation to one another as do the castes in India as a whole. They do not enjoy monopoly of occupation, as under caste, nor are they tied to one another by tradition in reciprocal exchange of goods and services. There is also no ritual grading of occupation into high and low. But preference for or avoidance of some kinds of work are expressed in class differences among occupations, as can be

[16] Ashok K. Dutt, "Daily Shopping in Calcutta," *Town Planning Review*, XXXVII (1966), 207–16.

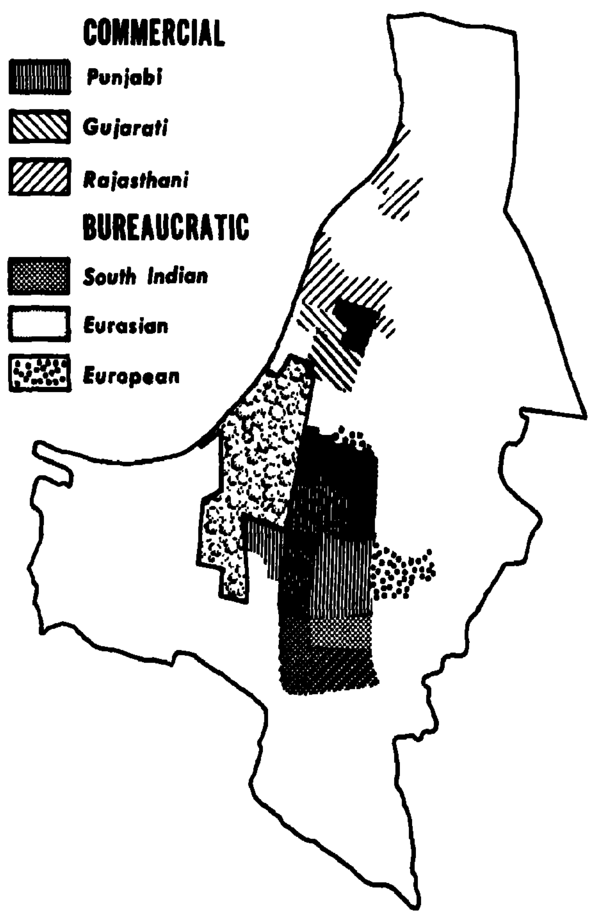


FIG. 7.—Non-Bengalis of the commercial and bureaucratic classes (after Bose)

observed elsewhere in the world. The social order of Calcutta might therefore seem to be evolving through a transitional stage, in which caste is being replaced by an increasingly distinct class system.

Actually, the superstructure that coheres the castes under the old order seems instead to be reestablishing itself in a new form. Calcutta today is far from being a melting pot on the model of cities in the U.S. There the Irish, Italian and eastern European immigrants have merged their identities within a few generations. The communal isolation of the first generations was quickly reduced by occupational mobility in the expanding American economy and by the uniform system of public education that Americanized their children.

In Calcutta, the economy is an economy of scarcity. Because there are not enough jobs to go around everyone clings as closely as possible to the occupation with which his ethnic group is identified and relies for economic support on those who speak his language, on his coreligionists, on members of his own caste and on fellow immigrants from the village or district from which he has come. By a backwash, reliance on earlier modes of group identification reinforces and perpetuates difference between ethnic groups.[17]

AN APPROACH VIA FACTORIAL ECOLOGY

If Calcutta is, as Bose argues, the scene of confrontation between traditional Indian institutions and industrial urbanization, then one should be able to see in its ecology not simply the ethnic diversity of region,

[17] Bose, "Calcutta: A Premature Metropolis."

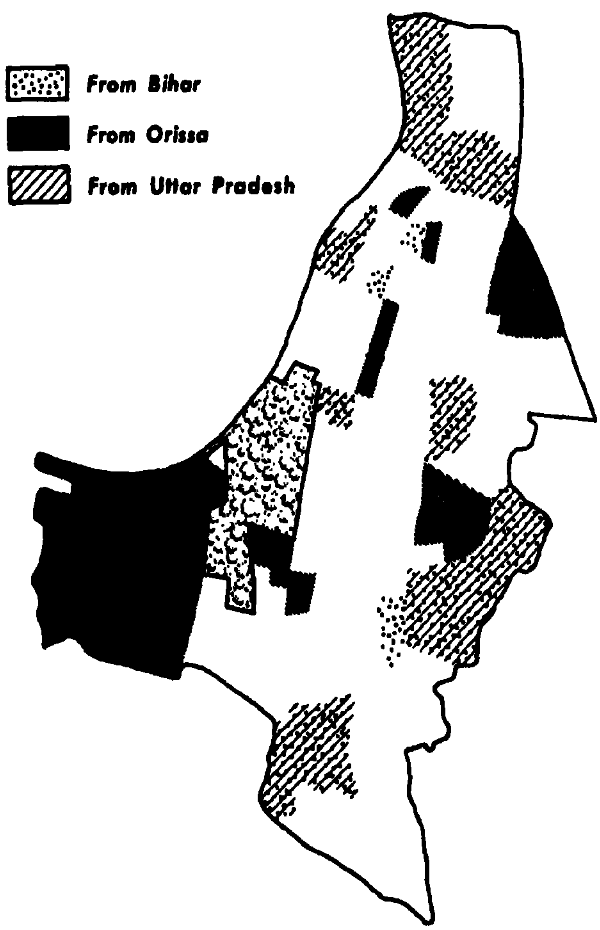


FIG. 8.—Non-Bengali poor (after Bose)

caste, and culture that he describes, but also the two further ecological themes of the preindustrial city categorized by Sjoberg (preeminence of center over periphery, low incidence of functional differentiation in land use[18]) and an increasing admixture of the ecological patterns of the industrial city. One way to determine whether this is the case is by assuming the analytic stance of factorial ecology.

Factorial ecology is an outgrowth of social area analysis.[19] Since there exists semantic confusion about this term, some clarification is required (Fig. 12).

[18] John E. Brush, "The Morphology of Indian Cities," in Roy Turner (ed.), *India's Urban Future* (Berkeley: University of California Press, 1962), pp. 57–70.

Social area analysis, sensu stricto.—The term "social area analysis, *sensu stricto*," applies only to that mode of analysis originally outlined by Shevky, Williams, and Bell in their studies of Los Angeles and San Francisco.[20] From a number of postulates

[19] The following sections represent a combination of lecture materials presented by Berry at the University of Chicago in a course (Winter, 1968) in urban studies, and their recording and development by Rees, *op. cit.*

[20] Eshref Shevky and Marianne Williams, *The Social Areas of Los Angeles: Analysis and Typology* (Berkeley: University of California Press, 1949); Wendell Bell, "The Social Areas of the San Francisco Bay Region," *American Sociological Review*, XVIII (1953), 29–47; Eshref Shevky and Wendell Bell, *Social Area Analysis: Theory, Illustrative Ap-*

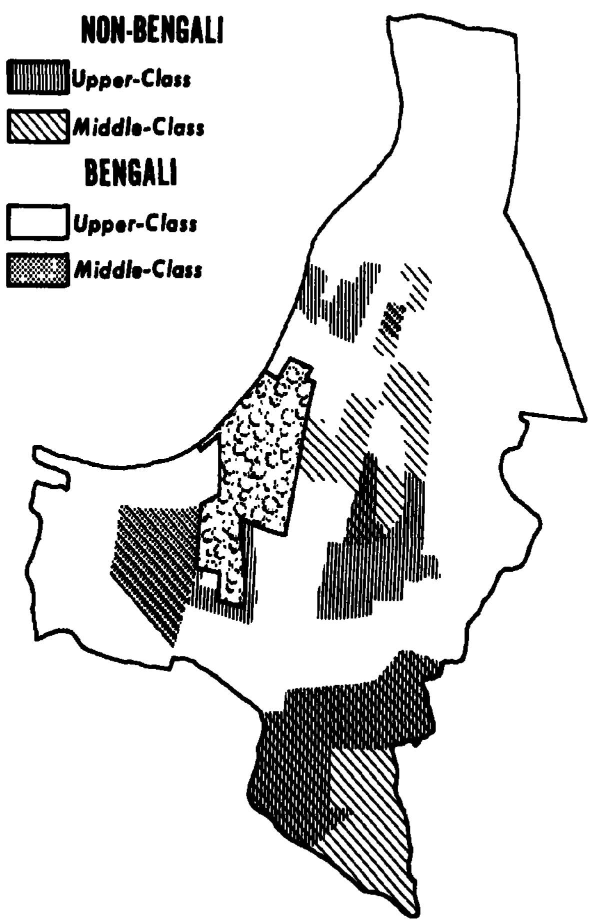


FIG. 9.—Upper- and middle-class Muslims (after Bose)

they derived three basic constructs which they considered describe the way in which urban populations are differentiated in industrial society. The three constructs were called *social rank* (economic status), *urbanization* (family status), and *segregation* (ethnic status).[21] They then proposed three indexes, one per construct, each made up of from one to three census variables, designed to measure the position of census tract populations on scales of economic, family, and ethnic status. The analysis also made possible the classification of census tracts into social areas based upon their scores on the indexes.[22]

Social area analysis, *sensu stricto*, has been criticized both on theoretical grounds (the theory underlying the constructs) and for empirical reasons (the method of dimensioning the constructs).[23] Bell attempted to meet the empirical objections that the social area analysts selected measures on the assumption that the constructs were correct

plication, and Computational Procedures (Stanford, Calif.: Stanford University Press, 1955).

[21] The terms in parentheses are Bell's; the preceding terms are Shevky's.

[22] The methods of construction are detailed in Shevky and Bell, *op cit.*, pp. 54–58.

[23] Amos Hawley and Otis Dudley Duncan, "Social Area Analysis: A Critical Appraisal," *Land Economics*, XXXIII (1957), 337–45; Otis Dudley Duncan, untitled review of *Social Area Analysis* by Eshref Shevky and Wendell Bell, *American Journal of Sociology*, LXI (1955), 84–85

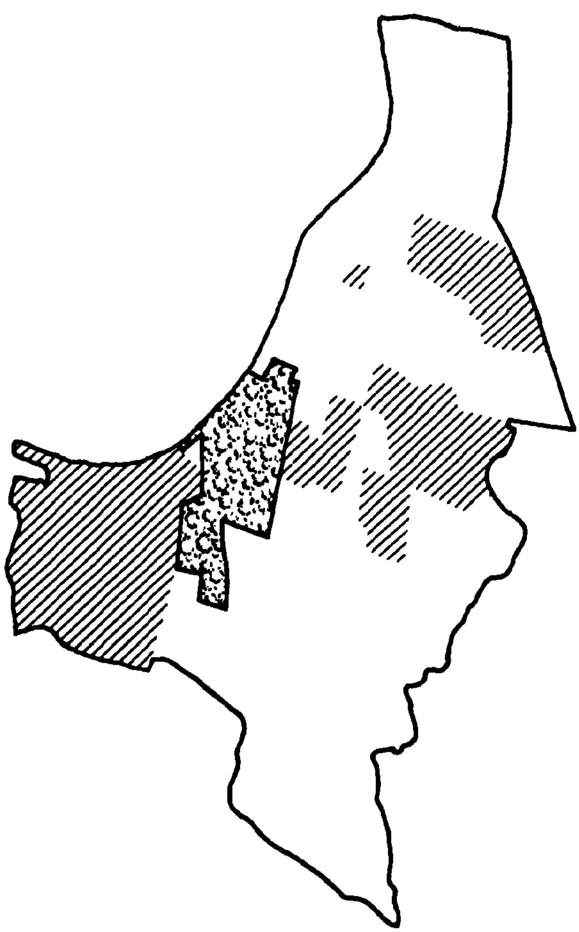


FIG. 10.—Lower-class Muslims (after Bose)

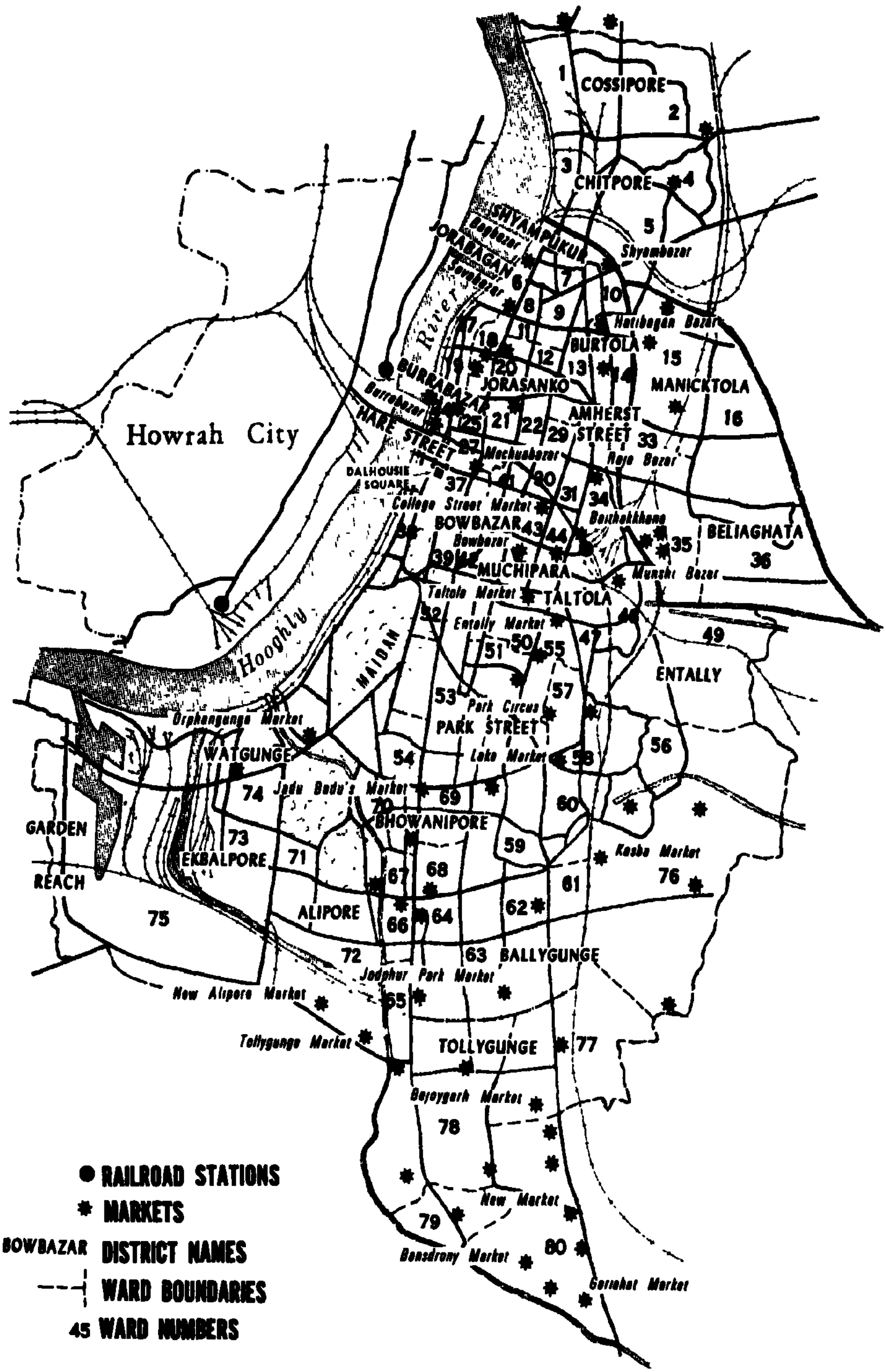


FIG. 11.—Wards, districts, and markets

but failed to provide a test of their validity. He used factor analysis to show that, in the cases of Los Angeles and San Francisco, the census measures selected to construct the indexes in fact formed a structure consistent with Shevky's formulations.[24] Van Arsdol, Camilleri, and Schmid went on to extend Bell's test of the Shevky model to some ten cities, six of which confirmed the validity of the Shevky indexes as measures of his constructs.[25] The fact that four cities did not conform to Shevky's constructs suggested, however, that the existence of the constructs should be left as an empirical question to be determined by the patterns in the variables rather than one to be assumed correct, a priori. The logical extension of this argument is that many more variables detailing the way in which census tract populations vary according to socioeconomic characteristics should be included in any study and that factor analysis should be used to isolate the fundamental patterns of variation in the data, be they Shevky-Bell construct patterns or otherwise.[26] Whether this viewpoint is valid may be debatable (see n. 23), but nontheless it constitutes the premise on which the balance of this paper is founded.

Factorial ecology.—"Factorial ecology" is the term now used to characterize studies involving this application of factor analysis to ecological study. A data matrix is analyzed containing measurements on m variables for each of n units of observation (census tracts, wards . . .), with the intent of (1) identifying and summarizing the common patterns of variability of the m variables in a smaller number of independent dimensions, r, that additively reproduce this common variance; and (2) examining the patterns of scores of each of the n observational units on each of the r dimensions.[27]

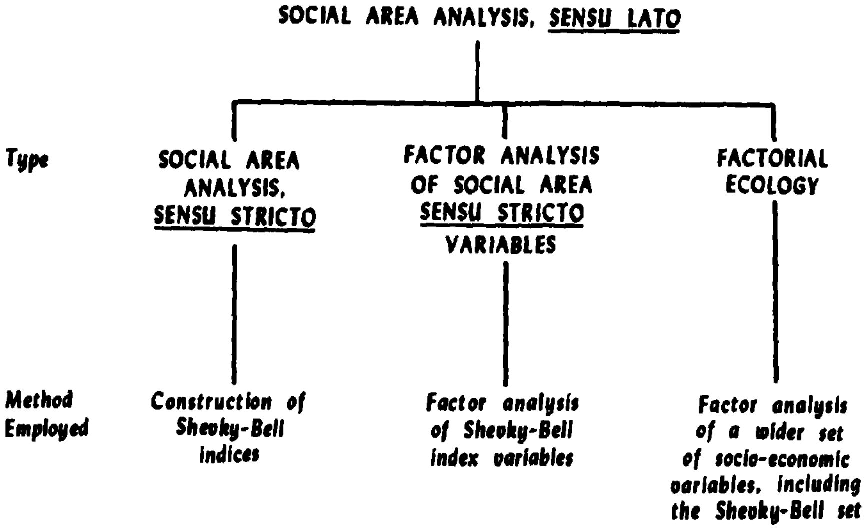


FIG. 12.—Typology of social area analysis

[24] Wendell Bell, "Economic, Family and Ethnic Status: An Empirical Test," *American Sociological Review*, XXII (1955), 45–52.

[25] Maurice Van Arsdol, Santo F. Camilleri, and Calvin F. Schmid, "The Generality of Urban Social Area Indices," *American Sociological Review*, XXIII (1958), 277–84.

[26] Adoption of this research path presupposes the availability of a large, high-speed computer and an operational program for performing factor analysis if large data matrixes are to be factor analyzed. Absence of such resources has (presumably) meant continuing use of the Shevky-Bell indexes.

[27] For an outline of factor analysis for the nonmathematician, see Rudolph Rummel, "Understanding Factor Analysis," *Journal of Conflict Resolution*, XI (1967), 440–80; for the more mathematically inclined, see Harry Harman, *Modern*

The dimensions isolated are an objective outcome of the analysis. *Interpretation* of the dimensions (factors) depends on the nature of the variables used in the analysis and the body of concept or theory that is brought to bear. Theory provides the investigator with a set of expectations regarding the factor structure which can be compared to the actual set of factors produced. This comparison was made formally by Van Arsdol, Camilleri, and Schmid but somewhat less formally by later writers. What is impressive, however, is that studies of American cities have, by and large, succeeded in isolating the three social area dimensions originally proposed by Shevky: socioeconomic status, family status, and ethnic status.

Moreover, it becomes increasingly evident that each of these dimensions captures the essential features of one of the classic spatial models (socioeconomic status—Hoyt, family status—Burgess, ethnic status—"segregation" studies, Firey), so that in Western metropolises born in the industrial age and populated by a variety of races or national groups, "the models are independent, additive contributors to the total socio-economic structuring of city neighborhoods."[28]

Those indexes that measure the socioeconomic status of individuals or groups vary principally by sector; those variables which measure the family and age characteristics of the population vary principally by concentric zone; those measures that isolate a minority group within the population show a tendency for that group to cluster in a particular part of the metropolis. Such is the conclusion of Anderson and Egeland[29] in their analysis of the spatial variance of a number of socioeconomic measures within four medium-sized American cities (Akron, Dayton, Indianapolis, and Syracuse). Those variables that were thought to measure social status were combined into a "social rank" index, and those variables that were considered indicative of family status and degree of urban acculturation were combined into an index of "urbanization" in the manner originally developed by the social area analysts. An analysis of the variance of these two indexes by sector and by concentric zone revealed that social rank varied principally by sector and urbanization by concentric ring. A third dimension corresponding to their "segregation index" is also present and describes the concentration of particular minority groups in limited neighborhoods of the city.

This basic triad of spatially arranged social dimensions can be superimposed to form, at the intersections of sectors, zones, and segregated areas, *communities* of similar social, family, and ethnic status. Figure 13 provides an idealized picture into which distortions can be successively introduced to closer approximate reality.

However, there is a further complication. The zones within the segregated area occupied by the minority group do not correspond to the general life cycle zones of the metropolis; the segregated area is a microcosm of the whole, compressed spatially, reproducing in miniature the metropolitan-wide pattern (Fig. 13, *E*). This modified pattern is then further distorted by city growth (Fig. 13, *F*). "Tear faults" develop as zones cross sectoral boundaries with displacement of zones outward in the early-growth sectors. Finally, introduction of secondary work-place nodes—a heavy industrial area in the southern part of the city, industrial satellites in a crescent—further changes the form of the model for the metropolitan region.

Behavioral basis: the residential location decision.—The attraction of the classical ecological view of the industrial city is that it postulates a process of group competition and mobility that produces the spatial structure of the city's population. In Hoyt's

Factor Analysis (Chicago: University of Chicago Press, 1960).

[28] Brian J. L. Berry, "Internal Structure of the City," *Law and Contemporary Problems* (Winter, 1965), p. 115.

[29] Theodore R. Anderson and Janice A. Egeland, "Spatial Aspects of Social Area Analysis," *American Sociological Review*, XXVI (1961), 392–98.

model, on the other hand, the operations of the real estate market are not spelled out, though he does recognize the importance of the decisions of prestigious individuals in the location of the high-grade rental sector.

An alternative view of the processes producing the structure may be proposed in the case of the North American city, however. This view involves the behavior of individuals and institutions. The inhabitants of the city are faced with a fundamental decision: where to live. The principal determinants of such a housing choice are three in number—the price of the dwelling unit (either in rental or in purchase value terms); its type; and its location, both within a neighborhood environment and relative to place of work. These determinants have parallels in the attributes of the individual making the housing choice: the amount he is prepared to pay for housing, which depends on his income; his housing needs, which depend on his marital status and family size, that is, his stage-in-life cycle; his life-style preferences, which will affect the type of neighbor he will want; and, finally, the location of his job.[30] When the values of the two sets of characteristics

[30] Preferences of people as to visual appearance of the home and its surrounding neighborhood have been investigated by George L. Peterson, "Subjective Measures of Housing Quality: An Investigation of Problems of Codification of Subjective Value for Urban Analysis" (unpublished Ph.D. dissertation, Northwestern University, June, 1965); "A Model of Preference: Quantitative Analysis of the Perception of the Visual Appearance of Residential Neighborhoods," *Journal of Regional Science*, VII (1967), 19–31; and "Measuring Visual Preferences of Residential Neighborhoods," *Ekistics*, XXIII (1967), 169–73.

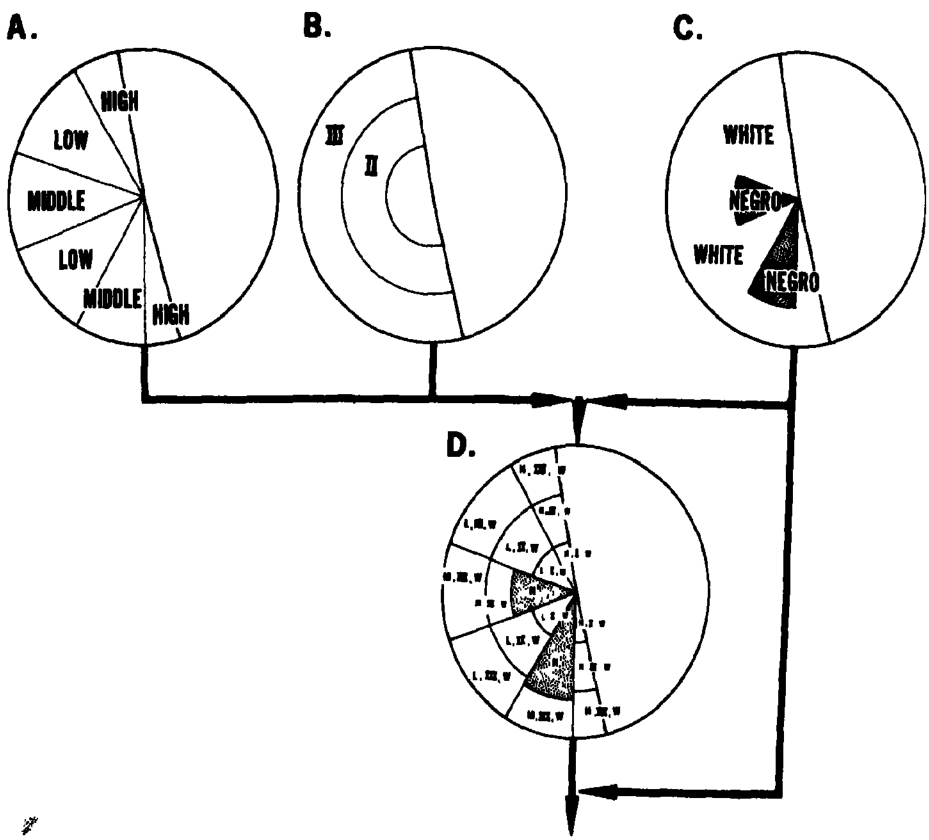


FIG. 13.—Integrated spatial model of the metropolis

match, a decision to purchase housing will be made (see Table 1).

The most important determinant is undoubtedly income. A large family with many children may need a large single-family home, but unless family income is above that minimum threshold required by the mortgage company, purchase of this type of home will not be possible. Similarly, low wages may make it essential for a worker to live near his place of employment in order to minimize the costs of the journey to work. He may therefore be unable to satisfy his preferences as to type of neighborhood.

It may be objected that other factors such as the nature of the school system in the community or the location of the prospective home relative to a golf course play an important role in the decision, but these influences become significant only if the

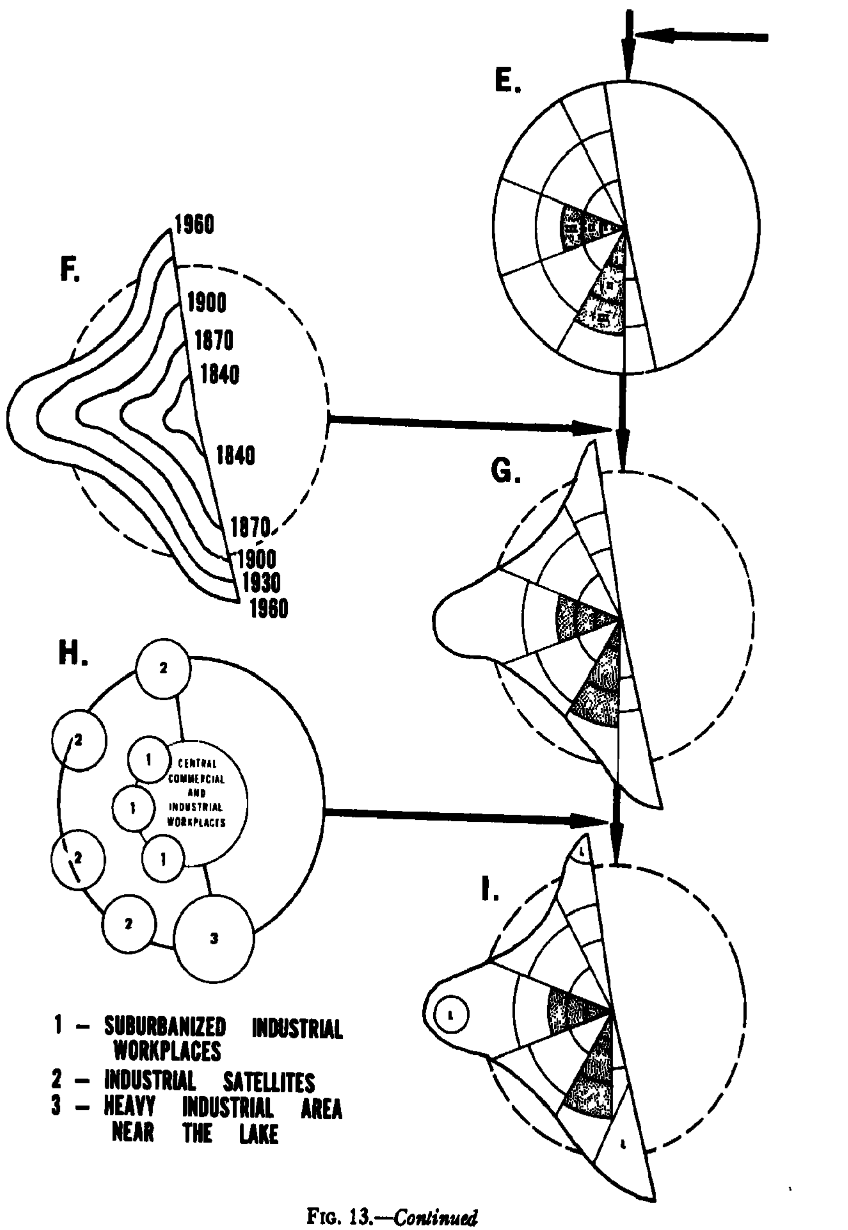


FIG. 13.—*Continued*

individual's income is large enough for him to be able to ignore the more basic determinants. Gans, in his participant-observer study of the inhabitants of a new suburban town in New Jersey,[31] asked a sample of Levittowners what their principal reason was for moving to Levittown. Over 80 per cent gave house-related reasons for moving.

Gans's study of Levittown, New Jersey, contains many valuable illustrations of the way in which some of the residential choice determinants outlined above operate. In response to a perceived demand for middle-income homes, Levitt built houses with prices ranging from $11,500 to $14,500.[32] To insure that he got paid for the houses he built, Levitt told his salesmen to apply an informal lower income limit of $5,500 a year for the would-be house purchaser. Anyone with less income than that was not to be sold a house. In fact, a small percentage of house buyers did have lower incomes, but the majority had higher. The house price acted as a selection mechanism: the middle-range prices attracted predominantly lower-middle-class families with small minorities of working-class and upper-middle-class families.

TABLE 1

Individual Characteristics		Housing Characteristics
Income	↔	Price
Stage-in-life cycle	↔	Type of home
Life-style preferences	↔	Neighbors, type of community institutions
Attitude toward journey to work	↔	Location with respect to the job

The houses were free-standing single family residences with three or four bedrooms, and, not unnaturally, young families with (or intending to have) children became their occupants. They were spacious and comfortable homes for a four-or five-member household, and the surrounding yard provided play space for the young children over whom the mother could keep an eye from the kitchen window. Since Levittown was an entirely new settlement, there were no neighbors in the community when the first inhabitants arrived, but the builder was careful to project a prestigious image in nearby Philadelphia (in order to counteract the unfavorable impression created by his other venture in the urban region which had experienced some trouble over racial integration and subsequent status decline), and, for the first two years, until legal action forced Levitt to integrate, discrimination was practiced against prospective Negro buyers.

The residential location decision was thus clearly a product of the interaction of the home purchaser and the home builder, a not unexpected conclusion! Their choices either effect (in the case of a new community) or affect (in the case of an established community) the urban ecology, creating relatively homogeneous areas containing similar sorts of people (areas called "natural" in the earlier ecological literature and generally "social" today). The character of established social areas in turn influences subsequent housing choices of later neighborhood occupants, because people seek compatible neighbors who share the same views on life (particularly on such important questions as the way in which children should be brought up) and find this compatibility by selecting their residence in the appropriate social area.

The choice of house and community type is only one part of the complete residential location decision, however. The housing consumer is also faced with the problem of *locating* his residence. This involves attitudes toward the journey to work, for the time and cost of commuting have to be traded off against the relative benefits of living in alternative communities that meet, within budgetary constraints, family needs. The lower the family or individual income, the more constrained will be the choice. Thus, lower-status people live closer to their work than people of higher status.

Therefore, to summarize more generally

[31] Herbert Gans, *The Levittowners* (New York: Pantheon Books, 1967), Table 2, p. 35.

[32] *Ibid.*

with reference to Figure 14: the individual or family occupies a position, s_i, in *social space*,[33] determined by economic status and family status. The household matches this position with that of a dwelling located in an analogous position, h_i, in *housing space*, and of housing in a similar location, c_i, in an equivalent *community space*, whose axes comprise socioeconomic status on the ordinate and familial characteristics on the abscissa. From a range of possible communities found in the same zone of community space, one dwelling in one community is selected on the basis of proximity to job location (if this is a constraint) or on the basis of other important neighborhood characteristics, thus fixing the choices in real geographic or *physical space*.

An orderly social geography results through like individuals making like choices, through regularities in the operation of the land and housing markets, and through the collaboration of similar individuals in excluding those of dissimilar characteristics from their neighborhood or in restricting certain minority groups to particular areas. The autonomous suburb is the prime example of the process of exclusion and the ghetto the most glaring illustration of the process of restriction. Sectoral patterning of such attributes of the neighborhood residents as education, occupation, income, and of neighborhood structural characteristics such as rent or value and quality of housing is a product of the differing abilities of various income groups to bear the costs of the journey to work. Lower-income workers, because of their restricted money budgets, must live close to their work (concentrated in the inner city around the central business district and along the rail and water routes radiating outward from it). The higher incomes of upper-status workers give them the freedom to locate their homes in areas of higher residential amenity—away from their places of work, away from the smoke and dirt of industry, and close to amenity features such as lakeshore and open space. The age structure of the population, average family size, and female labor force participation change as distance from the city center increases: young families locate further from the center than do older families. This pattern is a response to the change in house age and type as distance from the center becomes greater—the houses are newer, and single-family homes predominate as the city center is left behind. It is the lower land values toward the urban periphery that make possible this land-voracious construction, and the increasing real income of home buyers makes possible their purchase. Finally, minority groups find themselves segregated from the rest of the population to a greater or lesser degree as a result of recent arrival in the city, discrimination in the housing market, or through choice of home in congenial communities.

COMPARATIVE EVIDENCE: SEVERAL POSSIBLE FACTOR STRUCTURES

From these bases of residential choice, the ecological pattern of American cities is dimensioned cumulatively by socioeconomic status, family status, and the constraints of race and ethnicity. What of non-American cities? Do they exhibit the same patterns of variation in census tract populations?

Pedersen's study of Copenhagen provides one of the most comprehensive urban ecological analyses to date.[34] From a matrix of fourteen socioeconomic variables (related to age distribution, employment status, distribution by industry, household size, sex ratio, and female employment of the population) and seventy-six zones of the city, three basic factors emerged in both 1950 and 1960, namely: (1) an urbanization or family status factor, (2) a socioeconomic status factor; and (3) a population growth and mobility factor. The first factor, when mapped and graphed, displayed the classic

[33] See the similar use of the concept by Paul H. Chombart de Lauwe, *Paris et l'agglomeration parisienne* (Paris: Presses Universitaires de France, 1952).

[34] Poul O. Pedersen, *Modeller for Befolkningsstruktur og Befolkningsudvikling i Storbymorader Specielt med Henblik pa Storkobenhavn* (Copenhagen: State Urban Planning Institute, 1967).

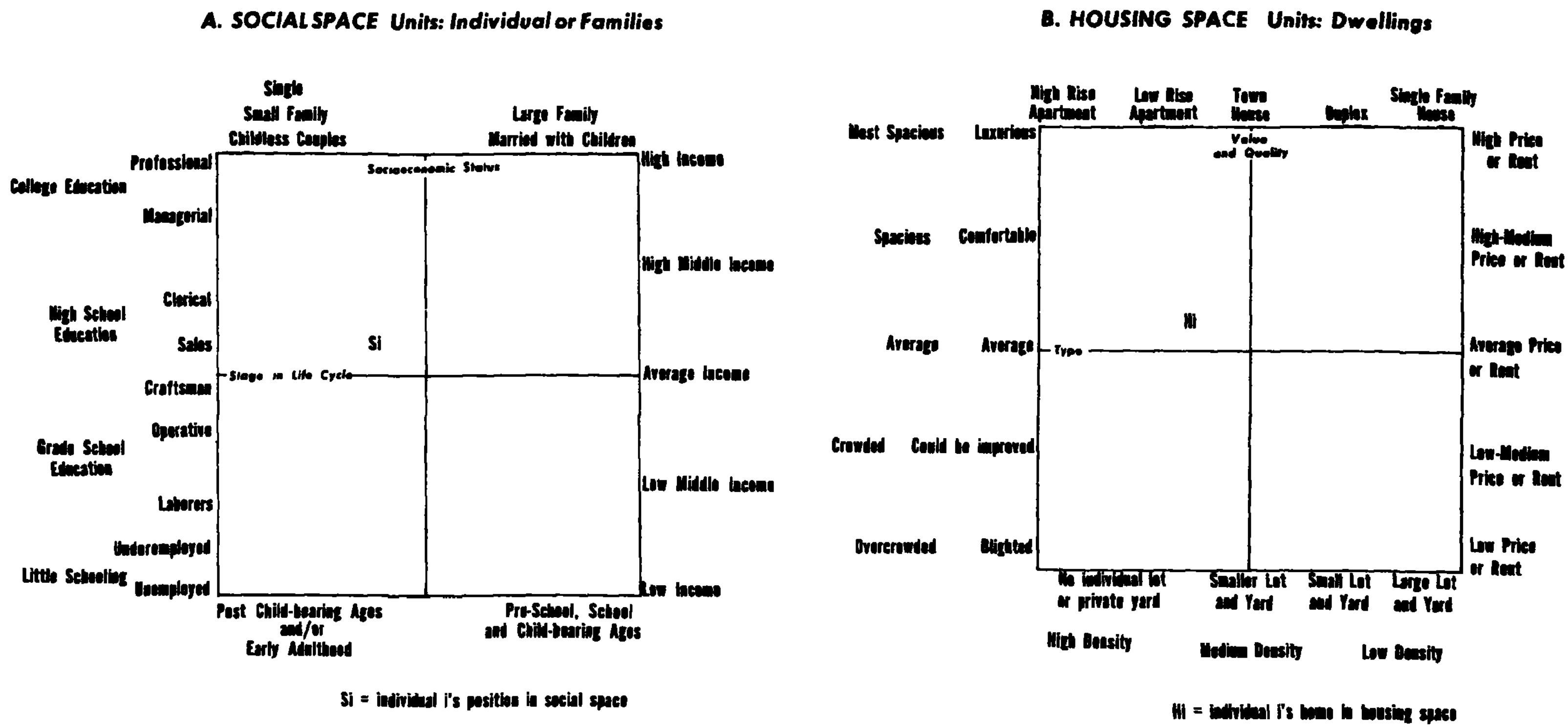
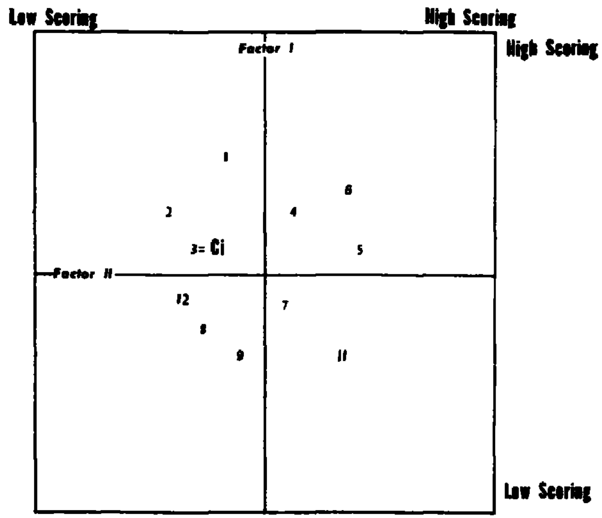


FIG. 14.—Residential location decision process

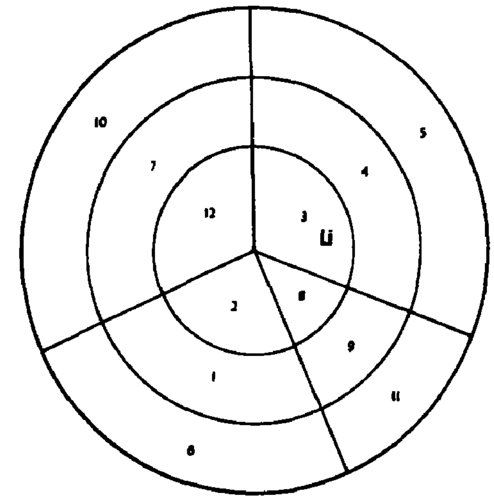
C. COMMUNITY SPACE *Units: Tracts or Larger Sub-areas*



Ci = the community in which i's home is located

D. LOCATIONAL OR PHYSICAL SPACE

Units: Tracts or Larger Sub-areas



Li = the zone in the community in which i's home is located

FIG. 14.—*Continued*

TABLE 2

Factor Conditions	Types of Variables Used	Necessary Conditions
Socioeconomic status factor.	Education, occupation, income	(1) "That the effective ranking system in a city be related to the operational definition of social status"; (2) "that the ranking system in a city be manifested in residential segregation of persons of different rank at a scale capable of being identified by the areal units of observation used in the analysis"
Family status factor...	Family size, portions of the age pyramid, fertility	(1) "That family types vary, either due to 'natural' causes such as those associated with sequential stages in the family cycle, or to 'social' causes such as those associated with other divisions in society, whether ethnic, socioeconomic or other"; (2) "that subareas within the city are differentiated in their attractiveness to families of differing types" at a scale capable of being identified by the areal units of observation used in the analysis
Disassociation between socioeconomic status and family status dimensions . .		Either (1) that there exists little or no association between social class and family type; or (2) if there is some association between social class and family type, that (*a*) there is a clear distinction between stages in the family cycle, each stage being associated with a change of residence; and that (*b*) "subareas within the city offer, at all economic levels, highly specialized housing accomodations especially suitable to families at particular points in their natural cycle of growth and decline" at a scale capable of being identified by the areal units of observation used in the analysis, and (*c*) "cultural values permitting and favoring mobility to maximize housing efficiency, unencumbered by the 'unnatural' frictions of sentiment, local attachments, or restrictive regulations"
Separate minority group .	Proportion of the subarea population in the minority group, measures of the relative concentration of minority groups	(1) That the characteristic(s) differentiating the minority group from the rest of the population be of perceived significance in the social system, i.e., that the urban population be truly heterogeneous; (2) that the minority group be residentially segregated from the rest of the population, at the scale of observation used in the analysis
Disassociation between minority group and socioeconomic status dimensions.	Measures used above for minority group, measures of education, occupation, and income (and the distribution of resources)	(1) That the minority groups be residentially segregated from the rest of the population, at the scale of observation used in the analysis; and either (2) that there exists little or no association between minority group status and socioeconomic status; or (3) that there is some association between social class and minority group status, that (*a*) the minority group still spans most of the social status range, though it may be concentrated at the lower end of the range; (*b*) a fairly full range of housing accommodation quality be available for families within the residentially segregated area

TABLE 2—*Continued*

Factor Conditions	Types of Variables Used	Necessary Conditions
Disassociation between the minority group and stage-in-life-cycle dimensions...	Measures used above for the minority group, measures of age distribution, family size, and fertility	(1) That the minority group be residentially segregated from the rest of the population at the scale of observation used in the analysis; and either (2) that there exists little or no association between minority group status and family status; or (3) that if there is some association between family status and minority group status that (*a*) minority groups still span most of the family status range, though it may be concentrated at one end of the range; (*b*) a fairly full range of housing accomodation types be available for families within the residentially segregated area

Burgess pattern of concentric rings; the second factor exhibited sectoral distribution, with the exception that the central zones of the city were of uniformly low status; the third factor exhibited highest values in the zone of new suburban development and in the low-status areas of the inner city. Pedersen devotes a separate chapter to detailed consideration of each of the three principal factors. Age structures of the communities of the city are considered as functions of the population growth and migration process. Income differentials in the suburban portion of the metropolitan region are shown to have narrowed over time as the area has been converted into residences for city dwellers from farmland, whereas the gap between inner city and suburban incomes has widened. Absent from Pedersen's findings is a differentiation of Copenhagen's population on linguistic or racial lines—such differentiation does not exist in Denmark.

Confirmation of such homogeneity of population in Scandinavian cities is found in Sweetser's comparison of Helsinki and Boston.[35] In addition to the basic social area dimensions, the Boston study yielded a number of ethnic dimensions (Irish middle class; Italian blue collar). The Swedish-speaking population of Helsinki, however, failed to load on a separate factor but rather loaded on the socioeconomic status factor, since they were of generally higher status than the Finnish-speaking population.

Studies of non-Western cities[36] highlight the cultural context of the American and Scandinavian factor structures and have led recent investigators to try to isolate those basic conditions in the urban system, social and spatial, which are necessary in order to produce the observed factor structures. A principal conclusion of Janet Abu-Lughod's study of Cairo was that "no factorial separation between indicators of social rank and the indicators of family cycle stage could be obtained."[37] This contrasts with the normal separation of these two sets of indicators in factor analyses of American city data matrixes. As a result, Abu-Lughod was led to outline, in an extremely effective way, the conditions that were necessary and sufficient to produce the dimensions of socioeconomic status and family status that are found to have independent existence in almost all American cities, conditions that were not fulfilled in the case of Cairo, as shown in Table 2.

These conditions shown in Table 2 in

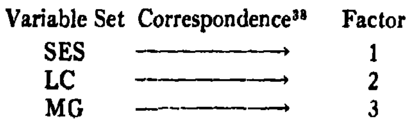
[35] Frank L. Sweetser, "Factor Structure as Ecological Structure in Helsinki and Boston," *Acta Sociologica*, XXVI (1965), pp. 205–25.

[36] Janet Abu-Lughod, "The Factorial Ecology of Cairo" (unpublished paper, Northwestern University, 1968).

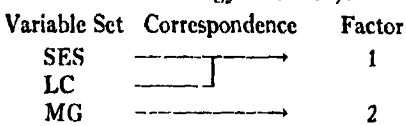
[37] *Ibid.*, p. 21.

fact give rise to several alternative factor structures representing permutations of the three basic sets of variables (SES—socioeconomic status set; LC—stage-in-life cycle of family status set; MG—minority group set [only one minority group is discussed here, although clearly the argument could be extended to several]):

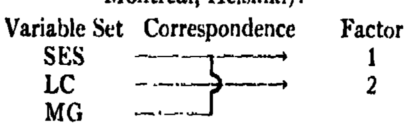
Combination 1 (Shevky-Bell):



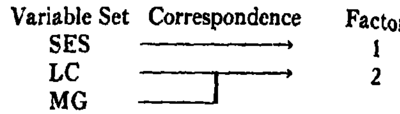
Combination 2 (Abu-Lughod, "The Factorial Ecology of Cairo"):



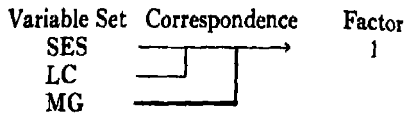
Combination 3 (American South, Montreal, Helsinki):[39]



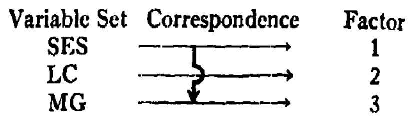
Combination 4 (Miami):[40]



Combination 5 (Unidimensionality—Erewhon?):



Combination 6 (Chicago):[41]



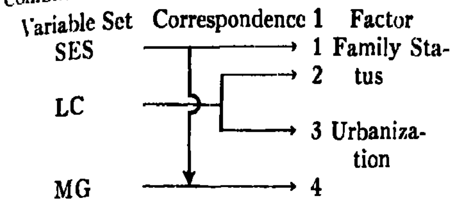
[38] Correspondence indicates highest loadings of variables on factors.

[39] Minority group status is a social status indicator. In one case, the minority group occupies the lower end of the socioeconomic ladder, with few members of high social status. The minority group variable(s) loads most highly on a socioeconomic status factor. Examples of such situations are found in the factor structures of southern U.S. cities. Van Arsdol, Camilleri, and Schmid (*op. cit.*) found a simple spatial segregation factor to be present in Atlanta, Birmingham, and Louisville in 1950, but more recent studies of the latter two cities and for Shreveport, Louisiana, employing a wider range of variables for 1960, found that the variable "percentage non-white" loaded most highly and negatively on a general socioeconomic status factor. See Yehoshua Cohen, "The Urban Ecology of Birmingham, Alabama" (unpublished paper, Center for Urban Studies, University of Chicago, March, 1968); Philip Peters, "The Urban Ecology of Louisville, Kentucky" (unpublished paper, Center for Urban Studies, University of Chicago, March, 1968); and Howard Spodek, "The Urban Ecology of Shreveport, Louisiana" (unpublished paper, Center for Urban Studies, University of Chicago, March, 1968). The American situation may be reversed, however, in certain societies. In Montreal, e.g., the English-speaking minority occupies the higher end of the socioeconomic scale, and variables identifying this group load highly on a socioeconomic status factor. See Geoffrey Cliffe-Phillips, John Mercer, and Yue Man Yeung, "The Spatial Structure of Urban Areas: A Case Study of the Montreal Metropolitan Area" (unpublished paper, Center for Urban Studies, University of Chicago, March, 1968). See also Sweetser's study of Helsinki (*op. cit.*), which shows an analogous upper-status position of the Swedes.

[40] Minority group status is an indicator of family status. The minority group occupies one end of the family status scale with larger families and a younger population than the majority group. The minority group variable loads most highly on a stage-in-life cycle factor. The factor structure of the central city of Miami (Bruce Caswell, "The Urban Ecology of Miami, Florida" [unpublished paper, Center for Urban Studies, University of Chicago, March, 1968]) displays this convergence of racial and family status, but only because of the special nature of the study area which excluded the younger, larger family population of suburban whites.

[41] The minority group is residentially segregated from the rest of the population but occupies a position on the socioeconomic scale midway between that which it holds in Combination 1 and Combination 3 situations. Members of the minority group are found in substantial numbers at every position but in greater proportion at the bottom than the top. The minority group variables and those concerned with the distribution of resources (particularly those highlighting the lower parts of distributions) load highly on the same factor, identifying a pattern of discrimination in resource allocation along minority group (usually racial) lines. That is, at the same level of socioeconomic status less resources are commanded by the minority group. This structure has

Combination 7 (Anderson and Bean, Toledo):[42]



These combinations constitute the frame within which a factorial ecology of Calcutta can be evaluated.

CALCUTTA'S ECOLOGY: DIMENSIONS OF VARIATION, 1961

Principal axis factor analysis with normal varimax rotation of all factors with eigenvalues exceeding unity was used to analyze an 80-ward × 37-variable data matrix for Calcutta. The variables came from the 1961 census and represent all the quantitative materials available at the time we performed the analysis. They were obtained as special hand tabulations (as of the time of writing, the census tables have yet to be published for Calcutta), and they relate to family structure, literacy, type of employment, housing characteristics, and land use.

The units of observation were the city's eighty municipal wards—administrative units which, as Bopegamage[43] points out, possess social, economic, and demographic homogeneity only by accident. This lack of homogeneity is regrettable, but perforce to be tolerated. Because, too, the wards vary in their total population size, this variable was left in the analysis as a "screen" for the possible influence of size differences. All other variables were converted to ratios (population density, average size of household) or to percentages (percentage of workers employed in manufacturing . . .).

The factor analysis routine utilized the University of Chicago's computation system and produced as output matrixes of descriptive statistics for each variable, simple correlations among variables, factor loadings for ten factors (correlations of variables with factors), eigenvalues indicating that the ten factors reproduced 79 per cent of the total variance or 95 per cent of the common variance, and orthonormal factor scores for each ward with respect to each factor.

Not all of these materials can be reproduced here; instead, only the principal results of the analysis will be reviewed. However, the findings do reveal that alongside the rich ethnic variability described by Bose, Calcutta is also characterized by a broadly concentric pattern of familism, an axial arrangement of areas according to degree of literacy, and both substantial and increasing geographic specialization of areas in business and residential land uses, gradually replacing the former mixture of businesses and residences that were separated, rather, into occupational quarters. This mixture of pre-industrial and industrial ecologies thus lends support to the idea that the city is in some transitional developmental stage.

Factor 1: a land use and familism gradient.—According to factor 1, the larger peripheral wards of the greatest population size (factor loading 0.762)[44] are also those with the greatest percentages of females

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been found in recent studies of Chicago (Rees, *op. cit.*).

[42] T. R. Anderson and L. L. Bean, "The Shevky-Bell Social Areas: Confirmation of Results and Reinterpretation," *Social Forces*, XL (1961), 119-24. The life-cycle set of variables split into two subsets which load on separate factors, which Anderson and Bean call "family status" and "urbanization." There is a problem of interpretation of the factor loadings, of the extent of the study area, and of the range of study area population characteristics, since they dealt with only the central city of Toledo. There may, however, be a disassociation between the distribution of residential structures and the distribution of family types because either (*a*) "subareas within the city are not differentiated in their attractiveness to families of different types" by the measures employed, or (*b*) the city is not differentiated by type of residential structure sufficiently for one type of structure to house one type of family unit (at a scale capable of being identified by the areal units used in the analysis).

[43] A. Bopegamage, "A Methodological Problem in Indian Urban Sociological Research," *Sociology and Social Research*, L (1966), 236-40.

[44] The factor loading measures the correlation of the variable with the factor.

over fifteen (loading 0.817) and married women (0.749), the greatest non-Bengali component (0.591), proportion of buildings solely dwellings (0.892), with a higher proportion of the houses mud-walled (0.545). Conversely, the smaller wards at the heart of the city have greater proportions of their buildings occupied by businesses and offices (−0.825) and by commercial establishments and restaurants (−0.518), greater porportions of the population employed (−0.796), a greater mixture of the "houseless" male population (−0.727), and a greater percentage of the population residing in institutions (−0.813).

A sixth-degree trend surface[45] fitted to the factor scores reproduced 78 per cent of the variance in the scores in terms of spatial regularity and shows a clear concentric pattern around the city center (Fig. 15). *There is thus a clear and strong land use and familism gradient in Calcutta from commercial core to residential periphery*, modified only by the interjection of peripheral nodes at the cardinal extremities of the city. The commercial core (Fig. 16) houses most of the "sidewalk sleepers" (Fig. 17), largely young males, whereas the dominantly residential areas of the outer zones of the city have more balanced sex ratios (Fig. 18) and concentrations of mud-walled dwellings (Fig. 19). Comparison of trend surfaces for some of the variables (as depicted in Figs. 16–19) with Figure 15 reveals how their commonalities are captured by this factor.[46]

Factors 5 and 6: traditional commercial communities and the peripheral ring.—Surrounding the city core, two traditional ethnic commercial communities-*cum*-functional areas were identified by the factor analysis. Principal loadings on factor 6 were: percentage workers employed in trade, −0.560; percentage buildings in commercial use, −0.422; and percentage population in scheduled castes, 0.541. In the factor score map, areas of former Bengali commercial caste concentration in the southern half of the old native quarter stand out (Fig. 20; compare with Fig. 5), in stark contrast to the traditional peripheral ring of the lower-status scheduled castes (Figure 21).

Variables loading on factor 5 were the following: percentage of buildings in mixed residential-commercial use, 0.825; percentage of buildings in business and offices, −0.298; percentage of population Hindu, −0.678; and percentage of population non-Bengali, 0.303. This cluster of variables indexes the relative concentration of the non-Bengali commercial classes in the wards immediately east of the Maidan (Fig. 22; compare with Fig. 7).

The zones identified by factors 5 and 6 are not simply traditional ethnic community areas, however. They occupy the principal zones of commercial growth between 1911 and 1961 (Fig. 23), so that if their ethnic bases are weakening, as Bose suggests, their functional role in the city is becoming more marked, as areas of specialized land use.

Factor 4: the substantial residential areas.—The variable configuration and factor scores isolated by the fourth dimension identify the most substantial residential areas of Calcutta. Factor loadings are as follows: percentage of children under five, 0.778; percentage of houses with more than one room, −0.784; percentage of houses with walls and roof, 0.556; percentage of houses with mud walls, 0.361; percentage of population literate, −0.494; percentage of population in scheduled castes, 0.357;

[45] The equation is

$$F = a_0 + a_1X + a_2Y + a_3X^2 + a_4Y^2 + a_5XY + a_6X^3 + a_7Y^3 + a_8X^2Y + a_9Y^2X + a_{10}X^4 + a_{11}Y^4 + a_{12}X^3Y + a_{13}Y^2X + a_{14}X^2Y^2 + \ldots aX^3Y^3 \ldots$$

where X and Y are latitude and longitude coordinates for (approximate) geometric centers of the wards.

[46] All trend maps shown are fitted sixth-degree surfaces. The use of polynomials in this fashion is made to identify (*a*) whether there is systematic spatial variation in the surface, and, if so, (*b*) what are the underlying configurations. This method of smoothing out random variations is becoming standard in spatial analysis.

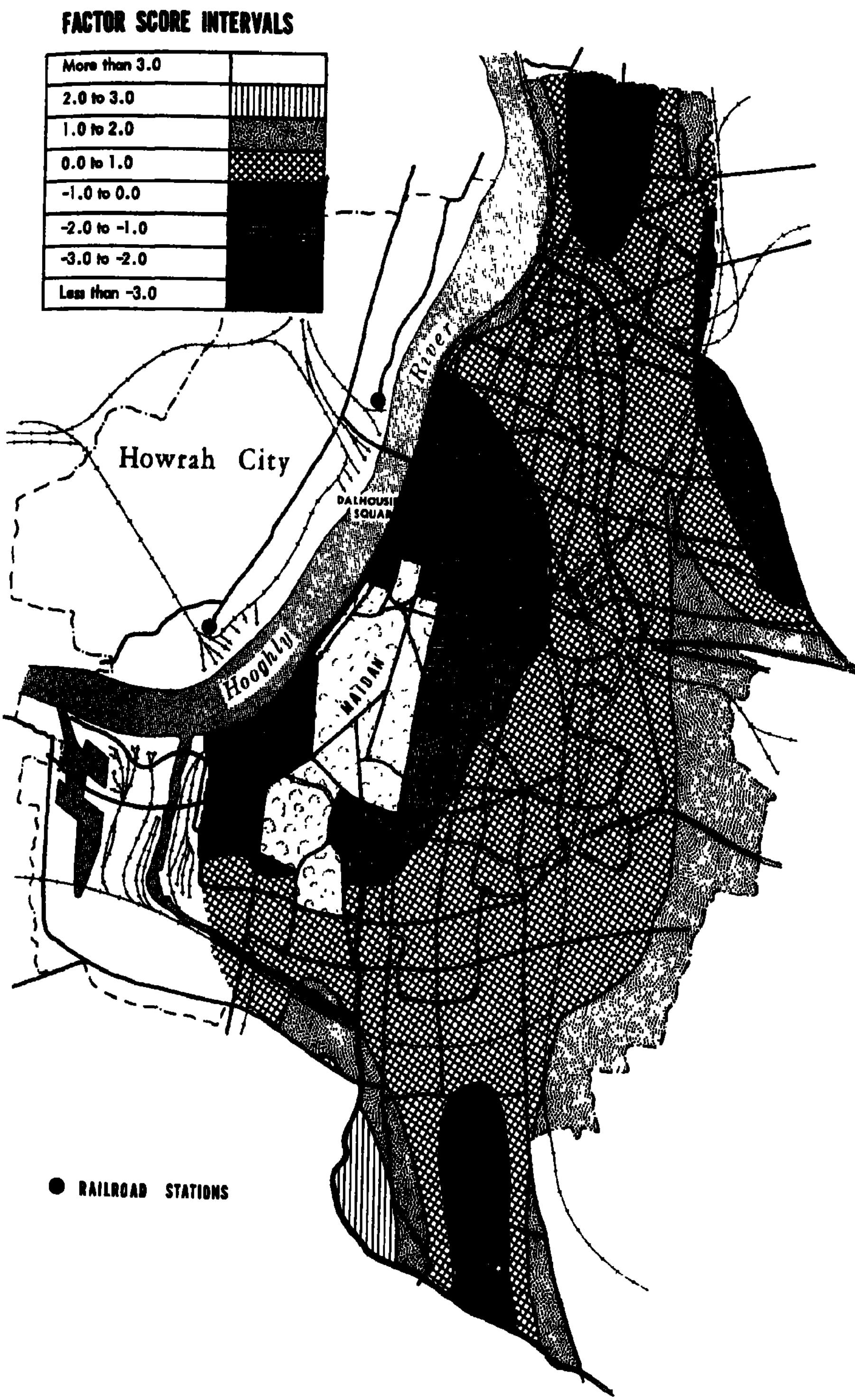


FIG. 15.—Factor 1: land use and familism

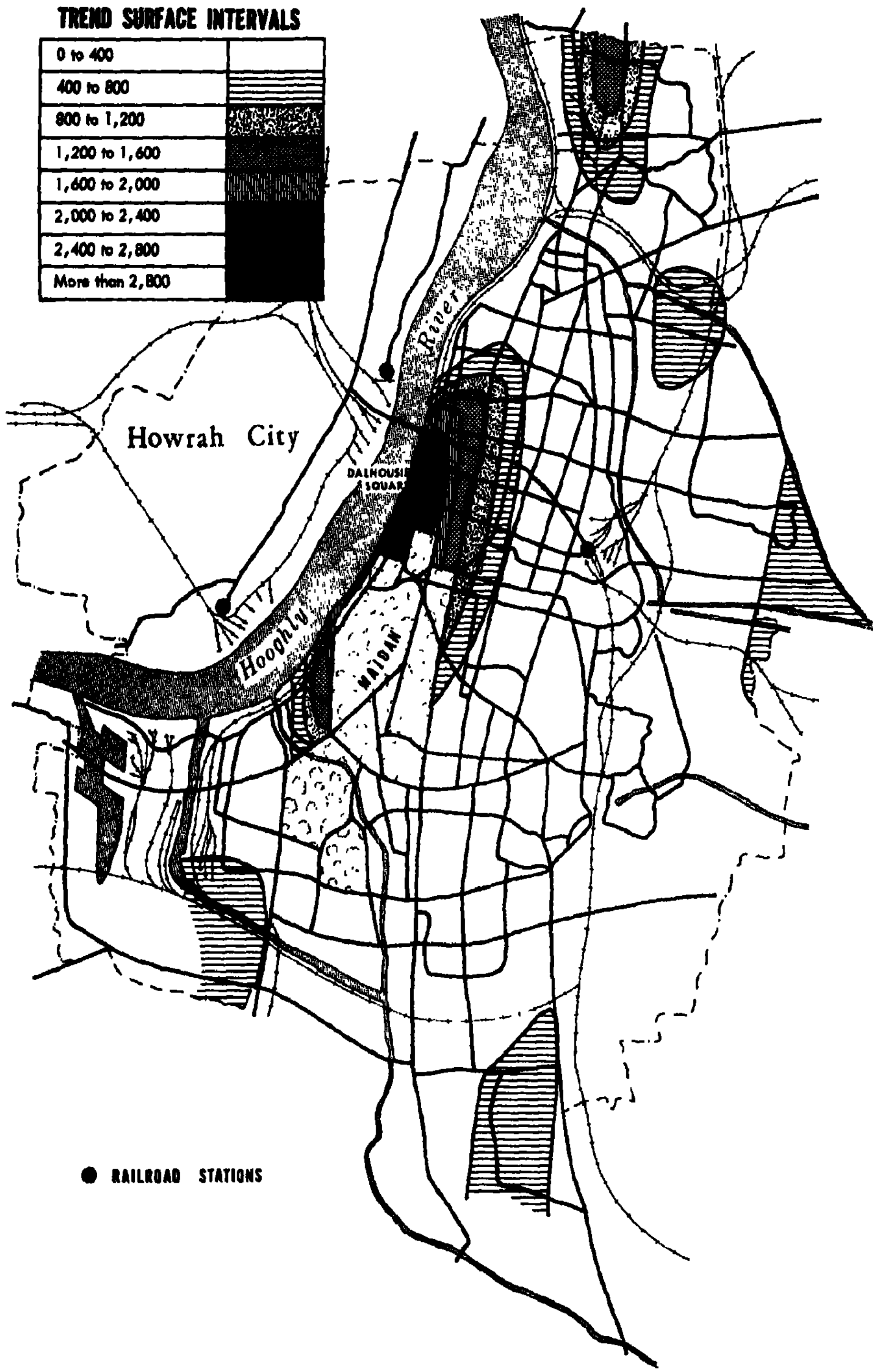


FIG. 16.—Trend surface of business and office land use

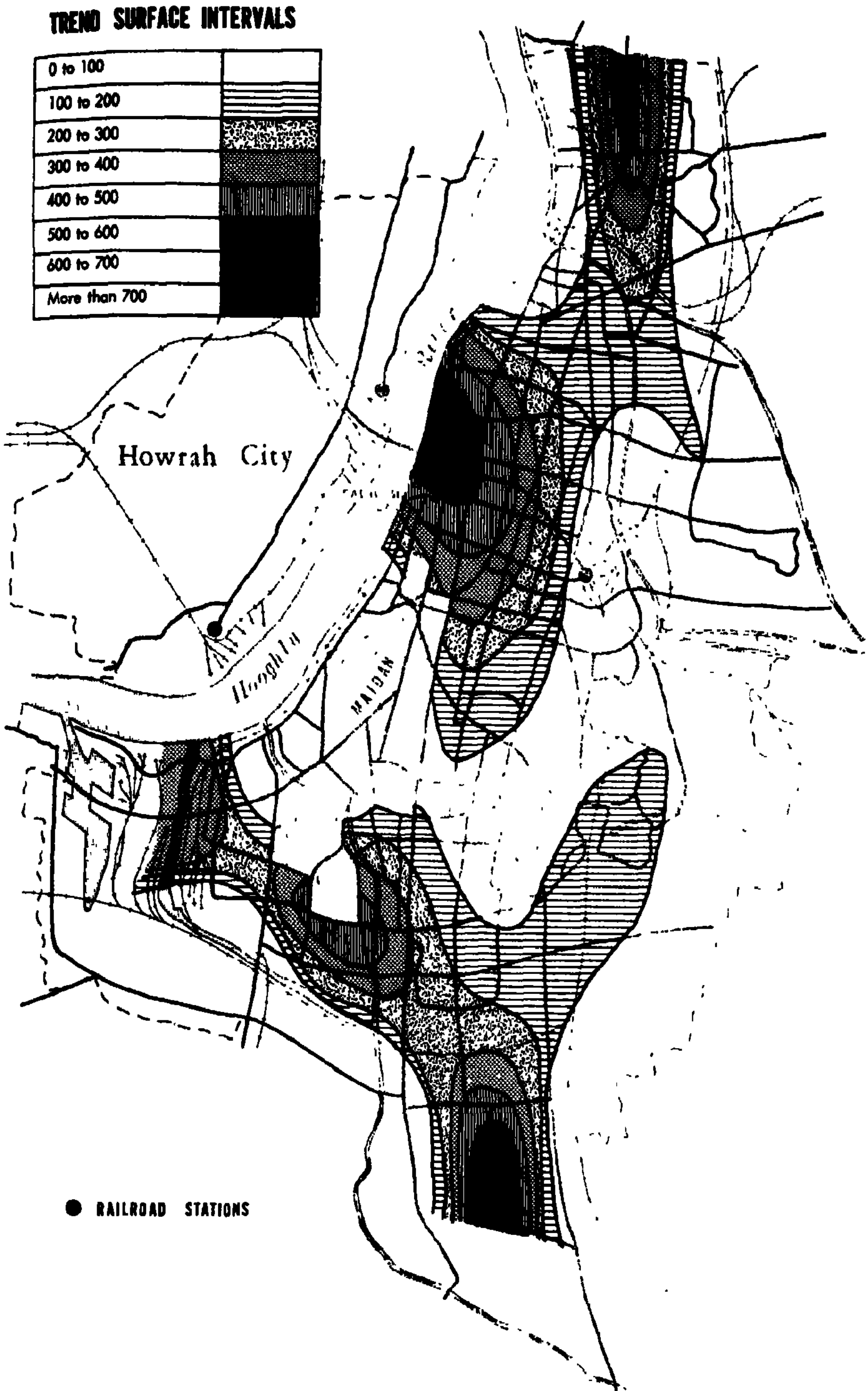


FIG. 17.—Houseless population

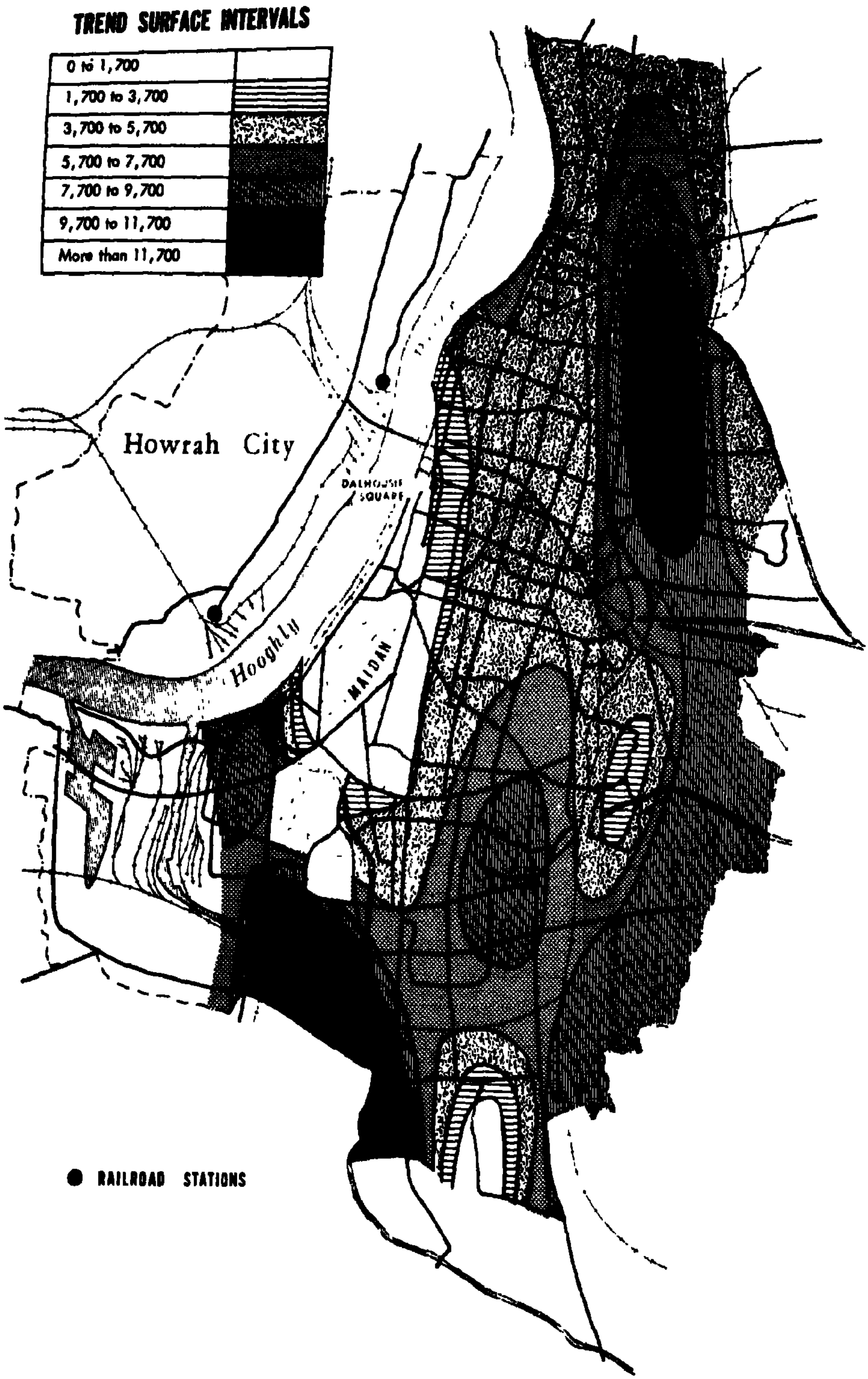


FIG. 18.—Distribution of married females

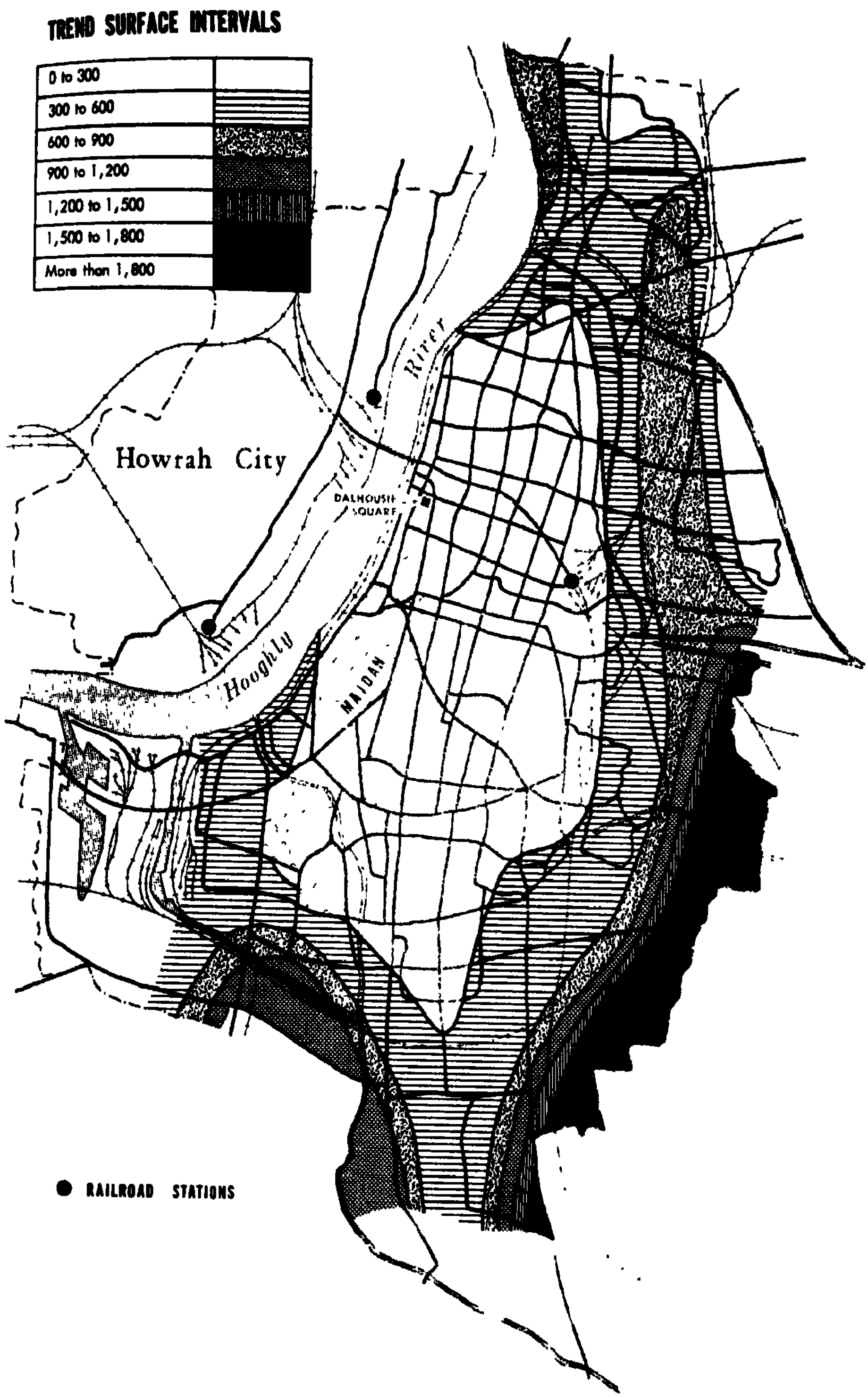


FIG. 19.—Houses with mud walls

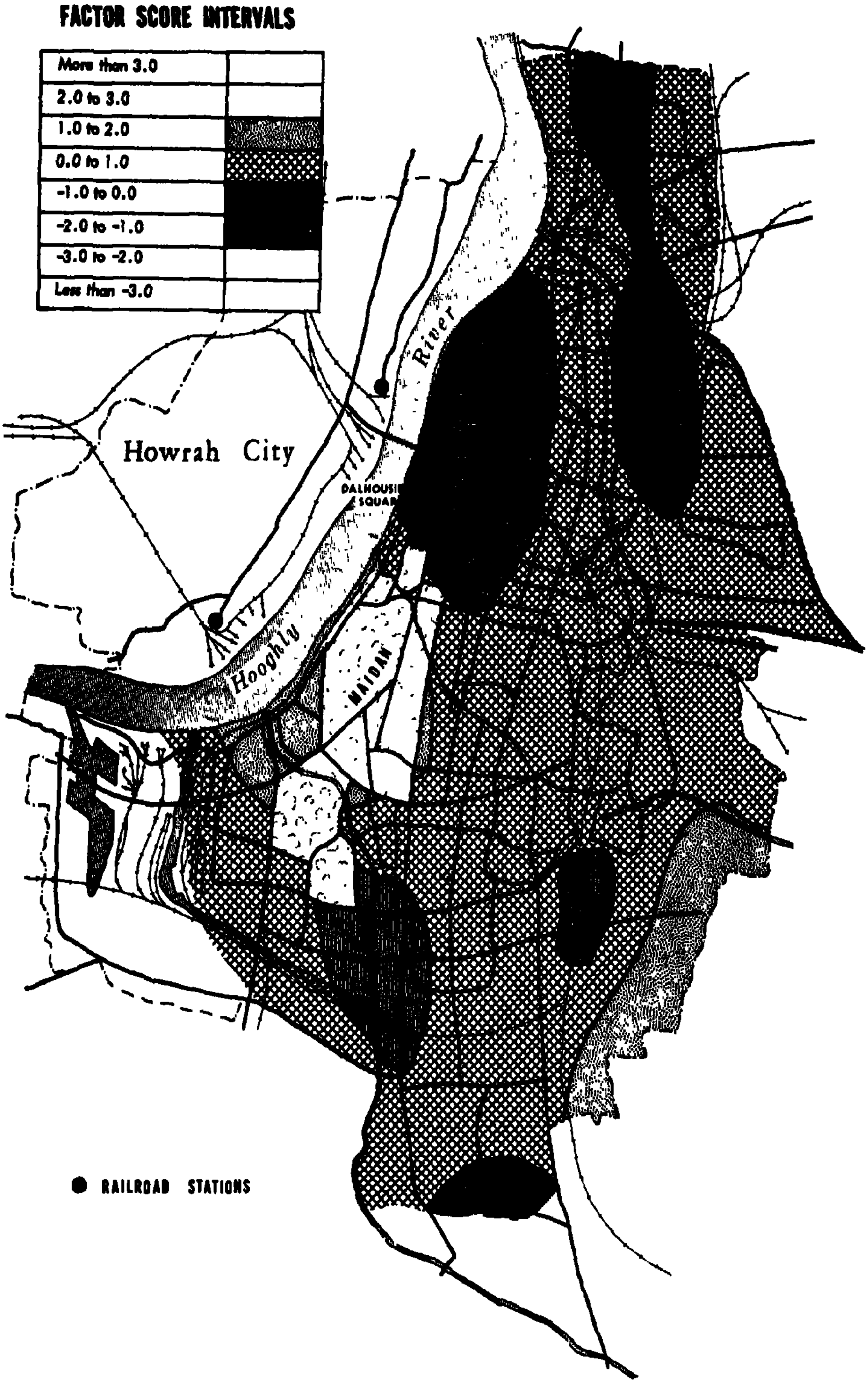


FIG. 20.—Factor 6: traditional Bengali commercial community

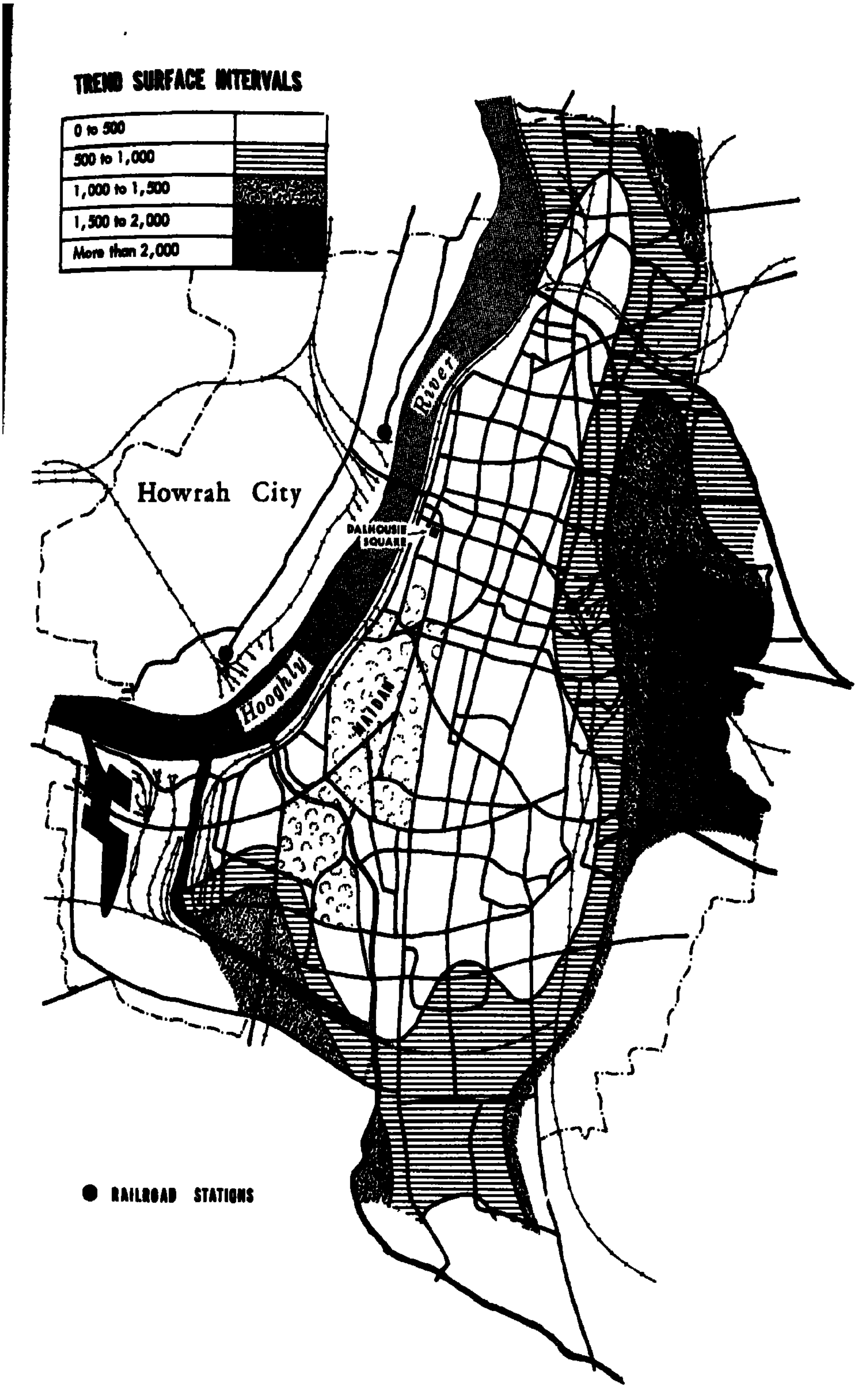


Fig. 21.—Females in scheduled castes

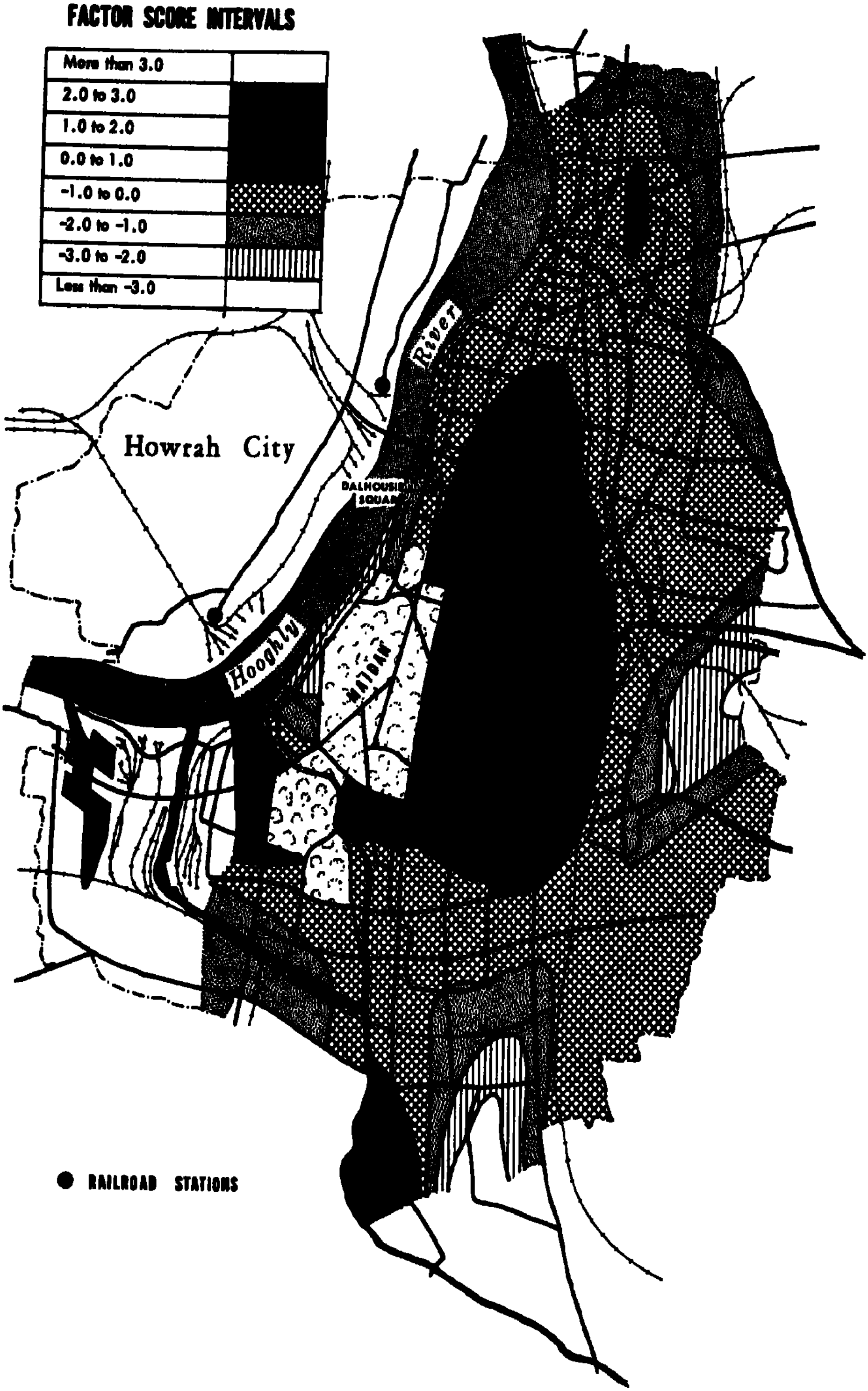


FIG. 22.—Non-Bengali commercial wards

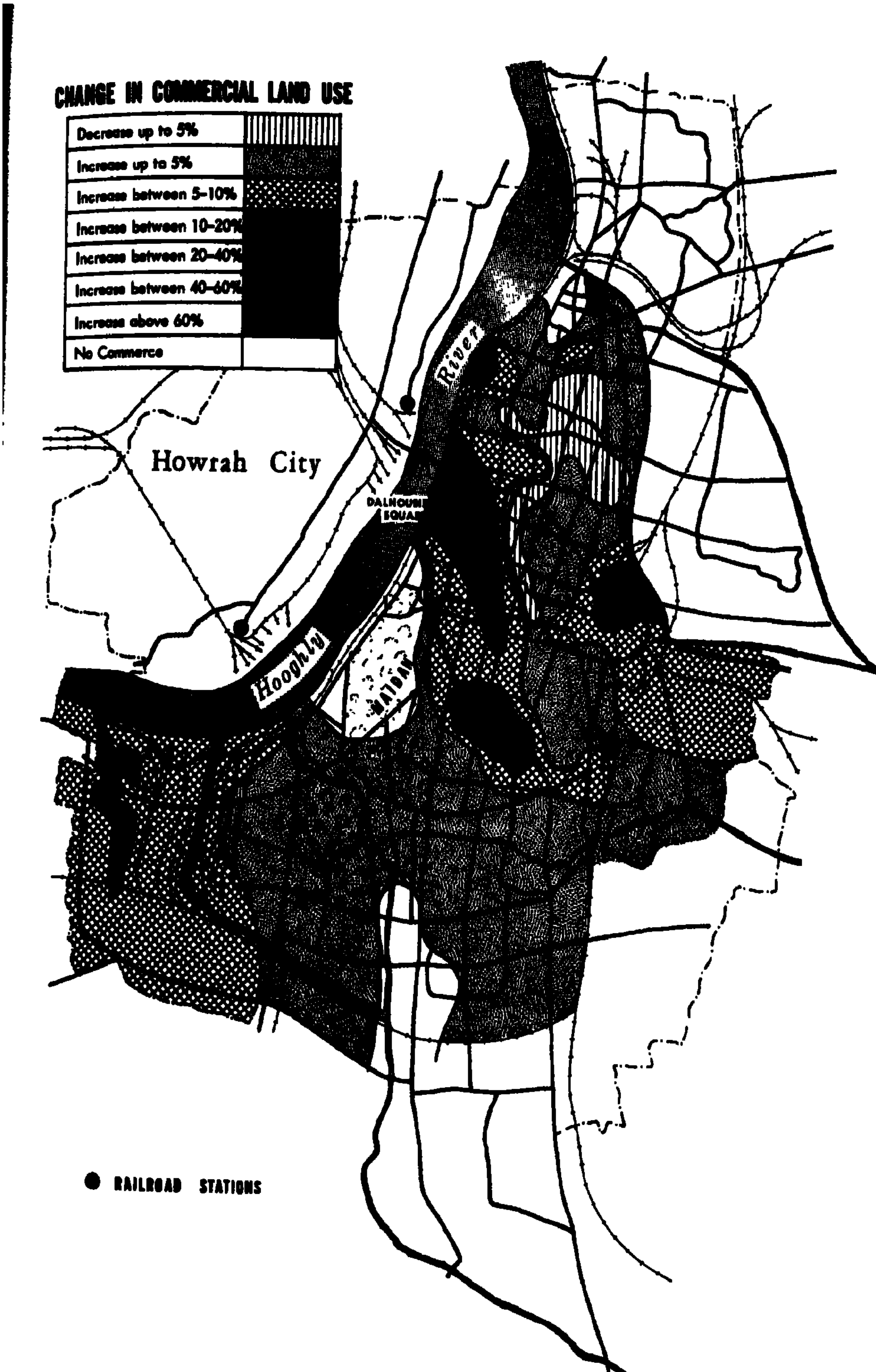


FIG. 23.—Changes in commercial functions, 1911–61 (after Bose)

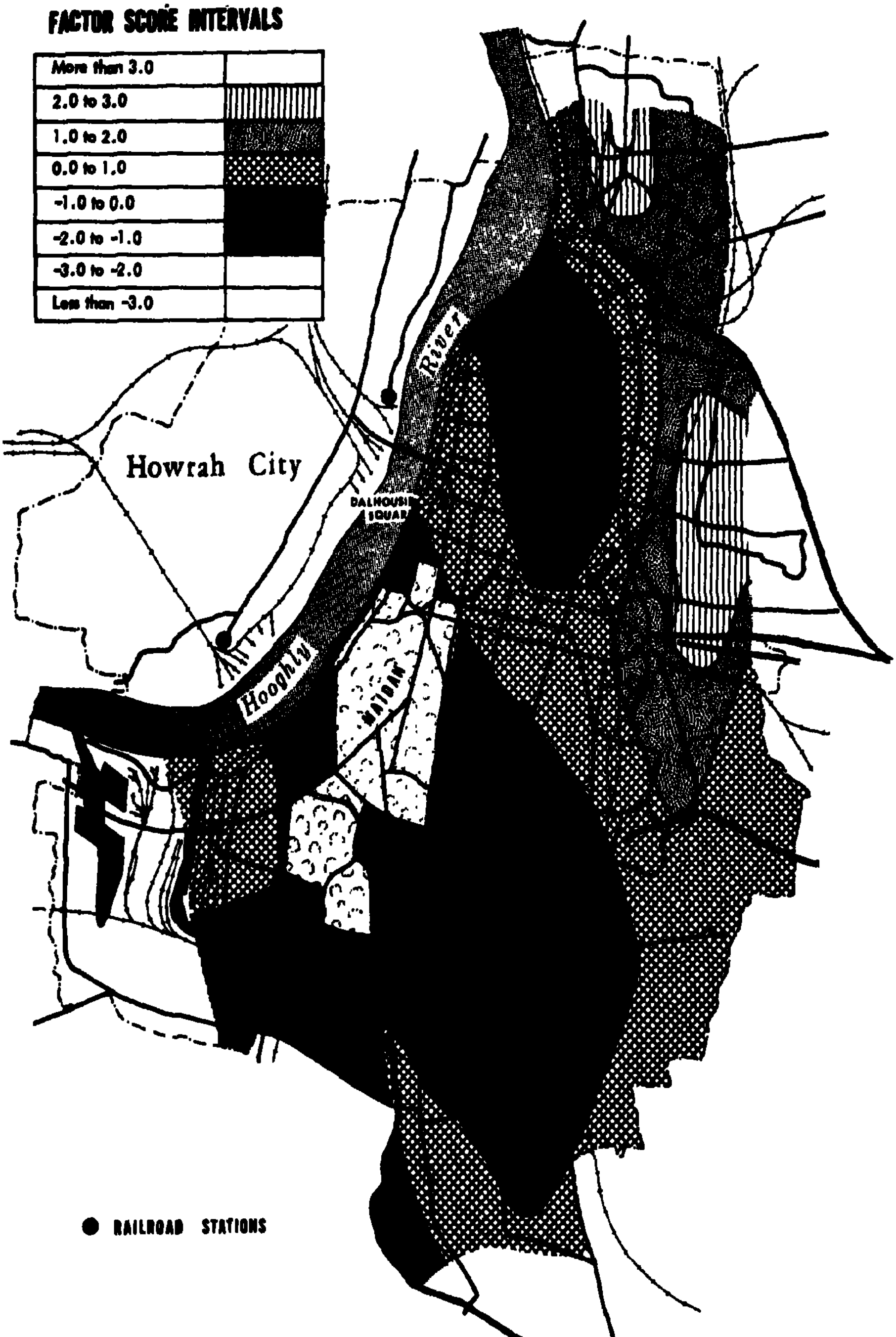


FIG. 24.—Factor 4: substantial residential areas

percentage of workers employed in manufacturing, 0.465; percentage of workers employed in services, −0.357; and percentage of buildings schools, −0.392. Scores are mapped in Figure 24.

Two zones are identified: the Bengali upper-caste northern half of the old native quarter, and the Europe residential zones east of the Maidan, with the latter rating somewhat higher than the former. Over the years, as their ethnic base has weakened, those areas have increased their residential function (Fig. 25).

Somewhat higher literacy rates than elsewhere and lower proportions of children under five (an inverse relationship noted in transitional communities elsewhere) are among the features that distinguish the residents of these two areas of larger, sounder homes from more peripheral residential wards in which there are greater proportions of mud-walled single-roomed bustees housing higher proportions of manufacturing workers (compare with Fig. 4). Thus, the factor apparently comes close to indexing status.

Factor 3: axiality in literacy.—However, a substantial variation in percentage of literate males (loading 0.808) and females (0.917) overlap the city's mixed and dominantly residential areas, especially in the areas of houses with more than one room (0.399) where the proportion of buildings that are places of education is greatest (0.566). The high-literacy wards stretch along a southward axis from Park Street through Ballygunge to Tollygunge and include sections of Alipore (Fig. 26). Characteristically, wards with the greatest concentrations of scheduled castes have the lowest literacy levels (−0.345); these are the wards with the greatest admixture of Oriya and Bihari in-migrants. This separation of factor 3 from factor 4 indicates that the links between status and literacy remain far from absolute in Calcutta.

Factor 2: higher-status Muslim concentrations.—Loadings on this factor are: percentage of workers employed in manufacturing, 0.696; percentage of workers employed in construction, 0.807; percentage of workers employed in trade, 0.722; percentage of workers employed in transportation, 0.776; percentage of workers employed in services, 0.851; percentage of females employed, 0.879; and percentage of houses with more than one household, −0.633. Interpretation of this configuration of variables is not particularly transparent, until the map of factor scores (Fig. 27) is studied simultaneously. Broad areas of the city have uniform levels of positive scores, but certain extreme-negative concentrations stand out, identifying communities having many houses containing more than one household, low percentages of females employed, etc. These communities (compare with Fig. 6) are the principal areas of upper- and middle-class Muslim concentration.

Factors 7 through 10: special land-use configurations.—The remaining four factors with eigenvalues exceeding unity produced in the analysis identified special features of Calcutta's land-use pattern:

a) Peripheral, essentially rural, areas with substantially higher sizes of households than the rest of the city (0.838).

b) Hotel-restaurant-entertainment districts (percentage of buildings commercial and restaurants [0.661]; percentage of places of entertainment [0.869]).

c) The medical center (percentage of buildings medical institutions [0.576]).

d) The factory zones (percentage of buildings factories [0.691]).

These round out the meaningful variations extractable from the original 80 × 37 data matrix, and they further indicate the substantial functional differentiation of land use achieved in Calcutta by 1961.

Calcutta's social areas.—One valuable outcome of the analysis was an 80 × 10 matrix of orthonormal factor scores. Relative similarity of each ward with every other could thus be measured as the distance separating the wards when plotted as points in the Euclidean space of the 10 dimen-

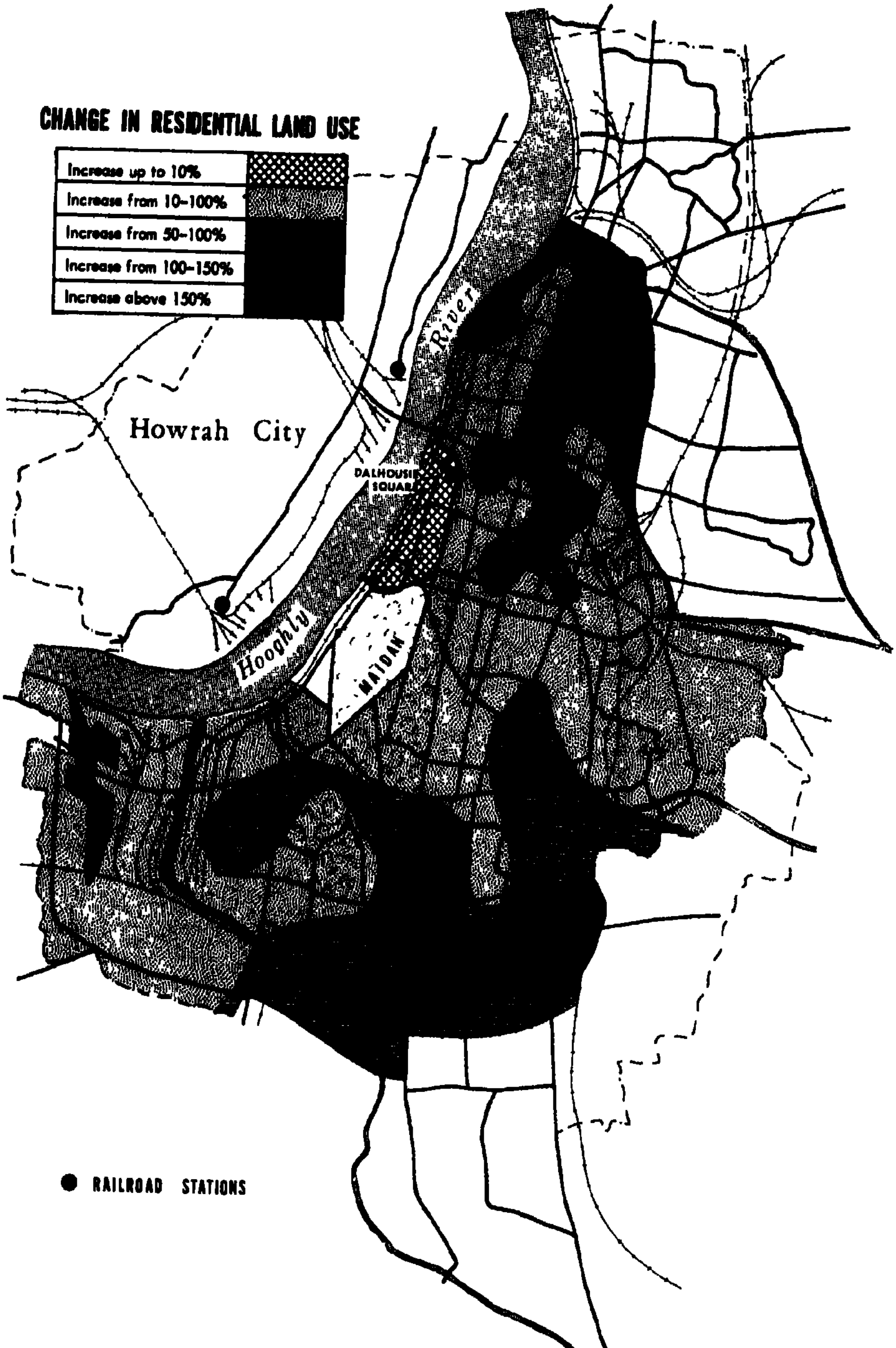


FIG. 25.—Changes in residential functions, 1911-61 (after Bose)

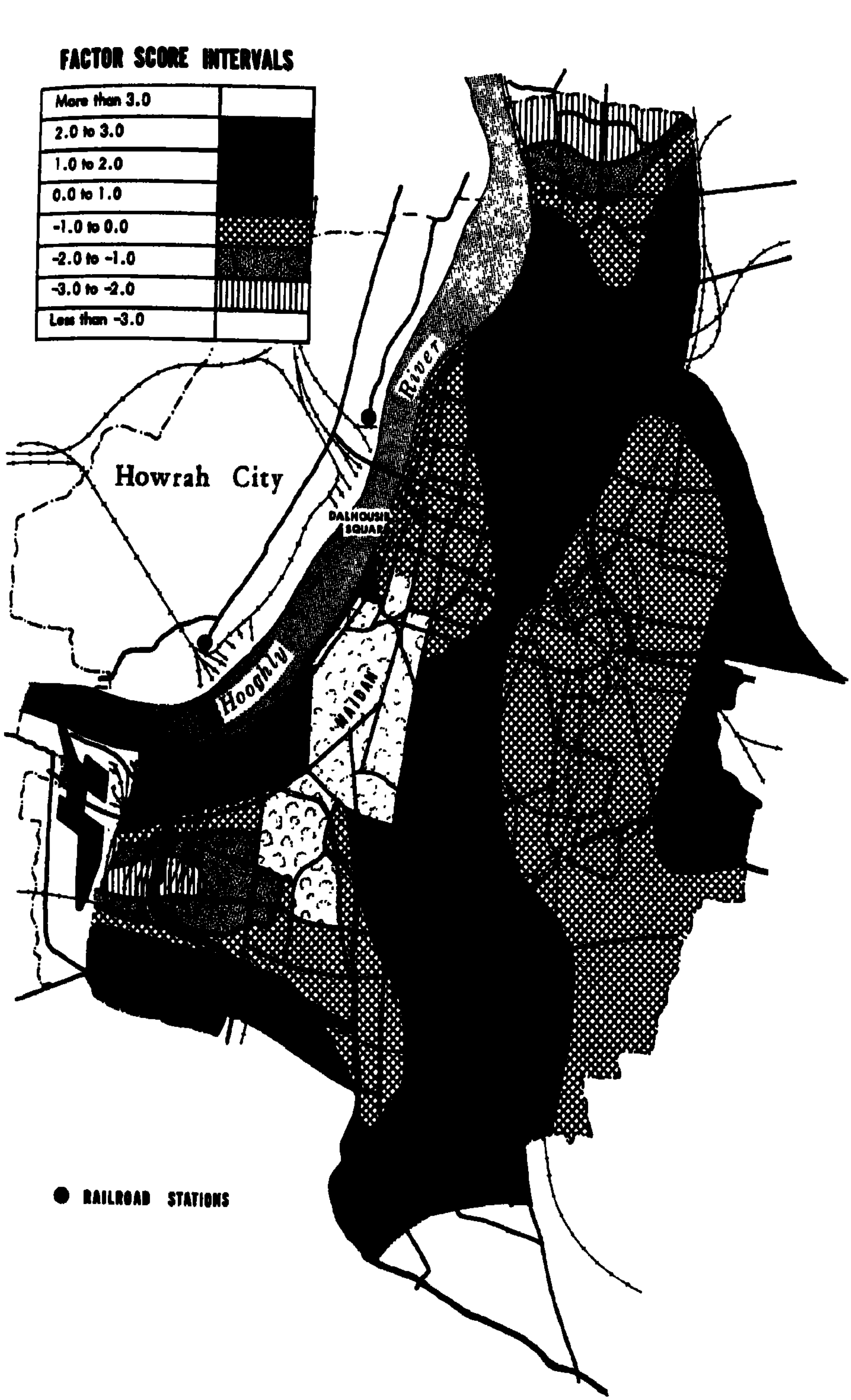


FIG. 26.—Factor 3: literacy in the population

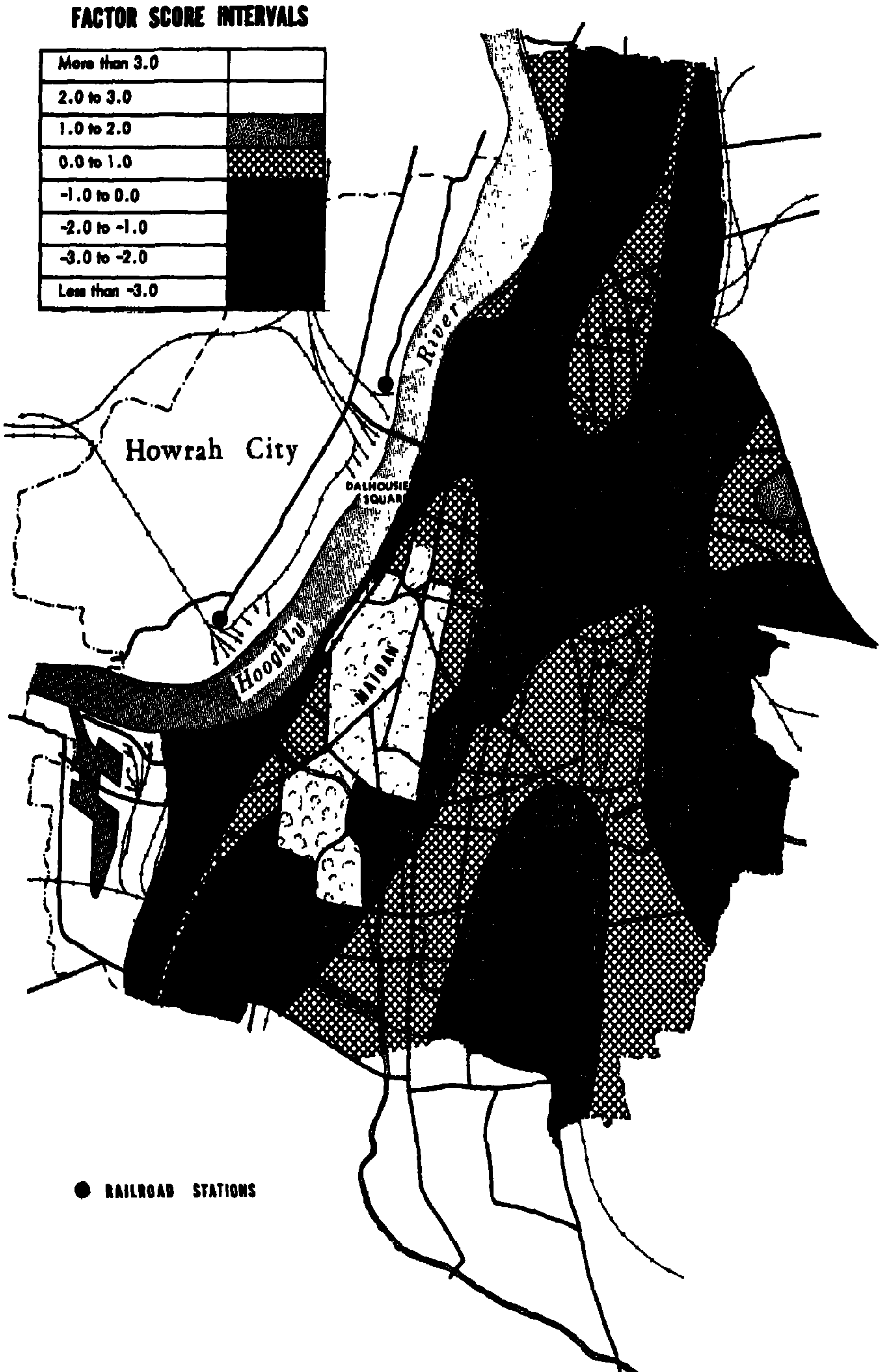


Fig. 27.—Factor 2: Muslim concentrations

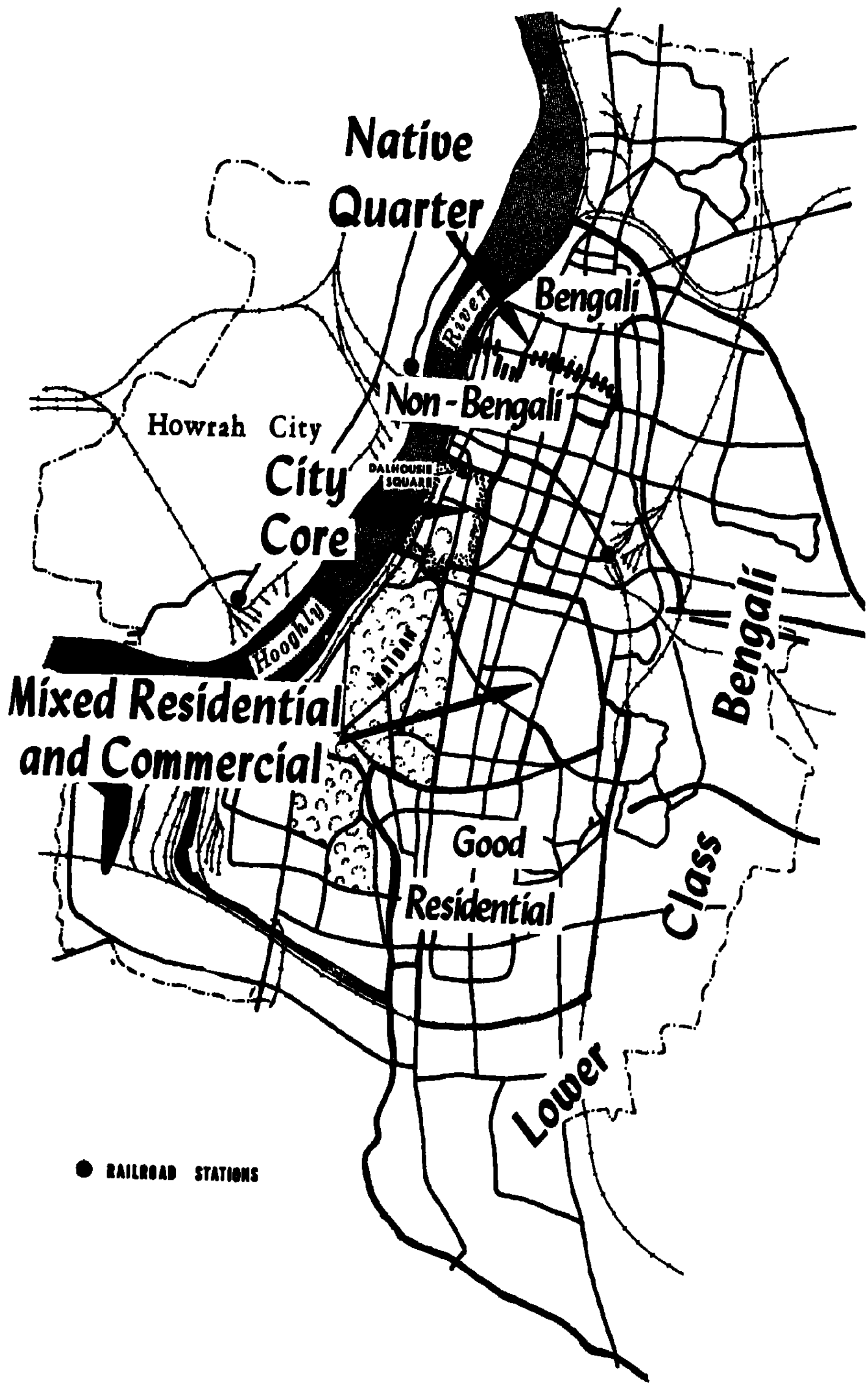


FIG. 28.—Calcutta's social areas

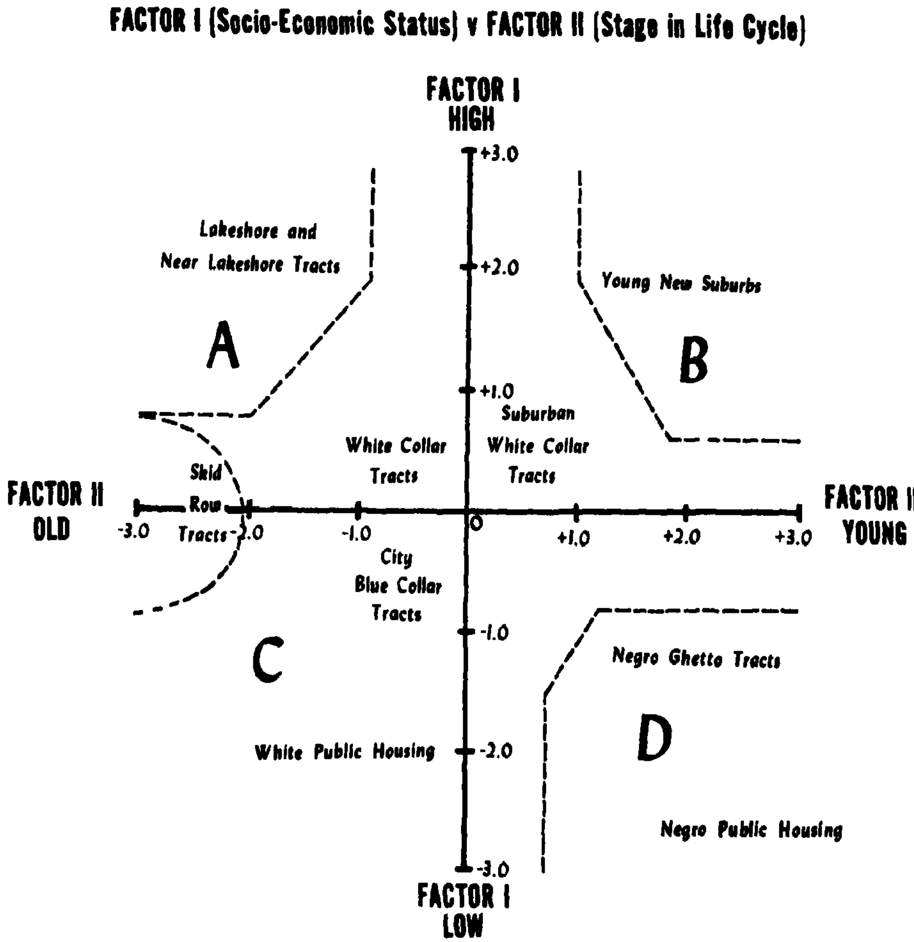


FIG. 29.—Chicago's social space

FACTOR 4 (Substantial Residential Areas) v FACTOR 1 (Familism and Land Use)

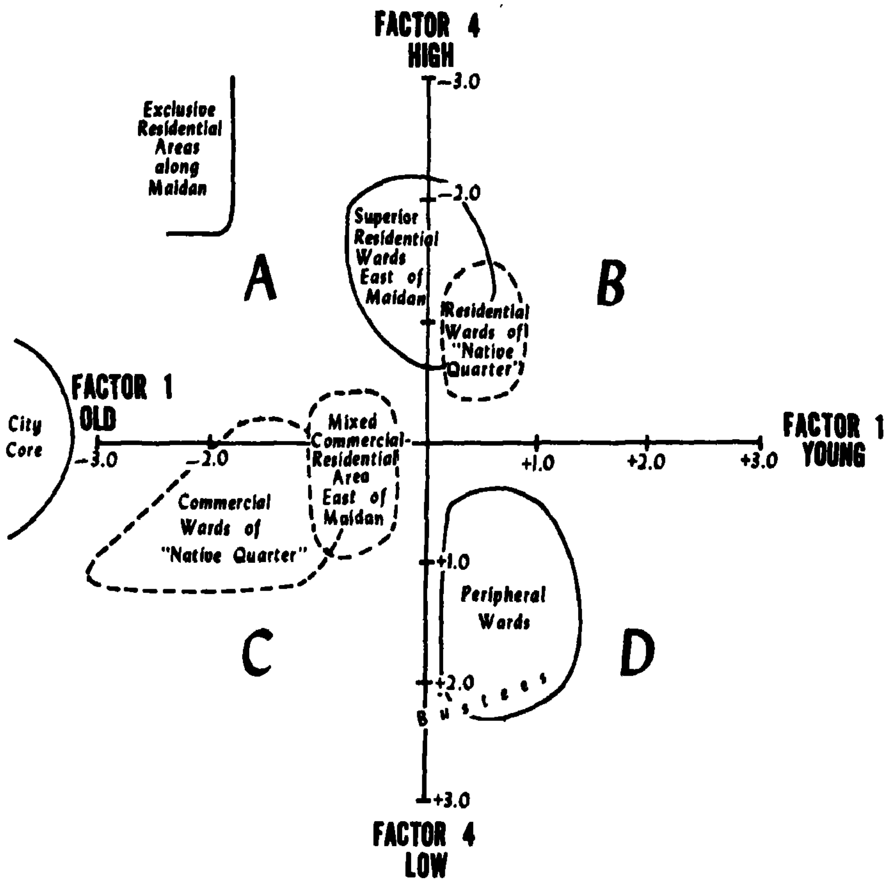


FIG. 30.—Calcutta's social space

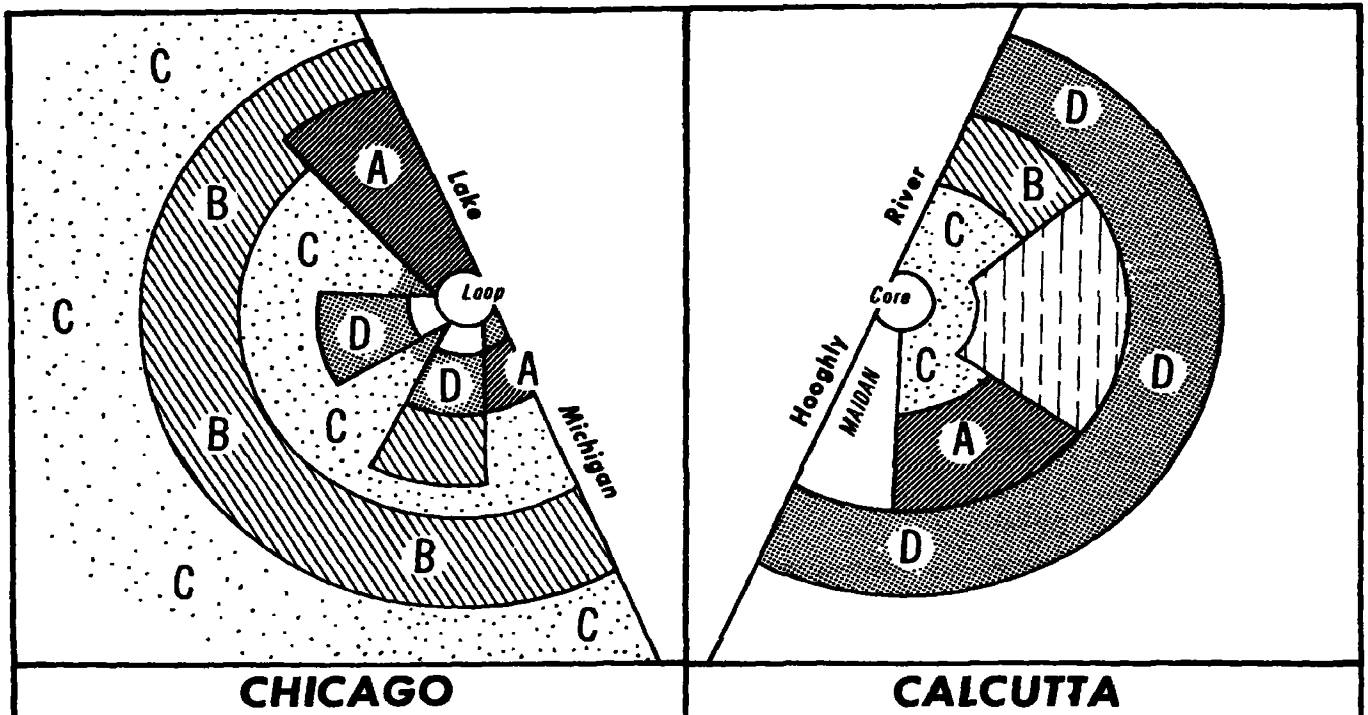


Fig. 31.—Generalized social areas

sions.[47] An 80 × 80 distance matrix portrays these similarities. Application of a grouping algorithm to this matrix permits the wards to be allocated to relatively homogeneous subsets, consistent with the notion of social areas.[48]

In the present case, a stepwise grouping algorithm that successively minimizes within-group variance was utilized.[49] The city was found to subdivide into a set of subregions (*social areas*), as portrayed in Figure 28. In brief, these comprise the city core, the superior residential areas in the northern part of the native quarter and east of the Maidan, the mixed commercial residential areas of the southern half of the native quarter and southeast of the city core, and the broad ring of peripheral wards. The legacy of the past remained strongly imprinted in the social space of the transitional city of 1961.

DISCUSSION

The factorial ecology belies the assumption made in a social area analysis, *stricto sensu*, of Hyderabad, however:[50] that the social space of an Indian city can be studied simply using Indian equivalents of the two principal Shevky-Bell constructs of social rank and urbanization. According to the Hyderabad research workers, the two constructs are constituted in the Indian case as follows:

I. Social rank
   A. Literacy among the general population
   B. Literacy among females
   C. Relative proportion of the population in scheduled castes (varies inversely with A and B)

II. Urbanization
   A. Proportion of workers in manufacturing industries
   B. Workers in commercial activities
   C. Workers in other services

Using these indicators, the Hyderabad analysts found that areas of lowest social rank formed a peripheral ring and also occupied the city's industrial zones, whereas the areas of highest social rank were found in the economic core of the city. But they also found that the residential quarters of workers in the three occupational groups used to define urbanization were highly segregated, almost mutually exclusive, so that the three constituent variables bore little relationship to each other. Nonetheless, they crossed the ratings of areas on the two constructs to develop four "types" of social areas.

In Calcutta, literacy and proportion of the population of "scheduled" castes are inversely related on factor 4, which separates the good-quality high-status residential areas from the others. The structure of this factor is similar to the Hyderabad social rank construct. But factor 1, the land-use and familism gradient, is the direct equivalent of the Shevky-Bell urbanization (family status) dimension. To confirm that this is so we can compare the distribution of social areas in the social status–family status space in Chicago (Fig. 29)[51] with a similar diagram for Calcutta in which factor 4 scores are used to locate wards on the ordinate and factor 1 scores to provide positioning with respect to the abscissa (Fig. 30). Similar segments of the social space are occupied by analogous social areas. In both instances, the high-status residential areas front superior amenities (lake in Chicago and Maidan in Calcutta). On the other hand, the geographic pattern of the other three social areas is inverted (Fig. 31).[52]

Calcutta's social geography still thus

[47] See, e.g., J. C. Gower, "Some Distance Properties of Latent Root and Vector Methods Used in Multivariate Analysis," *Biometrika*, LIII (1966), 325–38.

[48] See the extended discussion in Berry, *Essays on Commodity Flows . . .*, *op. cit.*

[49] Ward's criterion, *ibid.*

[50] Hyderabad Metropolitan Research Project, *Social Area Analysis of Metropolitan Hyderabad* (Hyderabad: Osmania University, 1966).

[51] Rees, *op. cit.*

[52] For a recent discussion of this point, see Noel P. Gist, "Urbanism in India," in Fava, *op. cit.*, pp. 22–32.

contains many traditional elements. But, if, as Bose argues, regional or ethnic groups in Calcutta are coming to bear the same relationships to each other as do the castes in India as a whole, the ethnic bases of the communities are being supplemented by strong functional correlates. There is increasing functional differentiation of land use, alongside regional occupational differentiation. And consistent with the notion of a city in transition, alongside these bases of differentiation are a strong land-use and familism gradient, comparable in many ways to "modern" American formulations of urbanization.[53] On the other hand, several dimensions are required to index status, because many of the ethnic linkages between occupation, region of origin, and caste remain—factor 6 differentiates the areas occupied by the Bengali "middle" commercial castes from those of the "scheduled" castes; factor 5 is needed to define the middle-status non-Bengali commercial zones; factor 4 separates the high-status residential areas but does not completely differentiate the commercial wards from the periphery; and factor 3 reveals the familiar "western" Hoyt-like axiality, but restricts it to literacy. Female employment is not part of the familism pattern, as in the Shevky-Bell model, but is related to the differences between Hindu and Muslim.

In terms of the factor models outlined in the fourth section of this paper, then, Calcutta conforms most closely to Combination Three, in which there is a separate family-status dimension, but socioeconomic status and minority group membership are linked. It is of some interest that the closest resemblances of Calcutta's ecology are to the ecology of cities in the American south, where traditionally one found links between race and status in a system of caste. One description, written in 1941, reads:

> "Life in the communities in the Deep South follows an orderly pattern. The inhabitants live in a social world clearly divided into two ranks, the white caste and the Negro caste. The colored castes share disproportionately in the privileges and obligations of labor, school, and government."[54]

This particular factor combination raises serious questions about relative emphasis placed on different dimensions in the choice model outlined at the beginning of this paper. Bose noted the interrelationships between scarcity and ethnicity in Calcutta. The resulting ethnic base of much decision making can be embodied in the residential choice model with appropriate modifications, however. To the two choice dimensions of individual social space discussed in the American context can be added a third dimension of ethnicity (the regional relativity of the "American" model must by now also be evident). The dimensions can be reordered, placing ethnicity at the head and altered in length, making ethnicity the most important element in the choice process.

Thus, in Calcutta, a new Rajasthani or Punjabi in-migrant usually seeks the safety of his ethnic enclave. Because most non-Bengali ethnic groups also occupy particular occupational niches, the variance in status within ethnic groups is substantially less than that between ethnic groups. In effect, ethnicity overrides choice based upon status, for the status and ethnic dimensions are collinear, with the latter more fundamental in defining the social dimensions within which choices are made. The exceptions are within the Bengali community, where occupationally derived differences corresponding to traditional caste differentiation still persist.

Similar less-than-economic decisions are involved in the creation of bustees, emphasizing the parallel relativity of location of

[53] Brian J. L. Berry, James W. Simmons, and Robert J. Tennant ("Urban Population Densities: Structure and Change," *Geographical Review*, LIII [1963], 389–405) present a comparison of Calcutta's density gradient with the same characteristic of Western cities.

[54] Allison Davis, Burleigh B. Gardner, and Mary R. Gardner, *Deep South* (Chicago: University of Chicago Press, 1941), p. 539. See also Christopher Jencks and David Riesman, "On Class in America," *Public Interest*, No. 10 (Winter, 1968), pp. 65–85.

many of the ethnic enclaves in physical space. Around the periphery of the city are found the poorest residents of and migrants to the city: scheduled castes, refugees from East Bengal, and lower-caste non-Bengalis. The process at work in the creation of the bustees they occupy was not one of orderly choice in an ethnically governed market framework but rather one of occupation, often illegally, of "empty" land at the urban periphery by poor families. There they erect a shack and attempt to survive in incredibly overcrowded and unhealthy living conditions. Abrams has vividly described the sequence of events: "As in a military campaign, some would bivouac during the night with their stock of materials behind a newly placed billboard. Next day, the horizon would be dotted with new rows of hovels, to which others would be added shack by shack, until the expansion was checked by a road, by a canal or by an owner prepared to spill blood."[55] With the passage of time, the bustee changes from a frontier community into a "regular," market-involved urban slum. Residences are bought and sold, and changing status is reflected in either physical improvement of the dwelling or a move to a new place of residence. Such changes are constrained, however, by the low status accorded the bustee dwellers by their occupations.

As the transitional process continues, however, one might reasonably expect the socioeconomic and ethnic bases of differentiation to separate. The expectation is, therefore, that differing urban ecologies related to differing factor combinations can be arranged along a scale of urban development from pre- to post-industrial forms, but testing of this notion awaits systematic comparative ecological analysis. On the other hand, this systematization may have been advanced by the formalization induced by the factorial approach.

University of Chicago

[55] Charles Abrams, *Man's Struggle for Shelter in an Urbanizing World* (Cambridge: M.I.T. Press, 1964), p. 16.

In Defense of Wirth's "Urbanism as a Way of Life"

Stanley S. Guterman

ABSTRACT

The criticisms that sociologists have voiced of Wirth's essay "Urbanism as a Way of Life" are examined. It is argued that the evidence on which these criticisms rely contains several inadequacies. To support Wirth's theory, data are presented showing a negative correlation between the size of the locality a person lives in and the intimacy of his friendship ties. The author concludes that a fresh look at Wirth's theory is needed utilizing research based on adequate measures and adequate design.

It is now thirty years since this journal published Louis Wirth's classic essay stressing the relative weakness of primary relations as among the distinguishing characteristics of the urban way of life. Wirth argued that the city's gigantic size, along with its density and its social and cultural heterogeneity, fosters an absence of personal acquaintanceship among interacting individuals. Interaction is based on segmentalized roles with a corresponding impersonality, superficiality, and transitoriness of social relations. All of these factors weaken, if not destroy, the bonds of sentiment and intimacy among the inhabitants.[1]

The flavor of his analysis is conveyed in the following passage:

> Characteristically, urbanites meet one another in highly segmental roles. . . . Their dependence upon others is confined to a highly fractionalized aspect of the other's round of activity. This is essentially what is meant by saying that the city is characterized by secondary rather than primary contacts. The contacts of the city may indeed be face to face, but they are nevertheless impersonal, superficial, transitory, and segmental. The reserve, the indifference, and the blase outlook which urbanites manifest in their relationships may thus be regarded as devices for immunizing themselves against the personal claims and expectations of others.[2]

CRITICISMS OF WIRTH

In the years since Wirth's paper originally appeared, his view of urban social relations has come to be widely questioned. Relying on a wealth of empirical research on ties with friends, neighbors, extended kin, and co-workers, scholars have contended that primary groups lead a vibrant existence and play an important role in the day-to-day lives of urban inhabitants. Wilensky and Lebeaux, for example, interpret the evidence to mean that the "alleged anonymity, depersonalization, and rootlessness of city life may be the exception rather than the rule. The typical city dweller maintains close relations with friends among either neighbors, or people in other parts of the urban area or both." In the opinion of these writers, the available data "suggest that the breakdown of primary group life and informal controls has been greatly exaggerated."[3]

In a review of forty studies on ties among extended kin, Sussman and Burchinal maintain that the "emphasis on the atomistic character of urban families has contributed to incorrect assumptions concerning interaction within the kinship matrix." A more accurate description of kin relations in the city, they believe, is provided by the notion

[1] See Louis Wirth, "Urbanism As a Way of Life," *American Journal of Sociology*, XLIV (July, 1938), 1–24.

[2] *Ibid.*, p. 12.

[3] Harold L. Wilensky and Charles N. Lebeaux, *Industrial Society and Social Welfare* (New York: Russell Sage Foundation, 1958), pp. 122 and 125.

of the "modified extended family" in which there "are mutual aid and social activities among kin related families."[4]

On the idea that areas within a city differ in the degree to which they are urbanized, Scott Greer says, "Although highly urbanized populations are not typical of most city dwellers (they are an extreme of a continuum), those who do exist deviate widely from the stereotype of the atomistic man. They are greatly involved in the family and kinship group, and they participate intensively in friendship and cliques."[5] If the "stereotype" is not descriptive even of the highly urbanized segments of a city, how much less so must it be of the other segments?

Most recently, Tomeh has written that a major criticism of "Wirth and others of the Chicago school is that they exaggerated the degree of secularization and disorganization that supposedly typifies urban communities." Research has disclosed "strong kinship and neighborhood ties in those areas of the city where such relations were often assumed to be quite weak."[6]

The more recent views of social life in the city, then, differ sharply in emphasis from those of Wirth—if, indeed, the two sets of views are not in outright conflict with each other. The proponents of these newer views, moreover, can marshal an impressive array of empirical studies to support the contention that isolation from friends and kin is a rare occurrence in the city.[7]

WEAKNESSES OF THE CRITICISMS

Despite the formidable case that Wirth's detractors appear to have made out, it is the thesis of this paper that they have not done full justice to his conception of urban life. When one examines the studies on which their arguments are based, he discovers that —with two partial exceptions that are discussed toward the end of this paper[8]—these studies are less than adequate for testing Wirth's views. To begin with, the measures employed often deal with the frequency with which an individual interacts, or gets together socially, with his associates.[9] The high rate of interaction that is generally found among city dwellers is thought to refute Wirth's views. The fallacy here is that Wirth was not concerned with the *quantity* of interaction. In one passage, for example, he explicitly remarked, "This is not to say that the urban inhabitants have fewer acquaintances than rural inhabitants, for the reverse may actually be true."[10] His concern, rather, was with the *quality* of interaction. Thus he spoke of the "impersonal, superficial, transitory, and segmental" character of social ties in the city and of "the reserve, the indifference, and the blase outlook which urbanites manifest in their relationships."[11] Insofar as the measures used in the empirical research do not tap the dimensions implicit in Wirth's discussion, this

[4] Marvin B. Sussman and Lee Burchinal, "Kin Family Network: Unheralded Structure in Current Conceptualizations of Family Functioning," *Marriage and Family Living*, XXIV (1962), 234–35.

[5] Scott Greer, *The Emerging City: Myth and Reality* (New York: Free Press, 1962), pp. 92–93.

[6] Aida K. Tomeh, "Participation in a Metropolitan Community," *Sociological Quarterly*, VIII (1967), 85.

[7] A number of these studies are cited in nn. 9 and 12 below.

[8] The partial exceptions are John P. Sutcliffe and B. D. Crabbe, "Incidence and Degrees of Friendship in Urban and Rural Areas," *Social Forces*, XLII (October, 1963), 60–67; and William H. Key, "Rural-Urban Differences and the Family," *Sociological Quarterly*, II (1961), 49–56.

[9] For studies dealing with frequency of interaction with associates, see Scott Greer, "Urbanism Reconsidered: A Comparative Study of Local Areas in the Metropolis," *American Sociological Review*, XXI (1956), 19–24; Scott Greer and Ella Kuba, "Urbanism and Social Structure: A Los Angeles Study," in Marvin B. Sussman (ed.), *Community Structure and Analysis* (New York: Thomas Y. Crowell Co., 1959), pp. 93–112; Morris Axelrod, "Urban Structure and Social Participation," *American Sociological Review*, XXI (1956), 13–18; Aida K. Tomeh, "Informal Group Participation and Residential Patterns," *American Journal of Sociology*, LXX (July, 1964), 28–35; and Tomeh, "Participation in a Metropolitan Community" (n. 6 above), pp. 85–102.

[10] Wirth, *op. cit.* (n. 1 above), p. 12.

[11] *Ibid.*

research cannot be regarded as truly testing his ideas.

I must immediately concede that there are studies that do use indicators referring to the quality of social relations. But even these investigations leave something to be desired. For they are not comparative. The samples are confined to persons living in large cities, so that they do not permit one to make inferences about differences between urban areas, on the one side, and small towns and rural areas, on the other.[12] As Wirth observed in one passage, "We must . . . infer that urbanism will assume its most characteristic and extreme form in the measure in which the conditions with which it is congruent are present. Thus the larger, the more densely populated, and the more heterogeneous a community, the more accentuated the characteristics associated with urbanism will be."[13] To the extent that Wirth intended to depict the ways in which highly urbanized settlements differ from less urbanized settlements, the absence of a comparative design vitiates the existing studies as a test of his theory.[14]

Finally, these studies are limited to relations with kin, friends, neighbors, and the like. To be sure, the studies frequently touch on relations with co-workers, but such relations are invariably viewed in terms of leisure-time, friendship activities. What is wrong here is that the relationships that these studies focus on constitute only a part of any person's network of social relations. Wirth never intended to confine his analysis to that part. On the contrary, his interest was in the total network. Consider his remark that the "distinctive features of the urban mode of life have often been described sociologically as consisting of the substitution of secondary for primary contacts.";[15] or his mention of "the number of people . . . with whom they [urban inhabitants] rub elbows in the course of daily life";[16] or his reference to the absence of "sentimental and emotional ties" and to "a spirit of competition, aggrandizement, and mutual exploitation."[17] The language Wirth uses in these and other passages seems to refer in large part to economic and business relationships. Because the existing studies are confined to ties with friends and kin, they fail to deal with the secondary types of interaction that play a large role in most urbanites' day-to-day existence. Thus even if research were to demonstrate that ties with friends and kin are no less "impersonal, superficial, transitory, and segmental" in the city than in small towns and rural areas, it would still not justify rejection of Wirth's ideas inasmuch as it would tell us nothing about the comparative quality of social relations outside of the kin and friendship networks.

Wirth's critics, then, appear to have allowed the deficiencies and limitations of the

[12] For investigations that contain measures of the quality of social relations but are not comparative in their design, see Wendell Bell and Marion T. Boat, "Urban Neighborhoods and Informal Social Relations," *American Journal of Sociology*, LXII (1956–57), 391–98; Marvin B. Sussman, "The Isolated Nuclear Family: Fact or Fiction," *Social Forces*, VI (1959), 333–40; Nicholas Babchuck and A. P. Bates, "The Primary Relations of Middle-Class Couples," *American Sociological Review*, XXVIII (June, 1963), 377–84; and Nicholas Babchuck, "Primary Friends and Kin: A Study of the Associations of Middle Class Couples," *Social Forces*, XLIII (May, 1965), 483–93. Two reviews of research focusing on the quality of ties among extended kin are Sussman and Burchinal, *op. cit.* (n. 4 above), pp. 231–40; and Joan Aldous, "Urbanization, The Extended Family and Kinship Ties in West Africa," *Social Forces*, XLI (October, 1962), 6–11.

[13] Wirth,*op. cit.* (n. 1 above), p. 9.

[14] Admittedly there are studies that compare residents of census tracts that vary in their degree of "urbanism" or "family status." But in a given study, these tracts are taken from a single metropolitan area. Whatever the merits of such studies, they are no substitute for research comparing residents of different-sized localities. For examples of research making intrametropolitan comparisons, see Greer, "Urbanism Reconsidered," *op. cit.* (n. 9 above); Greer and Kuba, *op. cit.* (n. 9 above); Tomeh, "Informal Group Participation and Residential Patterns," *op. cit.* (n. 9 above); and Bell and Boat, *op. cit.* (n. 12 above).

[15] Wirth, *op. cit.* (n. 1 above), pp. 20–21.

[16] *Ibid.*, p. 12. [17] *Ibid.*, p. 15.

existing studies to lead them astray.[18] Instead of designing investigations that would come to grips with the subtlety and complexity of Wirth's theory, they implicitly reinterpreted the theory to make it congruent with the procedures that the researchers had used—and in doing so, they stripped it of its trenchant qualities.

DATA SUPPORTING WIRTH'S VIEWS

In addition to these considerations rebutting the critics, I have data which—by showing a negative correlation between the intimacy of friendship ties and the size of the locality in which a person lives—support Wirth's depiction of social relations in the city. A by-product of a study of Machiavellianism among hotel employees, these data avoid two of the pitfalls of previous investigations. For one thing, the index of intimacy employed here seems a reasonably valid measure of the quality of friendship relations. For another, the sample includes residents of both large cities and small towns, thus permitting comparisons between respondents in the two.

SAMPLING AND FIELDWORK PROCEDURES

The sample consisted of 483 employees in twenty-six hotels operated by two chains and located in the eastern coastal states between Washington, D.C., and Bangor, Maine. All of the hotels were year-round establishments for transients. The main consideration in deciding which hotels in the two chains would be included in the study was to minimize the amount of travel that would have to be done in the course of the field work. This procedure was necessitated by limitations in the research budget.

Budgetary limitations also dictated the use of a self-administered questionnaire. Because giving such a questionnaire to manual workers would have created insurmountable problems, the sample was confined to employees on the white-collar and managerial levels. Among the types of personnel included were general managers and their assistants, heads of various departments, front-office clerks, switchboard operators, headwaiters, chefs, accountants, bookkeepers, security officers, and secretaries. In twenty-three of the hotels, the sample was simply defined as all white-collar and managerial employees. In the three largest hotels there were too many such employees to include all in the sample, so I stratified the population by the respondent's type of work, set a sample quota for each type, and selected a random sample from each stratum.[19]

The fieldwork was conducted in the autumn of 1963 and the winter of 1964. Each respondent filled out a highly structured self-administered questionnaire, which took on the average an hour and a half to complete. Usually the hotel management set aside a room in which groups of respondents assembled to fill in the questionnaire on company time. The number of respondents filling out the questionnaire at the same sitting varied from one to fifteen. As each respondent completed the questionnaire, I briefly checked it over and had him correct any gross errors.

To maximize the candor of replies to the questionnaire, it was important to convince respondents that no information about individuals would be disclosed to the management. In addition to oral and written assurances of confidentiality and not requiring the respondent to identify himself in the questionnaire, I took several steps to demstrate my good faith. Accompanying each

[18] Let me emphasize that in calling attention to the inadequacies of the studies cited here, I am speaking strictly from the standpoint of their suitability for testing Wirth's theory. From other standpoints, including that of their intrinsic merit, these investigations may be unexceptionable.

[19] Following are some of the characteristics of the sample. Almost two-thirds consisted of males. There was a fairly even distribution of respondents on the age variable, those in the "50 and over" age category, however, being somewhat more numerous than those in the other three age categories (18–29, 30-39 and 40–49). Approximately half of the sample came from three giant urban hotels, each of which had no less than 700 personnel. At the other extreme, about 30 per cent worked for units having less than 90 employees each.

questionnaire was a letter on university stationery emphasizing the purely academic nature of the research and promising to send each respondent a summary of the preliminary findings. This promise was later kept. On the first page of the instrument, moreover, was an "official acknowledgement" of financial assistance from the United States Department of Health, Education, and Welfare.

Finally, participation in the study was voluntary. Respondents were expressly told that they could refuse to fill out a questionnaire if they so wished. Aside from my probable inability in most instances to make individuals participate, there was an additional pragmatic reason for this practice. Had I been seeking information about individuals in order to turn over such information to management, most respondents would have expected me to permit no refusals. The policy of keeping participation voluntary was thus designed to underscore my independence of management.

MEASUREMENT PROCEDURES

The index of the intimacy of friendship ties is based on a section of the questionnaire in which the respondent was asked to list the first names of the "*five* persons (or married couples) *not related* to you, *whom you know best*." The respondent, in effect, gave five replies to each item, one for each of the friends listed. One item, for example, read "You know the immediate family of this person well." A response consisted of placing a check mark under the name of each friend to whom the statement was applicable.

Four dimensions have been employed in constructing the intimacy index. The first three are taken from Sorokin's modalities of social interaction.[20]

1. Intensity refers to the strength of affect that an individual feels toward another person and the extent to which an individual is psychologically affected by the actions of another person. One of the items intended to tap this dimension asked if the respondent "would feel badly if you happened to lose touch with this person."

2. Extensity, according to Sorokin, is the "proportion of the activities and psychological experiences involved in interaction out of the sum total of the activities and psychological experiences of which the person's whole life process consists." This dimension appears synonymous with what Wirth had in mind when he wrote of segmentalization. The one item designed to measure extensity in the questionnaire inquired about the range of topics the respondent talked about when he was with each friend.

3. The duration of a relationship is simply the length of time that the relationship has existed. To measure this dimension, I have calculated the mean percentage of the respondent's life during which he knew the five friends listed.

4. The interconnectedness of the respondent's circle of friends has been gauged by the extent to which the respondent knew the other associates of each friend.[21] One item asked whether the "two closest friends of this person (not including yourself) are good friends of yours."[22]

[20] See Pitirim A. Sorokin, *Social and Cultural Dynamics* (New York: American Book Co., 1937), III, 6–15.

[21] The idea for this fourth dimension comes from Elizabeth Bott, *Family and Social Network* (London: Tavistock Pub., 1957).

[22] The intimacy index is based on six items, each of which is assigned code values of 0 to 3. A "no answer" to any constituent item results in the respondent being left out of the analysis. The theoretical range is from 0 to 18. The scoring of the first four items is based on the number of friends to whom, according to the respondent, a given statement applies.

1. You would feel badly if you happened to lose touch with this person.
2. You know the immediate family of this person well.
3. This person has given you a gift within the past 12 months (for your birthday, for Christmas, etc.).
4. The two closest friends of this person (not including yourself) are good friends of yours.
5. Write in the number of years you have known each person. [This item was used to compute the mean per cent of the respondent's life that he had known the five friends listed. This proportion determined the score assigned to each respondent.]
6. How about the *number of different kinds* of things you talk about with each of the above

To measure the size of the locality, I have relied on an item in the questionnaire which asked the respondent to name the "town (or locality) and state" in which he was living at the time he answered the questionnaire. With this information, it was easy to go to the 1960 Census to obtain the population of the locale. If a respondent resided in an urbanized area, the size variable refers to the population of the urbanized area—not the population of the municipality—in which he lived. Three urbanized areas were included in the study. Two of them, Boston and Washington, had populations of between 1.5 and 2.5 millions; the third, New York City, a population of slightly over 14 million.[23] Of the respondents who lived outside of these three areas, none lived in communities having more than 120,000 inhabitants; over 90 per cent lived in towns having a population of less than 40,000; and over 75 per cent lived in towns having less than 20,000 population.

persons? *Write in the letters* designating those with whom you discuss:

One or two subjects of mutual interest___
Several subjects of mutual interest___
Quite a few subjects of mutual interest___
A very wide range of subjects of mutual interest___

(The first alternative was given a score of 1, the last a score of 4, and those in between scores of 2 and 3. The respondent received a score for each friend, and the scores for the five friends were then summed to yield a total score for the item. The latter score determined the code value assigned for this item.)

The work of Robin M. Williams, Jr., on friendship proved a valuable source of suggestions for item formulation; see Williams, "Friendship and Social Values in a Suburban Community" (Eugene: University of Oregon, 1956 [mimeographed]).

[23] The urbanized area is a census concept that refers to a large city—in the 1960 Census, one of 50,000 or more population—and the surrounding suburban territory. The effect of using the urbanized area instead of the town or city as the unit for measuring population is to classify suburbs by the population of the total urban complexes of which they are a part. Use of the urbanized area was dictated by considerations associated with the major study—that on Machiavellianism—from which the data given here are drawn. For a formal definition of the urbanized area, see U.S. Bureau of the Census, *United States Census of Population: 1960*, I: *Characteristics of Population*, Part A: "Number of Inhabitants" (Washington, D.C.: Government Printing Office, 1961), xviii–xix.

THE FINDINGS AND THEIR LIMITATIONS

Table 1[24] examines the relation of intimacy to the size of the locale in which the respondent lived at the time of the fieldwork. The data show a negative correlation: residents of large cities and their suburbs are less likely to have close friendships than residents of small towns. Insofar as this finding evidences the relatively unprimary character of social relations in urban settings, it raises even further doubts about the arguments of Wirth's critics.

TABLE 1

INTIMACY OF FRIENDSHIP TIES BY POPULATION OF URBANIZED AREA OR LOCALITY (IF OUTSIDE URBANIZED AREA) WHERE RESPONDENT CURRENTLY LIVES

Intimacy	Population		
	Under 120 Thousand (%)	Between 1.5 and 2.5 Million (%)	14 Million (%)
Low	42	47	58
Medium	24	26	19
High	34	27	23
	100	100	100
	(131)	(185)	(145)

NOTE.—$\chi^2 = 8.77$; d.f. = 4, $.10 > P > .05$.

Although our data are highly suggestive, they nonetheless have certain distinct limitations. First, the sampling procedures employed make it hazardous to generalize the findings. Aside from the fact that the sample was confined to white-collar and managerial employees in the hotel industry, selection of

[24] The *N*'s in the table add up to less than the size of the sample (483). This is due to a lack of information necessary for classifying some respondents on one or the other variable. In coding respondents on the intimacy index; e.g., it was my practice to give no score to those individuals who failed to reply to any of the items in the index.

the hotels for inclusion in the study was not based on probability sampling procedures. Added to this is the fact that in three of the hotels the proportion of the intended sample that refused to participate was exceedingly high (i.e., over 30 per cent). These three hotels were all in urbanized areas of 1.5 million or more inhabitants. That the high refusal rate is not evenly distributed over the range of the independent variable may have biased the findings here.[25]

A second limitation is that the only measure of the quality of a person's social relations used here is that of the respondent's friendship ties. I have presented data neither on relations with extended kin nor on relations with other kinds of associates.

Finally, the index of the intimacy of friendship ties is based solely on the respondent's report. I did not give questionnaires to the five friends named by each respondent in order to check the reliability of his report.

COMPARABLE DATA FROM PREVIOUS STUDIES

Apart from the inconclusiveness of the present data, uncertainties emerge from two studies that offer data comparable to those presented here. Sutcliffe and Crabbe studied five groups—each consisting of eight Australian first-year university students—matched on a number of variables. The first three groups lived in Sydney, the fourth in the suburbs, and the fifth in towns of less than 40,000 population. After listing all of the "various people you know and meet," the respondents answered seven items about their relationship with each such person. On the basis of the replies, the persons named were classified into three categories varying in degrees of friendship—"best friend," "friend," and "acquaintance." The findings of the study unfortunately are equivocal. The respondents who lived in the small towns characterized fewer persons as "best friends" than the respondents who lived in the urban and suburban areas. If, however, one lumps the "best friends" and the "friends" together, it turns out that the findings are just the opposite: the small town residents named a greater number than the urbanites and the suburbanites.[26]

With a Guttman scale measuring participation in the extended family, Key studied 357 individuals who lived in different-sized localities in the Midwest. He failed to find any linear relation between size and participation. His results are thus at variance with those presented here.[27]

The discrepancies between these two studies and the present one can perhaps be explained by examining some differences in the research procedures. First, the studies did not measure the quality of social relations in the same manner. The items in the Sutcliffe and Crabbe study deal with such things as willingness to lend articles, amount of confiding, and willingness to support the other person in the face of criticism. Aside from the fact that Key's measure refers to kin while mine refers to friends, his measure is conceptually impure in that it contains items that gauge both the frequency and quality of interaction. And only a minority of the five items in his scale—one dealing with the frequency of lending and borrowing and the other with the frequency of "favors other than lending"—measure quality. Thus the measures used in these studies seem to be tapping dimensions that are different from those on which the present

[25] There is some evidence that the effect of the high refusal rates operates against the arguments presented in this paper. We know that the proportion of respondents who are Machiavellian is somewhat lower in the hotels with the high refusal rates than in the other hotels located in the cities of the same size. We also know that there is a slight negative correlation between an individual's Machiavellianism score and the intimacy of his friendship ties. Thus the high refusal rates may have served artificially to raise the percentage in the cities having high intimacy scores and thereby spuriously to lower the correlation between size of locale and intimacy. This is only surmise on my part. But if it is correct, the evidence offered here in support of Wirth would have been even more favorable to his position had the high refusal rates not occurred.

[26] See Sutcliffe and Crabbe, *op. cit.* (n. 8 above).

[27] See Key, *op. cit.* (n. 8 above).

study is based (intensity, extensity, duration, and the interconnectedness of the social network). In my opinion, the measure used here does a much better job of capturing the nuances of quality as Wirth implicitly conceived it in his essay. In any event, the correlation between urbanism and the quality of social relations could conceivably depend on the dimensions used to measure the latter variable.

In addition, Key's study uses different cutting points for the population variable than those used here. In his data, the category at the high end of this variable consists of metropolitan areas of more than 100,000 population. In my study, by contrast, the medium and high categories consist of urbanized areas having *at least* 1.5 million population. One doubts that most, if any, of the respondents in his high category would fall into the medium or high category in the present study. If so, the discrepancies in our findings could, in part, be due to the fact that we focused on different portions of the population variable. Should this conjecture be true, there may be a relationship between urbanism and the quality of social relations, but it may hold only for the upper portion of the urbanism variable. In other words, it is conceivable that disintegration in the quality of relations does not occur in marked form except in metropolitan areas of several million inhabitants.

CONCLUSIONS

In summary, I have argued that the evidence that Wirth's critics rely on to refute his characterization of social relations in the city is inadequate in one or more respects. First, the studies often deal with the quantitative aspect of social relations, ignoring the qualitative aspect that was Wirth's central concern. Second, the studies are not comparative: with few exceptions, they fail to include the residents of both large cities, on the one side, and small towns and rural areas, on the other. Third, their focus is on relations with friends and kin; they ignore other relationships.

In support of Wirth's views, I presented data showing a negative association between the size of the locality a person lives in and the intimacy of his friendship ties.

Given the limitations of my data and given the discrepancies between my findings and those of two previous comparable studies, it would clearly be unwarranted to argue that this discussion has confirmed Wirth's theory of urbanism as a way of life. What this discussion has accomplished is admittedly more modest but nevertheless important: it has demonstrated the need for a fresh look at that theory utilizing research based on adequate measures and adequate design.

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An Index of Riot Severity and Some Correlates[1]

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ABSTRACT

While certain variables do not correlate with the presence or absence of riots in American cities, they do correlate with riot severity. Such variables are more influential in determining the severity of a riot, once it has begun, than they are in determining the outbreak of that riot. The index—a Guttman-type scale developed from materials describing disorders in 1967—suggests that the events that constitute riots and civil-criminal disorders are not bizarre, non-patterned, or randomly generated. On the contrary, employing the properties of Guttman scales, we may predict the sequence of events for levels of riot severity.

This paper is divided into two sections. The first describes an Index of Riot Severity. The second reports the results of an examination of select social, economic, and demographic variables to ascertain which among them is correlated to riot severity as measured by the Index.

AN INDEX OF RIOT SEVERITY

The Index is based upon the analysis of seventy-five riots and civil-criminal disorders reported to have taken place during the summer of 1967.[2] The analysis employed Guttman scaling techniques, and the derived Index is a Guttman-type scale. Information used in the construction of the scale was provided by mayors' offices at the request of a U.S. Senate subcommittee.[3] The subcommittee's request for information from cities experiencing major riots and civil-criminal disorders covered the years 1965–67. For each of these years the subcommittee tabulated the following kinds of information: date of disorder, the city in which it occurred, 1960 population, percentage Negro, number of civilians and law officers killed and injured, types of criminality (e.g., sniping, vandalism, arson, interference with firemen), the number of arrests, the number of convictions, police action, estimated financial losses, and a mayor's report of "triggering incident."

The information selected for the construction of the Index was based upon the subcommittee's tabulations for 1967, from April 1 to September 8. While an index of riot severity might ideally incorporate information from diverse sources, the Index reported here includes only those items that could be directly ascertained from official mayors' reports to the Senate subcommittee.

The final scale includes the following seven items of riot severity: killing, calling of the National Guard, calling of the state police, sniping, looting, interference with firemen, and vandalism.[4] The Coefficient of

[1] I wish to acknowledge the assistance of Alfred J. Claassen, Hunter H. Durning, Patricia L. Gotchall, and Sigmund W. Krane, who prepared some of the materials used in the following analysis. Tables 1 and 2 were previously published in "1967 Riots: A Test of the Congruity of Events," *Social Problems*, XVI, No. 2 (Fall, 1968), 193–98.

[2] Reported by mayors in *Riots: Civil and Criminal Disorders* (Hearings before the Permanent Subcommittee on Investigation of the Committee on Investigation of the Committee on Government Operations, U.S. Senate, 90th Cong., 1st sess., November 1, 2, 3, and 6, 1967. Part 1. [Washington, D.C.: Government Printing Office, 1967]).

[3] Of 137 cities contacted, 128 responded with reports for the years 1965–67.

[4] These items are ordered from most to least severe, or from least to most frequently reported. One aspect of the scale as shown in Table 1 should be noted. Of the cities, 4 per cent did not report any of the items in the ordered sequence generated by the Guttman scale. Since the item defined as "not responding to any of the items" lacks sufficiently

Reproducibility of this Guttman scale of riot severity for seventy-five riots and seven items of severity is 92 per cent. Table 1 shows the obtained scale.

With one exception, the items included in the scale are in the same form as they appeared in the mayors' reports, that is, indications of the presence or absence of the event. The exception involves the alteration of a quantitatively defined variable, number of civilian and police deaths, so that it could be coded simply as a dichotomy: "no deaths" and "*N*-deaths."

Events were selected as items for the scale if they appeared to be logically linked or related to riot severity. To obtain a Guttman scale means, of course, that the items are located in a systematic and cumulative fashion along the unidimensional continuum of severity and that the continuum also locates cities on the severity scale.

Cities are organized into eight scale types, or eight categories ordered on the severity dimension. Table 2 shows the cities that compose each scale type. The reader will note that each riot is treated as a unit.[5] The alternative is to treat each city as a unit, thus eliminating the repetition of those cities with two or more riots. As a consequence of adopting the riot as the unit in developing the Guttman scale, one city is included three times; three cities are included twice each; and one city is included four times.[6]

large marginals to be considered "scalable," it might be dropped from the scale. The inclusion of that item, however, does not inflate the Coefficient of Reproducibility; in fact, its inclusion deflates the Coefficient. In order to preserve the substantive character of the sequence of events, the item is included in the scale presented here.

[5] This approach has been used widely. See Stanley Lieberson and Arnold R. Silverman, "Precipitants and Conditions of Race Riots," *American Sociological Review*, XXX (December, 1965), 887–98; and Milton Bloombaum, "The Conditions Underlying Race Riots as Portrayed by Multi-dimensional Scalogram Analysis: A Re-analysis of Lieberson and Silverman's Data," *American Sociological Review*, XXXIII (February, 1968), 76–91.

[6] Cincinnati reported three disorders; Erie, Houston, and San Francisco reported two disorders; and New York City reported four.

SOME CORRELATES: A COMPARISON WITH OTHER STUDIES

Explanations of riots have followed from different lines of inquiry. Newspaper reports, personal accounts, and biographical statements have supplied us with a body of popular notions about riots, their causes, courses, and effects. Findings of technical studies have thrown additional light on the characteristics of large numbers of disorders.[7] In these studies, findings usually emerge from a comparison of riot with non-riot (control) cities.[8] In the discussion that follows, however, variables are examined in terms of their relationship to *riot severity*, not in terms of their relationship to the *presence* or *absence* of rioting.

Lieberson and Silverman studied seventy-six events classifiable as Negro-white race riots between 1913 and 1963. They found that when riot and control cities were compared, (1) percentage increase of Negro population was unrelated to the presence of riots, (2) racial composition did not distinguish between riot and control cities, and (3) housing measured in terms of higher or lower quality did not distinguish between riot and control cities.[9]

Percentage increase of non-whites.—Lie-

[7] Recent examples, to name just a few, include Peter M. Green and Ruth H. Cheney, "Urban Planning and Urban Revolt: A Case Study," *Progressive Architecture* (January, 1968), pp. 134–56; Anthony Oberschall, "The Los Angeles Riots of August 1965," *Social Problems*, XV (Winter, 1968), 322–41; Tom Parmenter, "Breakdown of Law and Order," *Trans-action*, IV (September, 1967), 13–22, which is but one of a series of papers in the same issue analyzing urban violence; Jerry Cohen and William S. Murphy, *Burn, Baby, Burn* (New York: E. P. Dutton & Co., 1966); Morris Janowitz, *Social Control of Escalated Riots* (Chicago: University of Chicago Center for Policy Study, 1968); Lieberson and Silverman, *op. cit.* (n. 5 above); Bloombaum, *op. cit.* (n. 5 above); and Irving Louis Horowitz and Martin Liebowitz, "Social Deviance and Political Marginality: Toward a Redefinition of the Relation between Sociology and Politics," *Social Problems*, XV (Winter, 1968), 280–96.

[8] Lieberson and Silverman, *op. cit.*, and Bloombaum, *op. cit.*

[9] These are only three of the variables that were examined by Lieberson and Silverman.

berson and Silverman found that percentage increase of Negroes did not differentiate between riot and control cities. They discovered "no sizable difference between riot and control cities in their percentage gains in Negro population during the decades."[10] For each of the cities in the scale reported here,[11] the percentage increase of non-whites in the total population of the riot city in the decade 1950–60 was computed.[12] The Spearman rank correlation coefficient, computed between the *scale types* ranked on severity and mean percentage non-white increase, was .833, significant beyond the .01 level.[13] It seems that while percentage increase of Negroes is unrelated to the presence or absence of a riot, the percentage increase of non-whites is significantly related to riot severity as measured by the Index. Once a riot takes place, the greater the percentage increase of non-whites, the greater the severity of the riot.

Racial composition.—Included in each mayor's report was information pertaining to the proportion of the city's population that was Negro. For each scale type the mean percentage of Negroes was computed. The rank order of scale-type mean percentages did not correlate significantly with the Index. A second step involved the elimination of southern cities and again computing scale-type mean percentages. The rank correlation was not significant. It appears that racial composition is neither related to the presence or absence of riots[14] nor to riot severity as measured by the Index.

Housing.—Six variables relating to housing were examined to ascertain their relationship to riot severity: (1) percentage living in multi-family dwellings, (2) percentage living in newer than ten-year-old dwellings, (3) percentage in units with substandard plumbing, (4) percentage in housing with more than one person per room, and (6) percentage who owned their own homes.[15] For each scale type, the median percentage was computed for each of the six variables. Of the six variables, only one showed a statistically significant rank correlation between median percentage and riot severity. Measured by the Spearman rank correlation, riot severity and percentage living in newer than ten-year-old dwellings correlated −.86, significant beyond the .01 level. Once a riot occurs, the fewer non-whites in dwellings constructed in the decade 1950–60, the greater the severity of the riot. None of the other five housing variables showed any systematic variation by the Index of Riot Severity.

Economics of housing.—Three additional housing variables were examined: (1) median non-white rent, (2) median value of non-white place of living, and (3) median percentage of non-white home owners.[16] For each of the variables, scale-type medians were computed and correlated with riot severity. None showed any relationship to severity.

Density.—It is popularly believed that areas of high population density provide arenas for a variety of disruptive behaviors. For each scale type the mean population density was computed.[17] The Spearman rank correlation coefficient between scale types and mean population density was .18,

[10] Lieberson and Silverman, *op. cit.*, p. 893.

[11] Southern cities were omitted from the calculations.

[12] Source of information is *United States Bureau of Census: County and City Data Book, 1967* (Washington, D.C.: Government Printing Office, 1967), pp. 464–572.

[13] A description of Spearman rank correlation may be found in Sidney Siegel, *Nonparametric Statistics* (New York: McGraw-Hill Book Co., 1956), pp. 202–11.

[14] As reported in Lieberson and Silverman, *op. cit.* (n. 5 above), pp. 893–94.

[15] Source of information on non-whites is *United States Bureau of the Census: Census of Housing* (Washington, D.C.: Government Printing Office, 1960), Table 8.

[16] Source of information for variables (1) and (2) is *ibid.* Source of information for variable (3) is *United States Bureau of the Census: Census of Population* (Washington, D.C.: Government Printing Office, 1960), Table 78.

[17] Source of information is *United States Bureau of Census: County and City Data Book, 1967* (n. 12 above), pp. 464–573.

not significant at the .05 level. It may be concluded that intensity of riots and civil-criminal disorders as measured by the Index is not related to population density.

Larceny and assault.—It may be thought that cities with traditions of violence and criminality lend themselves to the generation of more severe riots than cities without such a tradition. One measure of criminal tradition is indicated by the aggravated assault rate and the larceny rate of each city.[18] The mean aggravated assault rate for each scale type was computed and ranked. The rank correlation between riot intensity and mean aggravated assault rates as measured by the Spearman rank correlation is negligible.

In the same fashion, the mean larceny rate for each scale type and riot intensity were correlated. The Spearman rank correlation coefficient was .24, not significant at the .05 level.

Two measures of criminality are found to be unrelated to the severity of disorders as measured by the Index.

Police preparation.—To what extent are the preparations of the police before the outbreak of disorders related to the subsequent intensity of the disorder? Eight types of information on police preparations were examined.[19] They include: (1) presence of in-service training for police, (2) riot training for police, (3) presence of an auxiliary police force, (4) riot training for the auxiliary police, (5) mutual aid arrangements with a neighboring law enforcement agency, (6) presence of a special riot unit on the police force, (7) presence of a special riot plan, and (8) use of dogs to control crowds of people.

Only two of the above variables showed a tendency to vary with riot severity, and just one correlated significantly. The correlation between riot severity and mean percentage of scale-type cities employing dogs was .56, not significant at the .05 level. The mean percentage of cities in each scale type that reported having a special riot plan was computed and correlated with riot severity. The Spearman rank correlation coefficient was .70, significant beyond the .05 level.[20]

Interpretation of the relationship between the variable police preparation and riot severity poses special difficulties. Previously discussed variables did not imply direct human involvement in anticipation of riots and civil-criminal disorders. Consequently a significant correlation between riot intensity and police preparation may be interpreted in different ways. The correlation may lend support to the assertion that the magnitude of the riot was accurately assessed by the police beforehand and that their preparations thus corresponded to the severity of the riot. On the other hand, a contradictory assertion suggests that police preparation is an example of the self-fulfill-

TABLE 1

GUTTMAN SCALE OF RIOT SEVERITY

Scale Type	% Cities ($N=75$)	Items Reported	Scale Errors
8.	4	No scale items	2
7.	19	"Vandalism"	10
6.	13	All of the above+ "interference with firemen"	3
5.	16	All of the above+ "looting"	3
4.	13	All of the above+ "sniping"	7
3.	7	All of the above+ "called State Police"	4
2. . . .	17	All of the above+ "called National Guard"	11
1.	11	All of the above+ "law officer or civilian killed"	2
Total. . .	. . . . . .	. . . .	42

NOTE.—Coefficient = 1−42/515 = 92%.

[18] Source of information is *Uniform Crime Reports—1966: Crime in the United States* (Washington, D.C.: Government Printing Office, 1967), pp. 170–85.

[19] Source of information is *Municipal Yearbook, 1966.* (Chicago: International City Managers' Association, 1966), pp. 445–65.

[20] After correction for ties.

ing prophecy and is a factor in the escalation of the riot.

The absence of a statistically significant correlation between police preparation and subsequent riot intensity may also be interpreted in the same fashion. Either the police overprepared, or an accurate assessment of the potential led to preparations that did, in fact, reduce the intensity of the disorders. A final interpretation suggests that adequate police preparation might even have prevented the start of the disorder.

Sniping.—Janowitz distinguished between two patterns of riots in American cities, communal and commodity riots.[21] The latter are of recent vintage and characterized by widespread use of rifles and other

[21] Janowitz, *op. cit.* (n. 7 above), esp. pp. 9–13.

TABLE 2

CITIES BY SCALE TYPES

Scale type 8:
- Louisville, Ky.
- West Palm Beach, Fla.
- Mount Clemons, Mich.

Scale type 7:
- Fresno, Calif.
- Kansas City, Mo.
- Omaha, Neb.
- Montgomery, Ala.
- New Britain, Conn.
- Houston, Tex.
- Sacramento, Calif.
- Massillon, Ohio
- Rockford, Ill.
- Elgin, Ill.
- Kalamazoo, Mich.
- Albany, N.Y.
- Lima, Ohio
- Peekskill, N.Y.

Scale type 6:
- Hamilton, Ohio
- Long Beach, Calif.
- Paterson, N.J.
- Erie, Pa.
- Wyandanch, N.Y.
- Hartford, Conn.
- Greensboro, N.C.
- Erie, Pa.
- New York City, N.Y.
- Hattiesburg, Miss.

Scale type 5:
- Birmingham, Ala.
- Cincinnati, Ohio
- Buffalo, N.Y.
- Dayton, Ohio
- Mt. Vernon, N.Y.
- Passaic, N.J.
- Cincinnati, Ohio
- New York City, N.Y.
- Washington, D.C.
- Syracuse, N.Y.
- Minneapolis, Minn.
- Toledo, Ohio

Scale type 4:
- San Bernardino, Calif.
- Houston, Tex.
- Peoria, Ill.
- Waterbury, Conn.
- Riviera Beach, Fla.
- Chicago, Ill.
- New York City, N.Y.
- Englewood, N.J.
- Boston, Mass.
- New York City, N.Y.

Scale type 3:
- New Haven, Conn.
- Saginaw, Mich.
- Wichita, Kan.
- Wilmington, Del.
- Flint, Mich.

Scale type 2:
- Portland, Ore.
- South Bend, Ind.
- Providence, R.I.
- Tampa, Fla.
- Waterloo, Iowa
- San Francisco, Calif.
- Poughkeepsie, N.Y.
- Phoenix, Ariz.
- Nashville, Tenn.
- Grand Rapids, Mich.
- Cambridge, Md.
- Tucson, Ariz.
- San Francisco, Calif.

Scale type 1:
- Milwaukee, Wis.
- Rochester, N.Y.
- Detroit, Mich.
- Pontiac, Mich.
- Cincinnati, Ohio
- Newark, N.J.
- Plainfield, N.J.
- Jackson, Miss.

arms among rioters. It can be seen that in the scale of riot severity reported here, thirty-six scale-type cities reported sniping (see Tables 1 and 2). There are scale errors; in fact, thirty-two cities reported sniping. Thus sniping appears as an event midway along the continuum of severity, occurring in less than 50 per cent of the riots and civil-criminal disorders reported to the Senate subcommittee.

CONCLUSION

It has been shown that while certain variables do not correlate with the presence or absence of riots in American cities, they do correlate with riot severity. Such variables are more influential in determining the severity of a riot, once it has begun, than they are in determining the outbreak of that riot.

The Index—a Guttman-type scale—suggests that the events that constitute riots and civil-criminal disorders are *not* bizarre, non-patterned, or randomly generated.[22] On the contrary, employing the properties of Guttman scales, we may predict the sequence of events for levels of riot severity. The Index was developed from materials describing disorders occurring in 1967. Future riots and civil-criminal disorders may not be similarly constituted. Either the set of events or the sequence of events may be altered by social existential conditions.

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[22] For a review and criticism of the tradition which interprets crowd behavior as bizarre and sociologically primitive, see Carl J. Couch, "Collective Behavior: An Examination of Some Stereotypes," *Social Problems*, XV (Winter, 1968), 310–22; and Jules J. Wanderer, "1967 Riots: A Test of The Congruity of Events," *Social Problems* (Fall, 1968), pp. 193–98.

Teachers' Strike: A Study of the Conversion of Predisposition into Action[1]

Stephen Cole

ABSTRACT

Five non-teaching statuses (e.g., religion) were found to influence the predisposition of teachers toward a strike. But not all teachers acted in accord with their predispositions. This paper reports research on the social processes influencing the conversion of predisposition into action. Teachers were most likely to act in accord with their predisposition when they received social support from colleagues and were not under cross-pressure. Four different types of cross-pressure led teachers to avoid making a clear decision. Teachers who supported the union but feared sanctions were less likely to picket than those who did not fear sanctions. The social-psychological and structural determinants of fear are examined. Teachers who were negatively predisposed toward the strike but had colleagues as a salient reference group were less likely to cross the picket line than those who did not have colleagues as a reference group. Knowing whether or not colleagues were a reference group enabled us to predict the strength of the contextual variable when a context was in opposition to an individual's predisposition.

When survey research first became popular, its users employed primarily social-psychological variables characterizing individuals. In recent years sociologists have attempted to make survey research more sociological. In the late 1950's we began to use a mode of analysis called "contextual analysis," in which we examined how the relationship between two variables characterizing individuals was affected by different social contexts.[2] Today, although we disagree over the relative strength of contextual and individual variables,[3] we are nevertheless continuing our development of the survey as a tool for studying sociological processes as opposed to social-psychological phenomenon. In this paper we use data on the behavior of teachers during strikes as a research site to study the sociological processes that intervene between an individual's social-psychological predispositions and his actual behavior.

Sociologists have noted that predisposition is not invariably converted into action. For example, men who have characteristics which are ordinarily associated with voting Democratic sometimes vote Republican. In a study of political behavior in the International Typographical Union, Lipset, Trow, and Coleman found that in some circumstances liberal printers supported the more conservative party.[4] Knowing an in-

[1] This research was supported by a grant from the Columbia University sociology department. I would like to thank Jonathan R. Cole for his comments on an earlier draft of this paper. A full report of the data presented here will be published in 1969 by Praeger Press.

[2] An early use of contextural analysis may be found in Emile Durkheim's *Suicide* (Glencoe, Ill.: Free Press, 1951), p. 264, where he shows how the relationship between marital status and suicide depends on the norms concerning divorce. This mode of analysis was independently rediscovered by Paul Lazarsfeld and his colleagues at the Bureau of Applied Social Research.

[3] One area in which this disagreement may be seen is in the research on the effect of high school environment on the decision to go to college (see William Sewell and Vimal P. Shah, "Social Class, Parental Encouragement, and Educational Aspirations," *American Journal of Sociology*, LXXIII [March, 1968], 559–72).

[4] Seymour Martin Lipset, Martin A. Trow, and James S. Coleman, *Union Democracy: The Internal Politics of the International Typographical Union* (Garden City, N.Y.: Anchor Books, 1962), chap. xvi.

dividual's predisposition enables us to predict his behavior in a rough way. If 60 per cent of a certain group of teachers supported a strike, we would make a mistake 40 per cent of the time in predicting the behavior of teachers in this group. In this paper we attempt to develop a more sophisticated explanation of behavior in a decision-making situation by looking in detail at several processes mediating between an individual's predisposition and his behavior. When predisposition is held constant, how do differences in social environment influence behavior? In answering this question we have examined the influence of the presence or absence of social support, cross-pressures, and structurally determined fear of sanctions. Also, in the last part of the paper we have analyzed the significance of reference groups in explaining the behavior of teachers who were pulled in two different directions. Reference group data will enable us to predict the strength of a contextual variable when context and predisposition do not coincide.

Most of the data presented in this paper were obtained from a survey conducted in New York in June, 1962, two months after the United Federation of Teachers had sponsored a highly successful one-day strike on April 11. This strike, in which 22,000 of New York's 40,000 teachers stayed away from school, was perhaps the most crucial event in the subsequent success of the teacher-union movement.[5] The questionnaire was placed in the mailboxes of 900 teachers at three senior high schools, two junior high schools, and seven elementary schools. These schools were located in middle- and lower-class neighborhoods in three of New York's five boroughs. Three hundred and thirty-one questionnaires were returned. Despite the poor response rate the sample statistics were quite close to the known population parameters. Furthermore, we have made no use of marginals but have used only internal correlations.[6] In addition to the survey data some statistics on union membership and the behavior of teachers during the April strike were collected from the records of the Board of Education.[7] Finally, we have presented some data from a survey conducted in Perth Amboy, New Jersey, in January, 1966, two months after the Perth Amboy teachers had been involved in a two-week strike. Questionnaires were mailed to the homes of all 261 teachers in the Perth Amboy school system, and 126, or 48 per cent, were returned. Here too, the sample statistics very closely matched known population parameters.[8]

The first step in the analysis was to determine what characteristics of teachers influenced their predisposition to support or oppose the New York City strike. Since the survey data were all collected after the

[5] For discussions of the impact of the UFT success on the subsequent development of the teacher-union movement, see Myron Lieberman and Michael H. Moskow, *Collective Negotiation for Teachers* (Chicago: Rand McNally & Co., 1966), p. ; and Stephen Cole, "The Unionization of Teachers: A Case Study of Change in a Profession" (unpublished Ph.D. dissertation, Columbia University, 1967), chap. i.

[6] Statistics generated from a non-representative sample cannot be used to make generalizations about population parameters. Thus, on the basis of the sample, we would not be able to say what percentage of the teachers in the population supported the strike or had any particular attitude. However, the fact that a sample over-represents a particular group does not mean that the internal correlations among variables are not valid; e.g., even if our sample over-represents Democrats, this does not mean that the correlation between political affiliation and support of the strike is not a valid one. For a comparison of sample statistics and population statistics and a further discussion of the problems involved in generalizing on the basis of a non-representative sample, see Cole, *op. cit.*, Appendix A.

[7] I thank Professor Alan Rosenthal of Rutgers University for making these data available. Rosenthal reported some of his findings in "The Strength of Teachers Organizations: Factors Influencing Membership in Two Large Cities," *Sociology of Education*, Vol. XXXIX (Fall, 1966).

[8] For an analysis of why the response rate was so low despite a concerted effort to assure anonymity and several follow-up letters and phone calls, see Cole, *op. cit.* All tables specify whether the data are drawn from the New York sample, the Perth Amboy sample, or the aggregate data. All textual analysis refers to the New York survey unless otherwise indicated.

strike was over, there was no way of knowing what a teacher's pre-strike predisposition was. Therefore, we have taken the behavior of teachers on the strike as an indicator of their predisposition; for example, we assume that if a higher percentage of Jewish teachers than Christian teachers supported the strike, then being a Jew made a teacher more predisposed to accept the militant program of the union's leadership than being a Christian.[9]

TABLE 1

PERCENTAGE SUPPORTING THE STRIKE BY FIVE NON-TEACHING STATUSES (NEW YORK SAMPLE)

Status	% Supporting	Teachers (*N*)
Age:		
Under 30	74	78
30–40	59	76
41–50	38	54
Over 50	32	69
Class of origin:		
Middle class	60	94
Working class	72	161
Political affiliation:		
Republicans	38	60
Democrats	72	233
Religion:		
Christians	46	98
Jews	78	157
Sex:		
Women	57	159
Men	81	120

NOTE.—The number of teachers does not add up to 331 because not all teachers provided complete information. Class of origin was measured by the information provided by the teachers on the main occupation of their fathers. These occupations were coded using the census categories and split into two groups. Teachers whose fathers had professional, semi-professional, managerial, clerical, and sales occupations came from "middle-class" families, all others from "lower-class" families. Included with the Democrats were those who said they generally voted for a left-of-center third party. Whether or not teachers supported the strike was determined by answers to the following question: "What did you do on the day of the strike? (*a*) I punched the time clock that day as usual; (*b*) I walked on a picket line; (*c*) I called in sick on that day; (*d*) I just stayed home." Those giving the last three answers were considered to have supported the strike.

Table 1 presents the relationship between five non-teaching characteristics of teachers and support of the April 11 strike. Age, class of origin, political affiliation, religion, and sex are correlated with supporting the strike.[10] But the central point is that not all teachers acted in accord with their predisposition. Some who were predisposed to support the strike crossed the picket line; others who were predisposed to oppose the strike stayed away from school. Here we shall show the conditions under which predispositions created by these non-teaching characteristics were converted into action.

SOCIAL SUPPORT

One variable likely to influence the conversion of a predisposition into action is the amount of social support present for the action. A strike is a divisive event. Some workers put their jobs on the line in a fight which, if successful, will result in benefits for all, even those who actively jeopardize the success of the strike by crossing the picket lines. In this type of situation, behavior is especially influenced by the attitudes and behavior of significant others in the work environment. Sixty-nine per cent of those teachers who said that the majority of their school friends favored the strike supported the strike themselves, whereas only 26 per cent of those who said that less than half of their friends favored the strike did so.[11] We shall use the teachers' reports of their friends' attitudes toward the strike as a crude measure of the amount of social support for the strike that existed in their schools.[12]

[9] This procedure assumes that intensity of predisposition is reflected in action. Lipset *et al.*, *op. cit.*, adopt this same procedure when they use past voting behavior of "liberal" and "conservative" printers as the basis of establishing their predispositions.

[10] I have analyzed the reasons for these correlations elsewhere (see Stephen Cole, "The Unionization of Teachers: Determinants of Rank-and-File Support," *Sociology of Education*, XLI [Winter, 1968], 66–87).

[11] The question was: "What proportion of your friends in school favored the strike? (*a*) none of them, (*b*) about one-quarter, (*c*) about one-half, (*d*) about three-quarters, (*e*) all of them."

[12] Although, for the New York data, there is no direct evidence that the teachers' perceptions of the attitudes of their friends were correct, there is such evidence for the Perth Amboy survey. In Perth Amboy we know exactly how many teachers in each school supported the strike. This information was reported in the local newspaper. The Perth Amboy teachers were also asked what percentage of their

Of all non-teaching statuses, political affiliation was most highly correlated with support of the strike. Seventy-two per cent of Democrats and 38 per cent of Republicans supported the strike (see Table 1). In Table 2 we see how political affiliation influenced behavior under different conditions of social support. If Republicans worked in a school where the majority of their friends did not favor the strike, their inclination to oppose the strike was reinforced and only 14 per cent respected the picket lines. But if Republicans worked in schools where most of their friends supported the strike, then 69 per cent acted against their own predisposition and refused to cross picket lines. The data from Table 2 can be recomputed to show whether or not the teachers acted in accord with their predispositions depending upon the environment. Where social support, either positive or negative (i.e., in favor of or opposed to the strike), was present, 85 per cent acted in accord with their predisposition. When social support was not present, only 37 per cent acted in accord with their predisposition.[18]

TABLE 2

PERCENTAGE SUPPORTING THE STRIKE BY POLITICAL AFFILIATION AND SOCIAL SUPPORT (NEW YORK SAMPLE)

School Friends Favoring the Strike	Percentage Supporting the Strike	
	Republicans	Democrats
50% or less	14 (30)	38 (60)
More than 50%	69 (29)	85 (169)

NOTE.—Figures in parentheses represent the numbers of teachers in each category.

As a summary measure of predisposition, we constructed an Index of Militant Predisposition (IMP). A teacher's score on this index was simply the number of militant characteristics shown in Table 1 that he possessed. Thus, a Jewish, Democratic male, under the age of forty, from a lower-class family, received an IMP score of five. As may be seen in Table 3, the IMP was highly correlated with support of the strike. The importance of social support in converting predisposition into action is also clearly illustrated in Table 3. Without social support, even those teachers who were highly predisposed to be militant (i.e., had three, four, or five militant characteristics) found it difficult to support the strike. Only 38 per cent of those teachers with high IMP scores but little social support acted in accord with their predisposition. Likewise, those teachers who were predisposed to oppose the strike found it difficult to do so if they worked in a school where the majority

TABLE 3

PERCENTAGE SUPPORTING THE STRIKE BY INDEX OF MILITANT PREDISPOSITION AND SOCIAL SUPPORT (NEW YORK SAMPLE)

IMP	Percentage Supporting Strike	School Friends Favoring the Strike	
		50% or Less	More than 50%
Low (0, 1, 2)	43 (97)	24 (51)	65 (46)
High (4, 5, 6)	81 (179)	38 (37)	93 (142)

friends supported the strike. If we compare the actual number of teachers supporting the strike and the subjective view of the teachers of how many of their friends supported the strike, we find that only 14 per cent of the teachers gave an estimate that was off by more than 20 per cent. We do not imply that the 14 per cent whose report varied from the objective condition had poor cognitive perception or were lying. The teachers were asked what percentage of "friends" favored the strike. It was possible for the teacher answering the questionnaire to give a broad or a narrow definition of this term. However, in general, we may conclude that the answer to this question is a fairly reliable measure of the actual amount of social support that the teacher received either in the school as a whole or among his special friends in the school. This latter definition of the term "friends" would be more likely to be given by teachers in large schools, i.e., secondary schools.

[18] The figure 85 per cent was arrived at by combining the data in cells 1 and 4 of Table 2. The 37 per cent represents the combination of data in cells 2 and 3.

of their colleagues supported the strike. Sixty-five per cent of these teachers supported the strike. When social support was present, 89 per cent acted in accord with their predisposition. When social support was not present, only 36 per cent acted in accord with their predisposition.[14]

This analysis underscores once again how contextual variables influence the interaction of variables characterizing individuals. Knowing a teacher's IMP score tells us a lot about how he behaved during the strike. But when we know the attitudinal context in which he worked, we can interpret behavior that deviates from the prediction based upon the IMP. These findings confirm the analysis of Lipset, Trow, and Coleman of the effect of attitudinal contexts on whether political predispositions are acted on.[15]

The aggregate data collected from the records of the New York City Board of Education provide additional evidence on how social support influenced the behavior of teachers. We know that there was a higher probability that men would support the strike than women. Therefore, we would predict that women would be more likely to strike in schools where there were many men present. For in schools where there was widespread social support for striking, even teachers with relatively low scores on the IMP would be more likely to support the strike.

For the junior high schools in our aggregate sample, we had information on the number of men in each school and the proportion of each sex that supported the strike. We divided the schools into those in which 40 per cent or more of the staff were men and those in which less than 40 per cent of the staff were men. We then computed the percentage of women supporting the strike in each of these groups. In those schools which had less than 40 per cent male teachers, 51 per cent of the women supported the stiike. In those schools that had a relatively high proportion of male teachers, 62 per cent of the women struck.

These aggregate data provide further corroborative evidence that the social support of men was significant in the activation of militancy among female teachers. The majority of teachers in the girls' high schools are women, and the majority of teachers in the boys' high schools are men. The faculties of the co-ed high schools are about evenly split between men and women.[16] Forty-five per cent of the teachers in all girls' high schools struck, as opposed to 68 per cent of the teachers in all boys' schools and 69 per cent of those in the co-ed schools.[17] Since we can assume that men who taught in co-ed schools were no more likely to support the strike than men who taught in boys' schools, and since the percentage striking in these two types of schools was the same, the data indicate that in the co-ed schools the female teachers supported the strike just as strongly as did their male colleagues. Thus, the difference in militancy of the female teachers in girls' schools and in co-ed schools may possibly be explained by the presence of the militant men in the latter schools. Where men were present to take the lead and give social support, the predisposition of women to oppose the strike was overcome, where men were not present, the predisposition of women to oppose the strike was converted into action, and the women were likely to cross the picket lines and report for work.

Activation of militancy depended upon the sheer number of colleagues who supported the strike and also the presence of effective leadership in the schools. In fact, the presence of effective leadership was likely to be a factor influencing the amount of social support. The quality of union leadership varied from school to school, with

[14] The figure 89 per cent was arrived at by combining the data in cells 1 and 4 of Table 3. The 36 per cent represents the combination of data in cells 2 and 3.

[15] Lipset *et al.*, *op. cit.*

[16] This information was obtained from interviews with several union leaders.

[17] Of course it is possible that there may be other differences between female teachers in girls' schools and in co-ed schools that would explain these data.

some schools having strong chapter chairmen who enlisted a large number of UFT members, and others having weak and ineffective chapter chairmen. There were some schools in which there was no UFT chapter, nor even a single UFT member. For Table 4 we have classified the schools in all four divisions by the percentage of teachers in each school who were UFT members in January, 1962, three months before the strike of April 11.[18] The percentage of UFT members in a school is taken as an indicator of both the quality of union leadership and the number of teachers who were likely to be active supporters of the strike. Table 4 provides the obvious result that there was more support for the strike in those schools where union membership was large than in those where it was small or non-existent. But what is rather less evident is the further finding in Table 4 that the presence of strong leadership made the most difference in that division where support of the strike was weakest, that is, in the elementary schools. In the divisions where support for the strike was strong, the addition of effective leadership as a source of social support had less effect. We might say that effective chapter leadership had a greater impact in preventing the negatively disposed from crossing picket lines than it did in reinforcing the inclination of those predisposed to support the strike.

We thought it possible that the number of union members in a school might depend upon the conditions within the school. It was possible that those schools in which working conditions were worst would have the most union members and that the data in Table 4 would be a result of differential degrees of job dissatisfaction rather than differences in the quality of union leadership. To test this hypothesis we looked at the percentage of teachers belonging to the union in "special-service" schools and in regular schools. The special-service schools are all located in low-income neighborhoods and generally have more difficult working conditions and thus more dissatisfaction. We found that 19 per cent of teachers in both special-service schools and regular schools taught in schools where more than 10 per cent of the teachers were UFT members. Since union membership prior to the strike was not correlated with difficulty of working conditions, we may conclude that social support provided by effective leadership had an independent effect on the conversion of predisposition into action.

When teachers had to decide what to do about the strike, they were influenced by a set of attitudes they had acquired before becoming teachers. These attitudes or social-psychological characteristics had different consequences depending upon the teacher's social environment. Those teachers who found their predispositions supported by their colleagues' attitudes and behavior tended to act in accord with their predisposition. Those whose predisposition con-

TABLE 4

PERCENTAGE SUPPORTING THE STRIKE BY TYPE OF SCHOOL AND UNION MEMBERSHIP (AGGREGATE DATA)

Teachers Who Were Union Members Prior to the Strike (%)	Teachers Supporting the Strike			
	Elementary	Junior High	Academic High	Vocational High
0–9	30 (3,929)	65 (1,063)	55 (2,776)	60 (1,656)
10–29	45 (716)	69 (1,960)	73 (3,901)	62 (519)
30 or more	56 (204)	76 (2,021)	76 (1,075)	64 (98)

[18] Here we are using the aggregate data provided by Alan Rosenthal, *op. cit.*

flicted with their environment were less likely to act in accord with these predispositions. These latter teachers were under cross-pressure. In the next section we analyze the sources and consequences of cross-pressure.

CROSS-PRESSURES AND AVOIDANCE BEHAVIOR

The strike situation was analogous to a voting situation in that there were three alternative courses of action. In an election you may vote for one candidate or another, *or* you may choose not to vote, to avoid making a clear decision. In a strike a teacher could walk on the picket line or cross the picket line and report for work, *or* he could call in sick and thus avoid a firm commitment. Here, we shall show how teachers who experienced cross-pressures were the most likely to engage in avoidance behavior.[19]

TABLE 5

PERCENTAGE CALLING IN SICK BY IMP SCORE AND ATTITUDES OF FRIENDS TOWARD THE STRIKE (NEW YORK SAMPLE)

School Friends Favoring the Strike	Percentage Calling in Sick	
	Low IMP (0, 1, 2)	High IMP (3, 4, 5)
50% or less.........	10 (51)	16 (37)
More than 50%.....	41 (46)	22 (142)

We found four types of cross-pressure that led to avoidance behavior:

Type 1: conflict between a social context and a predisposition created by a status.—Cross-pressure occurred when a teacher occupied a status predisposing him to act in one way, while most of his colleagues were taking a contrary position. When a teacher occupying a status (or statuses) predisposing him to support the strike worked in a school where the majority of his friends did not support the strike, he was under cross-pressure. Correlatively, a teacher whose status-set made him predisposed not to support the strike, and who had a majority of friends supporting the strike, was also under cross-pressure (see Table 5). Of those teachers who said that more than half of their friends supported the strike, 41 per cent of those with low IMP scores, as opposed to 22 per cent of those with high IMP scores, called in sick. We found a similar but weaker pattern among teachers who said that 50 per cent or fewer of their friends supported the strike. If we recompute the data from Table 5, combining the data in cells 1 and 4 with the data in cells 2 and 3, we find that, when the context and the predisposition coincided, 19 per cent engaged in avoidance behavior; when the context and the predisposition conflicted, 30 per cent engaged in avoidance behavior.

Type 2: conflict between objective statuses and subjective attitudes.—An individual occupies statuses which ordinarily create predisposition to act in one way. His subjective evaluation of the situation may support or oppose this predisposition. The most common example of such conflict is the blue-collar worker who identifies with the middle class. When objective statuses and subjective evaluations do not coincide, the individual is likely to be under cross-pressure. Teachers who measured low on the IMP and thought the strike justified, as well as those who measured high on IMP and thought the strike unjustified, were under cross-pressure and were more likely to engage in avoidance behavior than their non-cross-pressured colleagues. Thirty-two per cent of teachers under this type of cross-pressure called in sick, as opposed to 18 per cent of those not under this type of cross-pressure.

It was possible that those teachers who who had non-teaching statuses predisposing them to support the strike but who

[19] The concept of cross-pressures was introduced by Paul F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet in *The People's Choice* (New York: Columbia University Press, 1948). It was extended further in Berelson, Lazarsfeld, and W. N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954). For a summary of other relevant literature, see Seymour Martin Lipset, *Political Man* (Garden City, N.Y.: Anchor Books, 1960), pp. 211–26.

viewed the strike as unjustified did not have those typical attitudes associated with their non-teaching statuses. However, if this were true, they should have behaved like their colleagues who had low IMP scores. Since they did not, we can assume that they had conflicting attitudes. This type of cross-pressure would seem to be psychological, that is, internal to the individual. However, we might ask under what conditions individuals will form opinions which conflict with their predisposition. We thought that the opinion of one's friends would be a crucial factor in the genesis of this type of cross-pressure. Indeed, we found that, where one's predisposition and the attitudes of friends were in agreement, only 14 per cent were under this type of cross-pressure. But when the individual's predisposition conflicted with the attitude of his friends, 62 per cent were under this type of cross-pressure. Thus, internal psychological conflict can be seen partially as a result of social location.

Type 3: polarization in a social context.—In a situation where polarization on an issue occurs, such as in a strike or an election, an individual will be under cross-pressure if some of his friends and associates are on one side and others on the opposite side. The more evenly split are one's associates, the greater the cross-pressure experienced. The Perth Amboy data provide a good site to test this hypothesis. Perth Amboy teachers were asked when they decided what they would do on the day of the strike. Making up one's mind right before the strike is a type of avoidance behavior. It turned out that the more agreement there was among friends, the more likely a teacher was to make up his mind early. If friends were split, a decision was more likely to be delayed. Sixty-eight per cent of those who reported disagreement among friends, as opposed to 81 per cent of those who reported unanimity, made up their minds a few days before the Perth Amboy strike.

Type 4: conflict between two or more statuses.—The first three types of cross-pressure were a result of conflict between an individual's predisposition and his school environment or a result of conflict within the school environment itself. Cross-pressure could also have resulted from conflict outside the school. A teacher could have occupied one or more statuses which predisposed him to favor the strike and one or more statuses which, at the same time, predisposed him to disapprove of it. As a result of having such a status-set, an individual would associate with groups of people who had conflicting expectations of how he should behave. The IMP may be used as an indicator of the degree of cross-pressure resulting from the occupation of non-teaching statuses which make for conflicting predispositions. Those teachers who scored 0 or 5 on the Index were under no cross-pressure. All their statuses combined to make them either highly predisposed to favor the strike or highly predisposed to oppose the strike. Those teachers who scored 1 or 4 on the IMP had one status which put them under cross-pressure, whereas those teachers who scored 2 or 3 on the Index were under the most cross-pressure. These teachers had two statuses pulling them in one way and three statuses pushing them in the opposite direction. As may be seen in Table 6, those teachers who were under the greatest cross-pressure were the ones most likely to engage in avoidance behavior. In part A of Table 6 we show the percentage of teachers in each category of the IMP who called in sick, and in part B we see how the number of cross-pressures influenced this type of behavior.[20] Only 8 per cent of the teachers with no cross-pressure-producing status, and 29 per cent of the teachers with two cross-pressure-producing statuses, called in sick.

FEAR OF SANCTIONS

Fear of sanctions influenced the decision of teachers about whether to support the strike. Although teachers had struck in the past, such events were rare and on a smaller scale than the New York City strike of April

[20] For a similar table, see Lazarsfeld *et al.*, *op. cit.*, p. 63.

11, 1962. The striking teachers had to violate the highly punitive Condon-Wadlin Act, which called for dismissal and loss of pay, tenure, and salary increments, among other frightening prospects. It would be incorrect, however, to assume that frightened teachers would be less likely to strike. A teacher who himself did not favor the strike and who did not feel pressured by his colleagues to support the strike had little to be afraid of. Having definitely made up his mind not to strike, he would have little reason to fear the penalties of striking. Thus, the teachers least likely to be militant were also least likely to be fearful.[21] Eighty per cent of those teachers who expressed fear thought the strike was justified.[22]

Table 7 presents the relationship between fear and action among those teachers who believed the strike justified.[23] Teachers who were afraid were more likely to punch the time clock, less likely to picket, and more likely to escape from the anguish of a direct commitment by calling in sick. Calling in sick was clearly less dangerous than picketing. If the strike failed and the Condon-Wadlin Act was invoked, a friendly physician could be found to legitimate the claim to illness. Also, by calling in sick the teacher did not lose a day's pay.

The effect of fear on the conversion of predisposition into action may be seen in Table 8. Of those teachers who scored high on the IMP, the ones who expressed fear were more likely to cross the picket line and more likely to call in sick. Fear had only minor influence on the teachers who were

TABLE 6

A. PERCENTAGE CALLING IN SICK BY IMP SCORE (NEW YORK SAMPLE)

Index of Militant Predisposition					
0	1	2	3	4	5
0 (11)	24 (34)	31 (58)	28 (78)	20 (80)	10 (39)

B. PERCENTAGE CALLING IN SICK BY NUMBER OF CROSS-PRESSURE-PRODUCING STATUSES (NEW YORK SAMPLE)

0 (IMP 0, 5)	1 (IMP 1, 4)	2 (IMP 2, 3)
8 (50)	21 (114)	29 (136)

[21] Lazarsfeld and Wagner Thielens, Jr., found similar results in their study of the reaction of college teachers to McCarthyism. Those teachers who were the most conservative were the least apprehensive (*The Academic Mind* [Glencoe, Ill.: Free Press, 1958], p. 239).

[22] The teachers who were afraid and did not think the strike justified were probably being subjected to social pressure to support the strike. However, fear had very little influence on the behavior of those teachers who did not think that the strike was justified. The great majority of these teachers, whether or not they were afraid, crossed the picket line and reported for work. Teachers who did not believe the strike justified have been excluded from most of the analysis of fear of sanctions.

[23] A teacher was classified as being afraid if he feared at least one of the following: loss of job, not being able to get a job in another school system, loss of pension benefits, that the principal would make things difficult, getting a black mark on his record, or loss of tenure. Only 19 per cent of our sample checked at least one of these items. Since these were not forced-choice questions, we believe

not predisposed to favor the union movement.

Fear was not randomly distributed throughout the school system but was created by both social-psychological and contextual variables. Some individuals had attributes which made them more frightened than their colleagues. We thought that both the oldest and the youngest teachers had most to lose if the strike failed and would, therefore, be more afraid to support the strike even if they believed it to be justified. Indeed, age was curvilinearly related to fear among those who believed the strike to be justified. Teachers under thirty and over fifty were more likely to say they were afraid than those between thirty and fifty.[24] Many of the younger teachers were either "substitutes" or regulars on probation without tenure. These teachers were afraid that participation in the strike would prevent them from getting tenure. The older teachers, in addition to fearing loss of their pension, knew that if they were fired it would be difficult for them to find new jobs. Most school systems prefer to hire young teachers whom they have to pay less and who, if given tenure, will give many years of service.

Fear was influenced by individual characteristics of the teachers and by the context in which the individual worked. In some schools there was more reason to be afraid than in others. The attitudes toward the strike of supervisory personnel having direct authority over the teacher differed from school to school. If supervisors gave indications that they would punish teachers participating in the strike, there was more reason for a teacher to be afraid. If supervisors either ignored the strike, or, as happened in some cases, indicated that they were in sympathy with the strikers, teachers were less likely to be afraid.

The New York survey did not include a question on the teacher's perception of his supervisor's attitudes toward the strike, but in Perth Amboy we asked the teachers such a question.[25] Forty-three per cent of those teachers who thought that their principals were opposed to the strike, and 31 per cent who thought that their principals favored the strike or were neutral, were

that this technique resulted in a substantial understatement of fear. In Perth Amboy, where the same questions were used in a forced-choice format, 38 per cent of the sample checked at least one item.

[24] Among those teachers who believed the strike to be justified, 25 per cent of those under thirty, 17 per cent of those from thirty to fifty, and 31 per cent of those over fifty were afraid.

[25] The exact wording of the question was: "Do you think that the attitude of your principal was: (a) favorable to the strike, (b) unfavorable to the strike, (c) neutral, (d) I don't know what the attitude of my principal was?"

TABLE 7

FEAR AND ACTION ON APRIL 11—ONLY THOSE BELIEVING THE STRIKE JUSTIFIED (NEW YORK SAMPLE)

Fear	Action on April 11			
	Punched Time Clock (%)	Picketed (%)	Called in Sick (%)	Teachers (N)
Not afraid	8	67	25	152
Afraid	21	46	33	48

TABLE 8

INDEX OF MILITANT PREDISPOSITION, FEAR, AND ACTION ON APRIL 11 (NEW YORK SAMPLE)

Fear	Action on April 11			
	Punched Time Clock (%)	Picketed (%)	Called in Sick (%)	Teachers (N)
Low IMP (0, 1, 2):				
Not afraid	59	19	22	80
Afraid	55	15	30	20
High IMP (3, 4, 5):				
Not afraid	17	64	19	139
Afraid	28	47	26	43

afraid. The perceived attitude of the principal was also related to action on the first day of the strike. Eighty-three per cent who said their principal favored the strike supported it themselves, as opposed to 33 per cent of those who said their principal opposed the strike. Although it is possible that there might be other variables explaining these data, they suggest that the visible attitudes of supervisors were probably significant in influencing the conversion of predisposition into action.

In New York another contextual variable influencing fear was type of school. In the

TABLE 9

INDEX OF MILITANT PREDISPOSITION, TYPE OF SCHOOL, AND ACTION ON APRIL 11 (NEW YORK SAMPLE)

School Type	Action on April 11			
	Punched Time Clock (%)	Picketed (%)	Called in Sick (%)	Teachers (*N*)
Low IMP (0, 1, 2):				
Elementary...	71	8	21	24
Secondary	55	18	26	65
High IMP (3, 4, 5):				
Elementary...	41	39	20	54
Secondary	16	63	21	135

elementary schools 30 per cent of the teachers were fearful, and in the secondary schools, 20 per cent. This finding does not result from differences in the sex composition of the faculties of the two types of schools. Men, in fact, were slightly more afraid than women.[26] Also, if the difference in fear between the two types of schools was dependent upon the sex distribution of the teachers, we would expect female secondary-school teachers to be just as frightened as their elementary-school counterparts. This was not the case. Thirty-one per cent of female elementary-school teachers and 15 per cent of female secondary-school teachers were afraid. We may conclude that the social organization of the elementary schools was a factor contributing to the fear experienced by teachers who taught there.

In the elementary schools there is a more direct relationship between school authorities and the teacher. The elementary schools are relatively small, and contact between the principal and the teachers is frequent, making it difficult to remain anonymous. Therefore, the threat of negative sanctions was closer and more menacing in the elementary schools than in the junior and senior high schools. In the high schools the teacher's immediate supervisor was his departmental chairman, who may also have been a member of the union and, in some cases, may have actively supported the strike. It is probable that because of the greater amount of fear that existed in the elementary schools, even those teachers who were highly predisposed to support the strike found it difficult to do so (see Table 9). Of those teachers who scored high on the IMP, 41 per cent of the elementary-school teachers and 16 per cent of the secondary-school teachers crossed the picket line.

Those teachers who were highly predisposed to be militant and yet were afraid of negative sanctions were under a type of cross-pressure. We wanted to specify the conditions under which an individual would conquer fear and act in accord with his militant predisposition. Perhaps the most crucial variable in determining whether fear would prevent the favorably predisposed from striking was social support. Fear could be conquered if the teacher was one of many putting his job on the line. Psychologically, the perceived danger of striking was inversely related to the number of teachers who supported the strike in one's *own* school.

The importance of social support for those who were afraid may be seen in Table 10. If teachers who were afraid did not perceive their friends as favoring the strike, 74

[26] Twenty-six per cent of the men and 21 per cent of the women were afraid. Men are the primary breadwinners for their families and had more to lose than women if the strike failed.

per cent crossed the picket line. If they did perceive their friends as favoring the strike, only 13 per cent crossed the picket line.

Social support was most lacking in the elementary schools. Whereas 74 per cent of the secondary-school teachers said that three-quarters or more of their school friends favored the strike, only 52 per cent of the elementary-school teachers reported as much support. If fear was more prevalent in elementary schools because their small size made it more difficult to remain anonymous and less likely that there would be social support for striking, we would expect that the larger the elementary school, the more nearly conditions would approach those found in high schools. To study the effect of size of elementary school we must return to the aggregate data. Twenty-two per cent of teachers in schools having staffs of fewer than thirty supported the strike, as opposed to 31 per cent in schools with staffs of thirty to thirty-nine, and 39 per cent in schools with staffs of forty or more. In large elementary schools the teacher who was highly predisposed to support the strike had a better chance of finding colleagues who felt the same way. In the small elementary schools there was a high probability that the militant teacher would find herself isolated.[27] In large elementary schools it was also more likely that the relationship between teacher and supervisor would be impersonal. It was easier to call in sick and go unnoticed. Large size created anonymity and provided social support and thus reduced the effect of fear.

COLLEAGUES AS A REFERENCE GROUP

Throughout this paper we have stressed the significance of interaction in converting predisposition into action. The way in which such interaction influenced behavior depended in part on whether colleagues were a significant reference group. The teachers were asked whether they were afraid that they would lose the respect of their colleagues if they did not support the strike.[28] We assume that colleagues were a more important reference group for those who feared losing their respect than for those who did not fear such a loss.[29]

In the first section of this paper we used the percentage of friends supporting the strike as an indicator of the amount of social support for striking. It is clear, however, that what was social support to some was social pressure to others. Those teachers who

TABLE 10

PERCENTAGE OF FRIENDS FAVORING THE STRIKE AND ACTION ON APRIL 11—ONLY THOSE WHO WERE AFRAID (NEW YORK SAMPLE)

Friends Favoring the Strike	Action on April 11			
	Punched Time Clock (%)	Picketed (%)	Called in Sick (%)	Teachers (*N*)
50% or less	74	17	9	23
More than 50%.....	13	49	38	39

[27] Lipset *et al.*, *op. cit.*, pp. 170–94, came to similar conclusions in their analysis of the influence of shop size.

[28] Teachers were asked to place a check next to the following statement if it applied to them: "I thought that if I *did* go to work I would lose the respect of some of my colleagues."

[29] We thought it possible that fear of loss of respect might only be a valid indicator of whether colleagues were a significant reference group for those teachers whose attitudes toward the strike differed from the attitudes of their colleagues. This was not the case. If the majority of friends favored the strike, 44 per cent of those believing the strike justified and 36 per cent of those believing the strike was not justified feared loss of respect. The feeling of those not believing the strike justified needs little explanation. Their friends felt one way and they, another. The majority of their friends expected them to support the strike. If they did not live up to this expectation, they would lose the respect of their friends. But what about those who did think the strike justified, those who agreed with their friends? It is most likely that every teacher, no matter how militant, was to some extent worried about the strike. It was possible for a teacher to favor the strike, perceive his friends as favoring the strike, and still be afraid of negative sanctions from authorities. In such a situation fear of loss of respect might very well have been salient.

had low scores on the IMP and who taught in schools where more than half their friends favored the strike were under considerable pressure to act contrary to their predisposition. What determined whether they yielded to this pressure? Here we are extending our analysis of how decisions are made when men are under cross-pressure. Those teachers who had low IMP scores and worked in schools where the majority of their friends favored the strike were under cross-pressure.

TABLE 11

FEAR OF LOSING RESPECT OF COLLEAGUES AND ACTION ON APRIL 11—FOR THOSE LOW ON IMP AND HAVING MORE THAN 50 PER CENT OF THEIR FRIENDS FAVOR THE STRIKE (NEW YORK SAMPLE)

Fear	Action on April 11			
	Punched Time Clock (%)	Picketed (%)	Called in Sick (%)	Teachers (N)
Feared loss of respect.....	17	28	56	18
Did not fear loss of respect.....	45	21	34	29

Above we showed how these teachers were likely to avoid a decision by calling in sick. But what of those who could not avoid a decision—what influenced the *direction* of their decision? We hypothesized that those teachers who had colleagues as a reference group would be the most likely to yield to pressure.

The data indicate that social pressure was difficult to ignore even for those teachers who did not fear losing the respect of their colleagues. Only 45 per cent acted in accord with their predisposition and crossed the picket line (see Table 11). However, when social pressure was exerted by colleagues who were a significant reference group, practically all teachers acted contrary to their predispositions. Only 17 per cent of teachers who had low IMP scores and reported that more than 50 per cent of their colleagues supported the strike, and who feared losing the respect of these colleagues, acted in accord with their predisposition.[30] Knowing the salience of man's relations with those around him helps us understand the effect that attitudinal context has on the conversion of the individual's predisposition into action.

Whether or not colleagues were a reference group also determined whether teachers who had high IMP scores but feared negative sanctions would act in accord with their predisposition. Although the percentages are based upon a very small number of cases, the data in Table 12 suggest that, if colleagues were a reference group, frightened teachers were able to overcome their fear and act in accord with their predisposition. Above (Table 10) we showed that social support enabled the frightened teacher to overcome his fear. The data in Table 12 suggest that the support of colleagues will be a more effective bolster if

TABLE 12

FEAR OF LOSING RESPECT OF COLLEAGUES AND ACTION ON APRIL 11—FOR THOSE HIGH ON IMP AND FEARING SANCTIONS FROM SUPERVISORS (NEW YORK SAMPLE)

Fear	Action on April 11			
	Punched Time Clock (%)	Picketed (%)	Called in Sick (%)	Teachers (N)
Feared loss of respect.....	23	42	35	31
Did not fear loss of respect	42	58	0	12

[30] Just as fear of sanctions induced avoidance behavior, so did fear of loss of respect. Thirty-two per cent of all those fearing loss of respect and 16 per cent of those not fearing loss of respect called in sick.

these colleagues are a meaningful reference group.[31]

Fear of loss of respect, like fear of sanctions, served to specify the conditions under which predisposition will be converted into action. Of those teachers who scored low on the IMP, 22 per cent fearing loss of respect and 69 per cent not fearing such a loss acted in accord with their predisposition. However, fear of loss of respect had little influence on those who scored high on the IMP. The data in Table 13 present an informative contrast to those of Table 8. Whereas fear of sanctions had little influence on those not predisposed to be militant, fear of loss of respect had little influence on those who were predisposed to be militant. Since the general climate in the schools was one of support for the strike, fear of sanctions from colleagues was more likely to influence those who were predisposed not to support the strike. Fear of sanctions from supervisors was more likely to influence those who were predisposed to support the strike.

Let us summarize our findings on the conditions which influenced the conversion of predisposition into action:

1. Those teachers who had a majority of their friends agreeing with them were more likely to convert predisposition into action than those who had a majority of friends disagreeing with them. Another type of social support was provided by union leadership. Where union leadership was active, the behavior of those predisposed to favor the strike was reinforced. Where local union leadership was absent or ineffective, those with favorable predispositions toward the strike were less likely to convert them into action.

2. When the strike was called, a teacher could cross the picket line and report for work, walk on the picket line, or call in sick. This latter alternative was a type of avoidance behavior. By calling in sick, the teacher was able to avoid making a clear commitment either in support of the strike or in opposition to it. We show how four types of cross-pressure influence avoidance behavior. A teacher was under cross-pressure when (*a*) there was a conflict between a predisposition created by a status and that created by a social context, (*b*) there was a conflict between a status predisposing him to act one way and his subjective attitude toward the strike, (*c*) he had some friends who favored the strike and other friends who opposed the strike, or (*d*) he occupied two statuses which predisposed him to act in different ways. Each of these types of cross-pressure influenced the conversion of predisposition into action.

TABLE 13

INDEX OF MILITANT PREDISPOSITION, FEAR OF LOSS OF RESPECT, ACTION ON APRIL 11 (NEW YORK SAMPLE)

Fear	Action on April 11			
	Punched Time Clock (%)	Picketed (%)	Called in Sick (%)	Teachers (N)
Low IMP (0, 1, 2):				
Feared loss of respect	22	30	48	23
Did not fear loss of respect	69	14	17	77
High IMP (3, 4, 5):				
Feared loss of respect	14	58	28	76
Did not fear loss of respect	23	61	16	106

3. Those teachers who were afraid of sanctions from authorities were less likely to convert a favorable predisposition into action. Fear was an emotion which was not randomly distributed. Fear was related to age, the authority structure in the different types of schools, and the amount of social support. The presence of other individuals who openly supported the strike was the

[31] Ideally we would run a five-variable table to demonstrate the validity of our assumption. However, this is impossible given the size of our sample.

most important ingredient in overcoming fear.

4. If colleagues were a significant reference group, those teachers who were not predisposed to be militant but worked in schools where there was a heavy support for the strike were likely to give in to pressure and act contrary to their predispositions. Likewise, those teachers who were predisposed to be militant but feared punishment were most likely to overcome their fear if colleagues were a significant reference group. Those teachers who were favorably disposed toward the strike were more affected by fear of sanctions from authorities, whereas those teachers who were predisposed to oppose the strike were more affected by fear of sanctions from colleagues.

Perhaps the most significant finding of this paper is the one concerning the importance of reference groups as a factor mediating the influence of social context on the individual. It would seem that all studies making use of "contextual analysis" would benefit by including data on the reference groups of the individuals studied.[32] Knowing the salience of relationships with the people making up the social context will aid us in predicting the strength of the contextual variable.

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[32] The analysis in Lipset *et al.*, *op. cit.*, chap. xvi, might have been further perfected by including data on the extent to which the other printers in the shop were a reference group. Also, the analysis presented by James S. Coleman in "The Adolescent Sub-Culture and Academic Achievement," *American Journal of Sociology*, LXV (January, 1960), 337–47, would have been more convincing had he broken down his student sample into those who had the "leading crowd" as a reference group and those who did not.

Marital Status and Suicide in the United States: A Special Test of the Status Integration Theory

Jack P. Gibbs

ABSTRACT

Data on suicide by age, sex, and marital status in the U.S., 1959–61, are employed to test the theory of status integration. On the whole, the findings are consistent with the theory, but more so for males than females. Such differential results are analyzed in terms of four principles that stipulate "ideal" test conditions. These principles are not *ad hoc* explanations of negative findings, meaning that they can be generalized to all tests of the theory. Data are available for systematic application of three of the four principles, and that opportunity should be realized in future research.

Two problems are encountered in assessing the predictive power of sociological theories. First, all too often the way a theory is stated precludes systematic tests. Second, even when a theory is testable, the requisite data may not be available. The second problem creates anomalous situations in sociology. Rarely is confidence in sociological data such that one test would incontrovertibly falsify a theory, which means that there can be no definitive test and, therefore, no particular body of data is crucial. However, if requisite data are not readily available, any opportunity to test a theory deserves what otherwise would be inordinate attention.

Opportunity for tests is an especially important consideration in further assessment of the theory of status integration and suicide,[1] which applies to virtually any kind of population—territorial (e.g., metropolitan areas, countries), age groups, marital statuses, occupations, sex, race, and parental status. Even though research on the theory relies on census and vital statistics, a variety of tests have been reported, and the series can be extended indefinitely. However, mortality statistics are such that one of the three types of tests—the "within-columns" test—can be conducted only rarely.

Given that situation, a recent publication by the U.S. National Center for Health Statistics, *Suicide in the United States, 1950–1964*,[2] is a crucial item. It reports suicide rates, 1959–61, by marital status, age, sex, and color. Data on the incidence of suicide in the United States by age, sex, and race (or color) are reported annually, but corresponding tabulations by marital status are rare. Incidence figures on marital status by age, sex, and color are noteworthy in that they can be used for within-columns tests of the theory. Therefore, the publication is of special importance even though restricted to one country.

BRIEF STATEMENT OF THE THEORY AND EXPLICATION OF MEASURES

Taking Durkheim's classic study as the point of departure, a generalization is stated as postulate I: The suicide rate of a population varies inversely with the stability and durability of social relationships within that population. This generalization cannot be tested systematically because of problems in measuring the stability and durability of social relations, the availability of data in particular. However, it is possible to designate a condition that would maximize

[1] Jack P. Gibbs and Walter T. Martin, *Status Integration and Suicide: A Sociological Study* (Eugene: University of Oregon Books, 1964).

[2] U.S. Government Printing Office, August, 1967.

stability and durability, as in postulate II: The stability and durability of social relationships within a population vary directly with the extent to which individuals in that population conform to the patterned and socially sanctioned demands and expectations placed upon them by others. For various reasons, the second postulate cannot be tested, but it does suggest an important consideration. Differences among populations with regard to conformity probably would be minute were it not for variation in the amount of role conflict, meaning inconsistent socially sanctioned demands or expectations, such that conformity to one entails deviation from the other. That observation is stated formally as postulate III: The extent to which individuals in a population conform to patterned and socially sanctioned demands and expectations placed upon them by others varies inversely with the extent to which individuals are confronted with role conflicts. Now it is admitted readily that there is no way to determine how much role conflict prevails in a population, especially since it probably is a matter of degree *for all individuals*. However, given the conceptual link between role and status, the structural correlate of the amount of role conflict in a population is the proportion of the members who occupy incompatible statuses, meaning two or more statuses with conflicting roles. Accordingly, we have postulate IV: The extent to which individuals in a population are confronted with role conflicts varies directly with the extent to which individuals occupy incompatible statuses in that population.

At this point a fundamental assumption is introduced. Given two or more statuses with conflicting roles, that configuration tends to be infrequently occupied. In some cases occupancy may be normatively proscribed, as when airlines will not hire women as pilots. Alternatively, even without formal proscriptions, potential occupants of the status configuration may recognize the role conflict and refrain from occupancy (as when a married man declines to become a seaman). Finally, if the occupants of incompatible statuses do not change one or more statuses voluntarily (as a response to conflicting demands), the change may be forced, as when a seaman is fired or divorced because of failure to conform to some of his roles.

If incompatible statuses are the primary source of role conflict and if incompatible statuses are occupied infrequently, then the amount of role conflict is reflected in the degree of status integration, a strictly structural and statistical concept. Statuses are integrated to the maximum degree in a population if knowledge of all but one of *any* member's statuses makes it possible to predict the remaining status correctly. For example, if knowledge of any individual's age, sex, race, occupational, ethnic, parental, and religious statuses permits a correct prediction of marital status, then maximum *marital* integration prevails in that population. Accordingly, with maximum status integration, there are no infrequently occupied status configurations. Thus, we arrive at postulate V: The extent to which individuals occupy incompatible statuses in a population varies inversely with the degree of status integration in that population.

For various reasons, especially the availability of data, none of the above generalizations is testable directly. However, since the suicide rate and the degree of status integration can be measured, the five postulates do generate a *testable* theorem. Using the sign rule and assuming that all relations in the postulates are close and/or no correlation exists among the error terms, the derived theorem is: The suicide rate of a population varies inversely with the degree of status integration in that population.

Measurement of status integration.—Understanding of status integration is furthered by considering its measurement, and to that end Table 1 provides an illustration.

Since the theory presupposes movement among some statuses, ascribed statuses never enter into the rows of an integration table. In this instance only marital statuses are in the rows, with combinations of all other

statuses in the columns (but excluding unoccupied configurations). As such, the measures refer to marital integration, and additional tables would be needed for other dimensions of status integration (e.g., occupational).

The elementary integration measure is the proportion of a column total in a cell. Accordingly, with reference to the first column of Table 1, the widowed should have the highest suicide rate and the single the lowest rate. Such expectations lead to a within-columns test of the theorem.

The ΣX^2 values in the last row of Table 1 enter into a "between-columns" test of the theorem. According to the theorem, the population in the last column of Table 1 should have the highest suicide rate, with the lowest rate in the second column.

Finally, the sum of column values (i.e., $\Sigma\Sigma X^2$) is the total marital integration measure, which is employed in "total-columns" tests of the theorem. Table 1 again serves as the illustration. Total marital integration for the social unit as a whole is 2.086 (i.e., .836 + 1.000 + .250), and any unit with a lower value would be expected to have a higher suicide rate. Observe, however, that several dimensions of status integration (e.g., occupational, parental) are ignored, and correct predictions of differences in suicide rates would be expected only to the extent that *all* dimensions enter into the measure.

MARITAL INTEGRATION AND SUICIDE IN THE UNITED STATES, 1959–61

Tables 2 and 3 provide data for tests of the theorem on white males and females in the U.S. Both sexes are classified by age, employing the conventional census and vital statistics groupings. Within all fifteen age groups, members are further distinguished by marital status, with marital integration and a suicide rate shown for each status in every age group. As such, the data can be used for both between-columns and within-columns tests of the theorem.

In addition to the rates in Tables 2 and 3, the source reports suicide by age for whites, non-whites, males, females, non-white males, non-white females, and the total population. These data were not used, and an explanation of their omission is in order. Ideal data would recognize all status distinctions. The data in Tables 2 and 3 are not ideal if only because several "status families" (e.g., parental, occupational) are excluded, and the data would be even more deficient if sex and color distinctions were ignored.

TABLE 1

MEASURES OF MARITAL INTEGRATION IN A HYPOTHETICAL POPULATION

MARITAL STATUS	STATUS CONFIGURATIONS*		
	I	II	III
	$A_2-S_1-E_3$ $O_4-R_2-P_2$	$A_4-S_1-E_3$ $O_3-R_1-P_1$	$A_5-S_2-E_3$ $O_7-R_5-P_3$
Single.....	.912	.000	.247
Married...	.063	1.000	.252
Widowed..	.010	.000	.251
Divorced..	.015	.000	.250
ΣX......	1.000	1.000	1.000
ΣX^2†....	.836	1.000	.250

* All possible combinations of occupied statuses in the population. In this illustration *A* signifies age; *S*, sex; *E*, ethnicity-race; *O*, occupation; *R*, religious affiliation; and *P*, parental status. Each number signifies a particular status with the family of statuses (e.g., S_1 would be a male, S_2 a female).

† Measure of marital integration for the status configuration.

Status distinctions are ignored also if statuses in the same family are combined, and one possible consequence is the "masking" of status integration. To illustrate, if marital composition varies appreciably from status to status, lumping all statuses together results in a low degree of marital integration for the composite category even though integration is substantial within each component population. Accordingly, tests of the theorem should not be based on categories that comprise two or more distinct statuses; hence no use was made of the suicide data for "non-whites," a category that includes Negroes, American Indians, and Orientals. True, census data are avail-

TABLE 2

MEASURES OF MARITAL INTEGRATION, 1960, AND AVERAGE ANNUAL SUICIDE RATES PER 100,000 POPULATION, 1959–61, BY MARITAL STATUS AND AGE, WHITE MALES IN THE UNITED STATES

Marital Status	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85+
Single:															
SR	5.4	13.1	23.9	31.3	35.9	38.6	43.8	52.4	55.9	57.4	61.6	74.2	84.5	90.2	72.6
Rank	4	3	3	3	3	3	3	3	3	3	3	3	2	2	3
M	.961	.526	.200	.113	.083	.071	.070	.075	.080	.076	.078	.079	.081	.076	.072
Rank	1	1	2	2	2	2	2	2	2	2	3	3	3	3	3
Married:															
SR	8.3	8.4	9.3	12.0	15.6	20.3	25.1	30.5	33.3	32.9	32.1	33.5	37.1	46.1	43.1
Rank	2	4	4	4	4	4	4	4	4	4	4	4	4	4	4
M	.038	.463	.781	.864	.890	.896	.889	.875	.855	.835	.800	.735	.650	.537	.383
Rank	2	2	1	1	1	1	1	1	1	1	1	1	1	1	2
Widowed:															
SR	26.0	142.3	95.7	83.7	121.0	92.8	91.6	100.8	88.1	82.5	72.3	78.7	80.3	83.4	82.3
Rank	1	1	1	1	1	2	2	2	2	2	2	2	3	3	2
M	.000	.001	.001	.002	.004	.007	.012	.019	.034	.060	.096	.162	.249	.371	.530
Rank	4	4	4	4	4	4	4	4	3	3	2	2	2	2	1
Divorced:															
SR	6.8	34.1	50.2	77.6	87.9	104.4	107.0	111.0	118.0	118.8	92.5	108.6	125.9	116.6	101.8
Rank	3	2	2	2	2	1	1	1	1	1	1	1	1	1	1
M	.001	.010	.018	.021	.023	.026	.029	.031	.031	.029	.027	.024	.021	.016	.014
Rank	3	3	3	3	3	3	3	3	4	4	4	4	4	4	4
ΣM	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.001	1.000	1.001	1.000	.999
ΣM²:*															
Value	.925	.491	.650	.760	.800	.809	.796	.773	.740	.707	.656	.573	.492	.432	.433
Rank	1	13	10	6	3	2	4	5	7	8	9	11	12	14	15
Suicide Rate:†															
Value	5.6	11.4	13.2	16.0	19.6	24.5	29.9	36.4	40.1	41.0	[illegible]	[illegible].5	54.1	65.1	67.7
Rank	15	14	13	12	11	10	9	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	3	2	1

NOTE.—SR = suicide rate; M = measure of marital integration.

* Measure of marital integration for the age group.

† Total suicide rate for the age group.

(Washington, D.C.: [illegible] Census of Popula- [illegible]

TABLE 3

MEASURES OF MARITAL INTEGRATION, 1960, AND AVERAGE ANNUAL SUICIDE RATES PER 100,000 POPULATION, 1959–61, BY MARITAL STATUS AND AGE, WHITE FEMALES IN THE UNITED STATES

Marital Status	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85+
Single:															
SR	1.3	3.8	8.9	11.5	11.3	10.8	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	3	3	3	3	3	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
M	.839	.274	.098	.066	.059	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	1	2	2	2	2	2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Married:															
SR	2.4	2.5	4.1	5.3	6.4	7.3	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	2	4	4	4	4	4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
M	.157	.705	.872	.896	.890	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	2	1	1	1	1	1	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Widowed:															
SR	0.0	10.9	10.8	17.9	12.8	13.4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	4	1	2	2	2	2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
M	.001	.003	.005	.009	.018	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	4	4	4	4	4	4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Divorced:															
SR	13.8	10.8	22.6	27.1	26.4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	1	2	1	1	1	1	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
M	.003	.018	.025	.029	.034	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	3	3	3	3	3	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
ΣM	1.000	1.000	1.000	1.000	1.001	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
ΣM^2:*															
Value	.729	.572	.771	.808	.797	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	5	9	3	1	2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Suicide Rate:†															
Value	1.6	3.1	5.1	6.5	7.6	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Rank	15	14	13	11	10	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

NOTE.—SR = suicide rate; M = measure of marital integration.

* Measure of marital integration for the age group.

† Total suicide rate for the age group.

TABLE 2

MEASURES OF MARITAL INTEGRATION, 1960, AND AVERAGE ANNUAL SUICIDE RATES PER 100,000 POPULATION, 1959–61, BY MARITAL STATUS AND AGE, WHITE MALES IN THE UNITED STATES

Marital Status	Age Groups														
	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85+
Single:															
SR	5.4	13.1	23.9	31.3	35.9	38.6	43.8	52.4	55.9	57.4	61.6	74.2	84.5	90.2	72.6
Rank	4	3	3	3	3	3	3	3	3	3	3	3	2	2	3
M	.961	.526	.200	.113	.083	.071	.070	.075	.080	.076	.078	.079	.081	.076	.072
Rank	1	1	2	2	2	2	2	2	2	2	3	3	3	3	3
Married:															
SR	8.3	8.4	9.3	12.0	15.6	20.3	25.1	30.5	33.3	32.9	32.1	33.5	37.1	46.1	43.1
Rank	2	4	4	4	4	4	4	4	4	4	4	4	4	4	4
M	.038	.463	.781	.864	.890	.896	.889	.875	.855	.835	.800	.735	.650	.537	.383
Rank	2	2	1	1	1	1	1	1	1	1	1	1	1	1	2
Widowed:															
SR	26.0	142.3	95.7	83.7	121.0	92.8	91.6	100.8	88.1	82.5	72.3	78.7	80.3	83.4	82.3
Rank	1	1	1	1	1	2	2	2	2	2	2	2	3	3	2
M	.000	.001	.001	.002	.004	.007	.012	.019	.034	.060	.096	.162	.249	.371	.530
Rank	4	4	4	4	4	4	4	4	3	3	2	2	2	2	1
Divorced:															
SR	6.8	34.1	50.2	77.6	87.9	104.4	107.0	111.0	118.0	118.8	92.5	108.6	125.9	116.6	101.8
Rank	3	2	2	2	2	1	1	1	1	1	1	1	1	1	1
M	.001	.010	.018	.021	.023	.026	.029	.031	.031	.029	.027	.024	.021	.016	.014
Rank	3	3	3	3	3	3	3	3	4	4	4	4	4	4	4
ΣM	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.001	1.000	1.001	1.000	.999
ΣM^2:*															
Value	.925	.491	.650	.760	.800	.809	.796	.773	.740	.707	.656	.573	.492	.432	.433
Rank	1	13	10	6	3	2	4	5	7	8	9	11	12	14	15
Suicide Rate:†															
Value	5.6	11.4	13.2	16.0	19.6	24.5	29.9	36.4	40.1	41.0	40.4	46.5	54.1	65.1	67.7
Rank	15	14	13	12	11	10	9	8	7	5	6	4	3	2	1

NOTE.—SR = suicide rate; M = measure of marital integration.

* Measure of marital integration for the age group.

† Total suicide rate for the age group.

SOURCE.—Public Health Service Publication No. 1000 (Ser. 20, No. 5) (Washington, D.C.: U.S. Government Printing Office, 1967), Table 6; U.S. Bureau of the Census, *U.S. Census of Population*, Final Report PC(1)–1D (Washington, D.C.: U.S. Government Printing Office, 1963), Table 176.

TABLE 3

MEASURES OF MARITAL INTEGRATION, 1960, AND AVERAGE ANNUAL SUICIDE RATES PER 100,000 POPULATION, 1959–61, BY MARITAL STATUS AND AGE, WHITE FEMALES IN THE UNITED STATES

Marital Status	Age Groups														
	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85+
Single:															
SR	1.3	3.8	8.9	11.5	11.3	10.8	10.2	10.5	9.6	9.6	8.7	9.8	5.2	5.7	4.1
Rank	3	3	3	3	3	3	3	3	3	3	3	3	4	3	3
M	.839	.274	.098	.066	.059	.060	.066	.078	.084	.079	.082	.087	.091	.099	.100
Rank	1	2	2	2	2	2	2	3	3	3	3	3	3	3	2
Married:															
SR	2.4	2.5	4.1	5.3	6.4	7.3	8.6	9.4	8.8	8.6	8.0	6.1	6.0	5.4	3.3
Rank	2	4	4	4	4	4	4	4	4	4	4	4	3	4	4
M	.157	.705	.872	.896	.890	.867	.833	.779	.709	.623	.524	.396	.278	.163	.081
Rank	2	1	1	1	1	1	1	1	1	1	1	2	2	2	3
Widowed:															
SR	0.0	10.9	10.8	17.9	12.8	13.4	17.7	16.3	16.0	14.1	11.0	10.9	9.9	8.3	5.5
Rank	4	1	2	2	2	2	2	2	2	2	2	2	2	2	2
M	.001	.003	.005	.009	.018	.034	.060	.102	.168	.265	.367	.496	.616	.727	.810
Rank	4	4	4	4	4	4	3	2	2	2	2	1	1	1	1
Divorced:															
SR	13.8	10.8	22.6	27.1	26.4	24.5	22.5	23.8	18.0	21.7	21.2	14.4	26.7	21.1	16.6
Rank	1	2	1	1	1	1	1	1	1	1	1	1	1	1	1
M	.003	.018	.025	.029	.034	.038	.041	.041	.039	.033	.027	.021	.015	.011	.008
Rank	3	3	3	3	3	3	4	4	4	4	4	4	4	4	4
ΣM	1.000	1.000	1.000	1.000	1.001	.999	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	.999
ΣM^2:*															
Value	.729	.572	.771	.808	.797	.758	.704	.625	.539	.466	.417	.411	.465	.565	.673
Rank	5	9	3	1	2	4	6	8	11	12	14	15	13	10	7
Suicide Rate:†															
Value	1.6	3.1	5.1	6.5	7.6	8.4	9.8	10.9	10.5	10.6	9.5	9.1	8.8	7.8	5.3
Rank	15	14	13	11	10	8	4	1	3	2	5	6	7	9	12

NOTE.—SR = suicide rate; M = measure of marital integration.

* Measure of marital integration for the age group.

† Total suicide rate for the age group.

SOURCE.—Public Health Service Publication No. 1000 (Ser. 20, No. 5) (Washington, D.C.: U.S. Government Printing Office, 1967), Table 6; U.S. Bureau of the Census, *U.S. Census of Population*, Final Report PC(1)-1D (Washington, D.C.: U.S. Government Printing Office, 1963), Table 176.

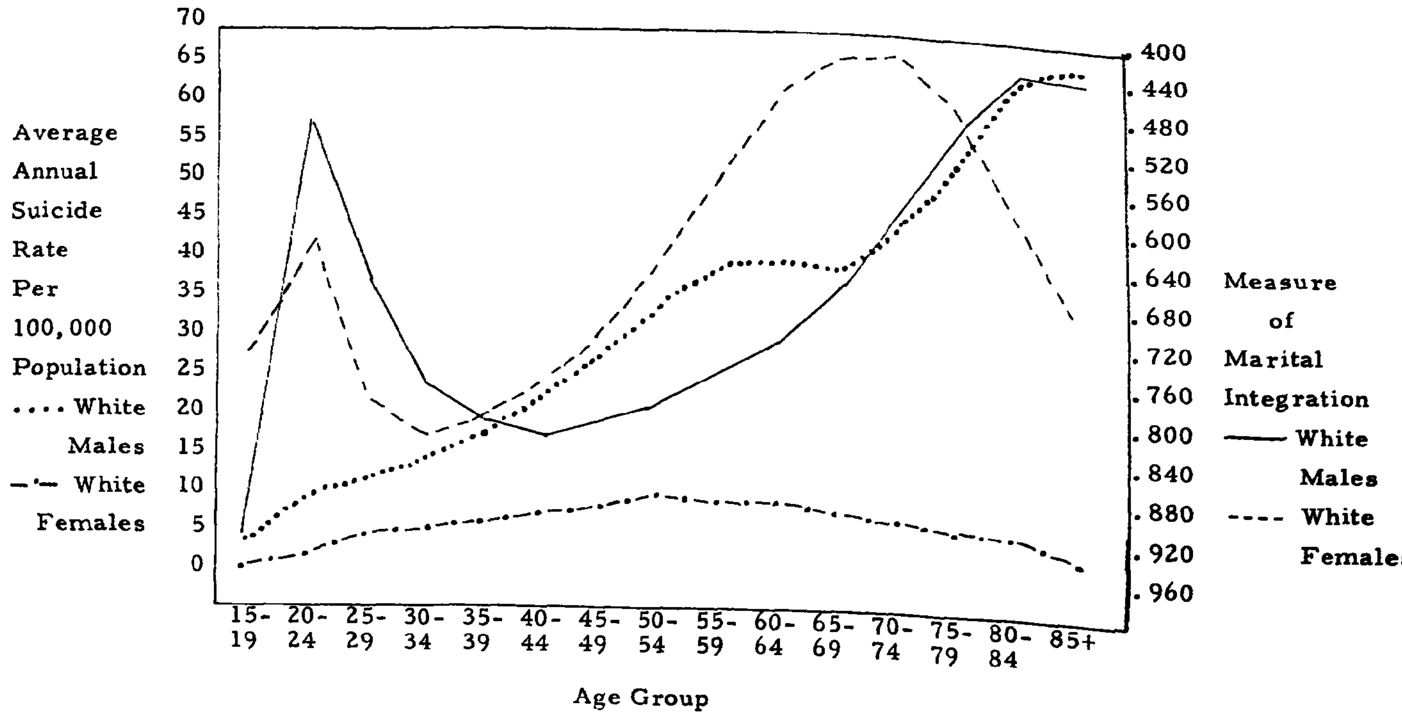


FIG. 1.—Average annual suicide rates, 1959–61, and measures of marital integration by age, 1960, for white males and females in the United States. Source: Tables 2 and 3.

able for such specific categories, but corresponding suicide rates by *marital status* are *not* available.

White males.—The last four rows of Table 2 provide data for a between-columns test on white males. According to the theorem, among the age groups an inverse relation should hold between the suicide rate and marital integration. Variation in the rank values is, on the whole, consistent with the theorem (ρ is $-.59$ [$N=15$]).

Figure 1 shows that some age groups are conspicuous exceptions to the inverse relation. Between ages 20 and 44 both marital integration and the suicide rate *increase*. Since only marital integration is considered, some exceptions are not surprising; but their concentration is noteworthy, and further commentary on that point is made later.

Figure 1 also reveals an instance of striking conformity to the theorem. Beyond age 65 the white male suicide rate *increases* substantially up to age 84 and then levels off abruptly. Significantly, marital integration *decreases* rapidly between age 65 and 84, and then levels off abruptly.

Returning to Table 2, the ranks of suicide rates and integration measures for a marital status are relative to each age group. For example, within the age group 15–19, the single have the greatest marital integration (rank = 1) and the lowest suicide rate (rank = 4). Accordingly, a within-columns test of the major theorem can be conducted on each age group. Since N is only four in each age group, Figure 2 rather than ρ shows the relation between ranks. Over-all, the relation is clearly inverse. Of 60 predictions, 43, or 72 per cent, are correct. By contrast,

Rank of Suicide Rate	IV (Low)	III	II	I (High)
(High) I	Males: 12 Females: 10	Males: 3 Females: 5		
II	Males: 3 Females: 4	Males: 8 Females: 2	Males: 3 Females: 5	Males: 1 Females: 4
III		Males: 4 Females: 6	Males: 10 Females: 8	Males: 1 Females: 1
(Low) IV	Females: 1	Females: 2	Males: 2 Females: 2	Males: 13 Females: 10

Rank of Degree of Marital Integration

FIG. 2.—The relation between measures of marital integration, 1960, and average annual suicide rates, 1959–61, for four marital statuses within each of 15 age groups, white males and white females in the United States. Source: Tables 2 and 3.

assuming a random distribution, the expected number of correct predictions is 15, or 25 per cent, which means that the ratio of correct predictions to the number expected by chance alone is nearly three to one. The magnitude of prediction errors is perhaps even more significant. Whereas 22.5 errors of more than one rank would be expected on the basis of chance, in only *one* instance is the error of that magnitude.

White females.—Figure 1 and the last two rows of Table 3 reveal a distinctive pattern in the white female suicide rate by age. Whereas rates for males increase somewhat regularly throughout all ages, the female rate increases only to about age 50. Concordantly, variation in marital integration by age among females is markedly different. For both sexes there is a sharp decrease in marital integration past age 40, but the decrease begins to level off some 20 years earlier for females.

The above observations on sexual differences are consistent with the theorem, but they do not constitute a formal between-columns test. That is accomplished by computing ρ for the two sets of ranks in the bottom rows of Table 3. The coefficient is $-.51$ ($N = 15$) and, as such, consistent with the theorem.

Some white female age groups are conspicuous exceptions to the general inverse relation, and those exceptions are obvious in Figure 1. As white females move through the life cycle, eventually their suicide rate decreases and marital integration increases, but the former changes at about age 50 and the latter at about age 74. Further, the white female suicide rate increases slightly between ages 20 and 34, but, contrary to the theorem, in the same interval marital integration also increases. Finally, from age 35 to age 79 marital integration is consistently less for females than males, but the male suicide rate is much greater. To summarize, even though the between-columns test results for white females are fairly consistent with the theorem, some age groups are conspicuous exceptions, so much so that they are considered later as special cases.

Figure 2 provides data for a within-columns test of the theorem on white females. With four marital statuses in 15 age groups, the test takes the form of predicting the rank of 60 suicide rates. Of that number, 30, or 50 per cent, are predicted correctly, as compared to 15, or 25 per cent, expected by chance. In seven instances the rank predicted is more than one rank removed from that observed; but, if there were no relation between marital integration and suicide rates, the number would be 22.5, which means that gross prediction errors are reduced more than two-thirds.

While a general inverse relation between marital integration and suicide rates is evident in Figure 2, the results are less positive for white females. The sexual difference may appear anomalous, especially if one assumes that marital status is more nearly the "key" status for women than men. However, the findings illustrate the limitations of "common knowledge" and the need to formulate principles for interpreting tests results.

SOME GENERAL PRINCIPLES CONCERNING TEST CONDITIONS

Sociology is a graveyard of monistic theories, so much so that the notion of multiple causation is virtually an article of faith in the field. That notion contradicts the principle of parsimony, and it is certainly questionable to reject a theory simply because it is monistic. On the other hand, few if any relations between social variables appear invariant, and the challenge in theory construction is to specify the conditions where a relation will assume one form rather than another (including no relation). If that strategy is adopted, negative evidence may lead not to the rejection of the theory but rather to a specification of the conditions where the predicted relation does hold, that is, a restriction of the theory.

The problem with the strategy is that some sociologists are prone to label the restriction of a theory as an "auxiliary hypothesis" and to interpret it as an attempt

to ignore negative evidence.[3] That objection would be valid only if the specification of conditions is *ad hoc* (e.g., dismissing negative evidence by merely declaring that the theory does not apply to a particular population).

Concern with conditions is especially relevant in assessing the status integration theory, which is monistic in that it asserts an invariant relation between status integration and the suicide rate. Some attention has been given to the conditions that influence the relation,[4] but a systematic articulation of principles is needed. In this instance the principles are illustrated by an interpretation of test results for Tables 2 and 3. Since the interpretation refers to variables not considered in the tests, it may appear ex post facto. Note, however, that the principles have general applicability, and their validity can be assessed systematically in future research. Published data are available for such an assessment, and they are not used here only because of space limitations.

Test conditions.—As long as tests are based on census data and official vital statistics, the relation between status integration and suicide rates will be only moderately close. Even if their reliability were assured, such published data would not be truly adequate for a test of the theorem.[5] The foremost problem is that census data *and corresponding* vital statistics are seldom cross-tabulated for more than four status families (e.g., marital status, age, sex, and race). Consequently, in most tests only a few dimensions of status integration enter into the measures, and the number of dimensions vary from test to test. Given that situation, inconsistent results are not surprising, and a related pattern is anticipated by the following generalization (principle I): *The greater the number of status families that enter into integration measures, the greater the inverse association between those measures and the suicide rates.*

Tables 2 and 3 illustrate the relevance of principle I and some related complexities. The only moderately close inverse relations may reflect the fact that *only* four families (race, sex, age, and marital) enter into the measures.[6] However, the principle does not provide a ready explanation of more positive results for males, which illustrates the problematical character of the relation stipulated in principle I.

Exclusion of a status family may not

[3] See, e.g., William J. Chambliss and Marion F. Steele, "Status Integration and Suicide: An Assessment," *American Sociological Review*, XXXI (August, 1966), 524–32, esp. 525–26.

[4] See Gibbs and Martin, *op. cit.*

[5] Douglas makes the reliability of official suicide rates *the* issue in his critique of the theory. Actually, the state of our knowledge precludes a truly defensible conclusion on the subject. In conducting tests of a theory the *absolute* reliability of official rate is not the issue; rather, it is the amount of variation among populations in *relative* reliability (the proportion of cases of suicide reported as such). Further, unless a particular kind of bias in official statistics can be demonstrated, the "unreliability" of suicide statistics produces negative findings in tests of the theorem. Douglas asserts such a bias by *assuming* that: (1) measures of status integration vary inversely with urbanization and (2) urbanization varies inversely with the *rate of concealment of suicides*. These two assumptions enable Douglas to dismiss positive findings in tests of the theorem. However, even if Douglas' first assumption is granted, it is difficult to see how urbanization creates a bias in official statistics on marital status by age. To explain away the positive findings reported in this paper, Douglas will have to invent additional rates of concealment. The word "invent" is used deliberately because Douglas speaks of rates of concealment without presenting any data whatever. See Jack D. Douglas, *The Social Meanings of Suicide* (Princeton, N.J.: Princeton University Press, 1967), esp. p. 227.

[6] Still another complicating factor is that the category "married" in the present statistics includes the "separated" (i.e., spouses living apart and estranged) and the "legally separated," both of which should be treated as distinct statuses. This situation suggests a more general consideration. The adequacy of data for tests of the theorem depends not only on the number of "status families" that enter into the integration measure but also on the proportion of status distinctions within each family that are recognized in the data. For the sake of simplicity, only the number of status families is considered in principle I, but status distinctions within each family is another factor that eventually will deserve attention in further development of the theory.

influence test results, and the amount of influence depends on how much the composition of the populations differs in regard to the excluded status family. For example, if women and men have approximately the same marital composition in each age group, measures of marital integration with age are not influenced by ignoring sexual differences. Similarly, if all males enter the labor force at the same age and remain economically active until death, then labor force integration would not be expected to influence variation in the suicide rate by age. The crucial point is that a very close relation may hold between status integration measures and suicide rates even though a few status families enter into the measures, which means that principle I is expected to hold only generally. But the principle is less problematical than might appear. While a fairly close inverse relation between integration measures and the suicide rate may be found *without* considering a large number of status families, instances of no relation (or a direct relation) are most likely when the integration measures are based on a small number of statuses. Stated another way, a small number of status families is more nearly a necessary than a sufficient condition for negative test results.

The problematical character of principle I is especially relevant in assessing the within-columns tests for males and females. The most important consideration is the possibility that exclusion of labor force status has different consequences for males and females. General observations indicate that labor force status is more closely associated with marital status among females than among males. Regardless of marital status, from age 20 to age 64 the vast majority of males are in the labor force, which means a fairly uniform degree of labor force integration from one marital status to another.[7] Not so for females. Among them the proportion varies considerably among the four marital statuses. To summarize, the hypothesis is that the within-columns test produced more positive results for males because exclusion of at least one status (labor force) had a more uniform effect from one marital status to the next among males.

Labor force status is also relevant in considering why the white female suicide rate decreases at about age 50 but marital integration does not increase until after age 74. White females typically cease participation in the labor force much earlier than males, with one consequence being a marked increase in the integration of labor force status with age among white females past age 50, and hence a possible reason for a decline in the suicide rate earlier than indicated by marital integration.

Both labor force and occupational integration may be relevant in explaining the sexual difference in the suicide rate and marital integration between ages 35 and 79, a difference, as noted previously, that is inconsistent with the theorem. While females generally have less marital integration, their labor force integration probably exceeds that of males substantially after about age 50. Below that age, occupational integration could be the crucial factor in sexual differences. Although occupational integration varies from one age group to the next among both males and females, 1940 and 1950 data clearly show that occupational integration is much greater for females than males in all age groups.[8]

Amount of variation in the suicide rate.—The status integration theory purports to explain only variation in the suicide rate and not particular instances (i.e., why some individuals commit the act). However, there is a random, problematical quality of a rate, such that a consistent unity correlation between a rate and any other variable is most unlikely, and questions concerning the reliability of official suicide statistics make the correlations all the more problematical. In other words, some variation in suiside rates is not explicable by reference to status integration and that is probably true

[7] See U.S. Bureau of the Census, *U.S. Census of Population: 1960*. Final Report PC (1)-1D (Washington, D.C.: U.S. Government Printing Office, 1963), Table 196.

[8] Gibbs and Martin, *op. cit.*, pp. 62 and 105.

regardless of how many status families enter into the measures. The amount of variation in the suicide rate that is random (or, put otherwise, inexplicable) is unknown and unknowable. Nonetheless, it can be taken into account indirectly as a test condition because the problematical quality of a rate becomes particularly crucial when differences in rates are minute. Given minute differences, the rate for one population may be greater than that of another because of random factors and errors in mortality statistics. But if the difference is substantial, there is reason to assume a close rank-order correlation between the "reported" and "true" suicide rates.[9] Accordingly, specification of another test condition is stated as principle II: *The greater the variation in the suicide rates of populations, the more inverse will be the association between those rates and measures of status integration among those populations.*[10]

The more positive within-columns findings for males are consistent with principle II. Only a glance at Tables 2 and 3 reveals that intracolumn variation in the suicide rate is far greater for males. Moreover, principle II is also relevant in explaining the somewhat higher coefficient of correlation for males in the between-columns test, as variation in the suicide rate among age groups is substantially greater for males.

Whereas principle II assumes that minute differences in suicide rates largely reflect random, problematical factors, the third principle assumes that extremely high *or* extremely low rates may be produced by the same factors. Some populations have uniformly high or uniformly low rates year after year, while in others the rate fluctuates markedly. While trend analysis reveals that populations differ sharply as to the stability of the suicide rate, it is nonetheless impossible to determine the "true" volume of suicide independent of fluctuations or, for that matter, to clearly distinguish "stable" from "unstable" rates. However, population size tends to be directly related to the stability of the rate.[11] That observation is the basis for principle III: *The greater the median size of the populations, the greater will be the inverse association between suicide rates and measures of status integration among those populations.*

Systematic application of the third

[9] Until better data are available for the measurement of status integration and until more confidence can be placed in the reliability of mortality statistics, tests of the theorem should focus exclusively on the ordinal differences in the suicide rates rather than on the magnitude of the rates. Moreover, regardless of the adequacy of the data, it may well be that the theory can never predict the magnitude of rates without considerable error. But that would be a basis for rejecting the theory only if one subscribes to Blalock's assertion that "regression coefficients . . . give us the laws of science." Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964), p. 51. Blalock's assertion reflects a very narrow conception of the "laws of science"; and, in any event, a demand for regression coefficients is an excellent way to reject a theory prematurely.

[10] Principle II is consistent with Blalock's observations on the effect of variation in values on coefficients of correlation. However, it is inconsistent with his pronouncement (*ibid.*, p. 96) that "manipulations should not be made in terms of the variable or variables that are taken to be dependent." The rationale for this "methodological rule" stems from Blalock's preoccupation with causal models, a preoccupation not shared by me. What he and some other methodologists in sociology ignore is that an exhaustive analysis of a finite body of data by some elaborate technique (e.g., path analysis, causal models) does not resolve the dilemma of induction. True, a resolution of that dilemma may not be possible, but, unless one subscribes to a naïve empiricism, techniques are not an adequate substitute for theory in attempting to formulate generalizations about an infinite universe. Nor should a concern with techniques be allowed to dictate inflexible rules that preclude solution of particular substantive problems. In this instance, the only feasible way in which the influence of random factors on suicide statistics might possibly be lessened is by manipulation of the suicide rate and not the measures of status integration.

[11] Blalock's observation (*ibid.*, p. 99) provides a succinct explanation: "One individual may be saved from suicide by an unusually understanding wife or policeman, another by the failure of some mechanical device. If the unit of analysis is relatively small (say a small county or city), the number of suicides may vary considerably from one year to the next. But with a larger unit, such idiosyncratic factors are more likely to cancel each other out, giving a more stable rate."

principle to the present findings is precluded by space limitations, but the outstanding negative case in the within-columns tests can be considered for the purpose of illustration. Among white females age 15–19, all of the within-column rank predictions are incorrect. Since there are so few widowed or divorced individuals in that age group, the median size of the four populations is very small, and the test results are consistent with principle III.

Duration of status occupancy.—It is virtually a sociological truism that socialization is not instantaneous and that behavioral uniformities are not immediate effects of a social condition. Stated another way, the behavioral consequences of a social condition become increasingly determinate with exposure to that condition. Accordingly, no claim is made that a low degree of status integration produces a disruption of social relations instantaneously, or even that suicide is an immediate consequence of the disruption. The point is that a low degree of integration within a status configuration produces a high suicide rate only to the extent that the individuals in that configuration have occupied it for a long period. For purposes of specifying test conditions, the general idea is stated formally as principle IV: *The greater and more constant the average duration of occupancy among a series of status configurations, the more inverse will be the association between suicide rates and measures of status integration among those configurations.*

Unfortunately, census data on the duration of status occupancy are rare and fragmentary; therefore, systematic application of the fourth principle is not possible for all tests. Nonetheless, a few general observations can be made in connection with present findings. Accepting principle IV as valid, the concordant finding is that prediction errors in within-columns tests vary substantially among the four marital statuses. To illustrate, considering both sexes (Tables 2 and 3 combined), there are nineteen errors in predicting the rank of the widowed suicide rate but only seven for the married. Such differences are not surprising because average duration of occupancy is extremely variable among the four statuses. As a case in point, whereas the duration for a single person (meaning "never married") is fixed by age, it is obviously problematical for the divorced. Continuing with general observations, the average duration of status occupancy probably is much less for the widowed than the married, but greater for either than for the divorced.

Until suspected differences among marital statuses as to duration of occupancy are systematically documented, the interpretation of the present findings with reference to principle IV is only plausible. However, the conjecture is minimized when the principle is applied to one conspicuous negative case. Among both sexes the suicide rate increases between ages 20 and 34, but, contrary to expectation, marital integration does not decrease in that interval. This anomaly may be due partially to the exclusion of several statuses from the integration measures, but an additional reason is that in that age interval the average duration of occupancy is not long for any marital status other than the single, and the single negate the possibility of constancy in average duration of occupancy among the four statuses. What has been said of the age group 20–34 applies even more so to the age group 15–19. Since the latter is a transitional period in connection with marital status (especially for females), the average duration of status occupancy is brief for all but the single.

SUMMARY AND CONCLUSIONS

The present research employed a rare kind of statistics to test the theory of status integration and suicide. Data were provided by an official publication that reports, *inter alia*, suicide rates by age and marital status for white males and females in the United States, 1959–61. On the whole, the test results are consistent with the theory.

Results for males are more consistent with the theory than those for females, and that finding is not surprising. Previous research has revealed that results are not

uniform from one test to the next, meaning that the association between the suicide rate and measures of status integration is generally inverse but by no means a constant in all conditions.

There are several bases for anticipating variant results in tests of the theory, and they have been stated formally in the way of four principles. These principles are summarized here by stipulating the condition under which a test is most likely to produce negative results: only one status family enters into the measures of integration, very little variation in the suicide rate among populations, a small median size of the populations, and an average duration of occupancy that is highly variable among the status configurations.

The four principles are illustrated by applying them to results of tests on males and females in the United States. Those results appear generally consistent with the principle, but space limitations precluded a systematic assessment. However, data are available for systematic tests of three of the four principles, and that opportunity should be realized in future research.

The principles are all the more important because it is improbable that a truly adequate test of the theory can ever be conducted. However, since the principles specify the ideal test conditions systematically, any test can be judged in terms of the degree that it approximates the ideal condition. That strategy does not mean that all relevant test conditions have been identified. There may be more, and the history of science clearly counsels against regarding the statement of a theory as final. At the same time, however, the formulation of "test" principles means neither a revision of the theory nor an attempt to negate the possibility of bringing evidence to bear on it. In that connection, however, the appropriate criterion for evaluating the theory is still its predictive power in comparison with other theories.

UNIVERSITY OF TEXAS AT AUSTIN

COMMENTARY AND DEBATE

Comments on Mark and Schwirian's Article on Population Growth

The article by Mark and Schwirian in the July, 1967, issue of the *Journal* (Harold Mark and Kent P. Schwirian, "Ecological Position, Urban Central-Place Function, and Community Population Growth," *American Journal of Sociology*, LXXIII [July, 1967], 30–41) suggested several interesting hypotheses, but unfortunately the statistical operations carried out were marred by a set of overlapping operationalizations of key concepts. The authors state that "One of the most fiequently used measures of central-place function and the one used in this study is per capita retail sales" (p. 36). As total retail sales were not available to them, they substituted total sales tax receipts in its place to obtain the following equation:

Central-place function

$$= \frac{\text{total retail sales} \times .02}{\text{total community population}} \times 10\,.$$

The concept of central-place function was intended to refer to "the extent of a community's retail-trade function" (p. 35). This concept, together with a delineation of five ecological classes of cities and the simple measurement of population increase, stands at the center of the authors' hypothesis: "That during regional industrialization while there is rapid population growth and expansion of central-place function for a certain ecological class of cities, central-place function is no longer a significant community-building activity" (p. 33).

As a test of their hypothesis, the authors correlated changes in a city's central-place function (c.p.f.) with changes in its population from 1950 to 1960 for ninety-two selected Iowa cities. They discard the correlations for the central cities and suburbs because they do not reach the .05 level of confidence. They go on to state "for the remaining towns (metropolitan neighbors, non-competitive trade centers, and competitive trade centers) the correlations are statistically significant, of moderate strength, and *negative*" (p. 39).

Mark and Schwirian interpret these negative correlations as indicating that "those towns growing in population are doing so in spite of dwindling central-place functions, while those towns losing population tend to have expanding central-place functions" (p. 39). I was surprised at this result, as were the authors. They find it curious that "59 per cent of the towns losing population actually experienced expansion of their central-place function" (p. 39). Their explanation is that "in the areas of rapid rural depopulation there is a marked reorganization and reconcentration of economic activities.... The urban centers with central-place expansions in these areas are probably those which become the focal points of the economic reorganization" (p. 40). They feel that this phenomenon is of "short-run duration" (p. 40).

The authors then claim that they have demonstrated that "community-population growth is not a function of central-place function expansion" (p. 40). To the contrary, the point of my critique is that c.p.f. expansion is of necessity a function of community-population growth by the very method of the operationalization of the two concepts.

To get to the heart of the authors' difficulties, we must go back to the definition of c.p.f. The numerator of the function is the

total retail sales of a community as multiplied by .02 (the state sales tax). However, the crucial component is the denominator of the function—the total community population. The reader will note that the c.p.f. is correlated with "changes in total community population" by the authors, yet both measures have a common component—community population. Once this fact is recognized, it is not difficult to see how the authors' results arose.

To take a simple case: If the community population increases while sales remain constant, then the denominator of the c.p.f. is increased and the value of the c.p.f. decreases. Conversely, if the community population decreases while total sales remain constant, then the denominator of the c.p.f. decreases and the value of the c.p.f. increases. (These facts are well known to anyone who has graphed the function of a hyperbola, e.g., $f(x) = 1/x$.) Of course, in actuality, sales do not remain constant as the population increases or decreases. However, it is already evident that the c.p.f. and community population are not independent measures of the concepts they purport to represent.

To be realistic, we have to deal with the matter of the numerator of the equation—total retail sales. As I have stated, sales do not remain constant since new residents of the city bring money with them, or departing residents cease to spend in the city. But, the amount each incoming or outgoing resident spends is of critical importance because of its effects on the c.p.f. Only if each new resident spends exactly as much as the c.p.f. average will the ratio remain unchanged. Likewise, only if each departing resident was an "average spender" will the measure be unaffected.

However, we have reasons to believe that it is not "average" spenders who are emigrants or immigrants. For instance, if the people coming into the larger cities are below-average spenders (as compared to the population of the host city), then their addition of the population will bring down the c.p.f. There is every reason to suspect this will be the case, since apparently the population increase is accounted for by people leaving farms and small villages to come into lower-level and lower-paying jobs in the city. Thus the gross sales of the city will increase but not in proportion to the population increase—thereby changing the c.p.f.

In fact, the two means by which a city increases its population (barring annexations) both appear to have an adverse effect on the c.p.f., even in a period of generally rising sales receipts. The two means, births and immigration, both add residents to the population who are below-average spenders. The very fact of a city growing rapidly would therefore tend to lower its c.p.f. relative to other cities, irrespective of actual changes in its "retail-trade function." For example, a classic instance of rural-urban migration is that of young women leaving rural areas to find employment in the cities. In rural areas, these women were counted only in the denominator of the c.p.f., thus lowering its value, while their leaving decreases the denominator, thus increasing the value of the c.p.f. Similarly, their addition to the city population (provided they find work) adds to the denominator and numerator of the c.p.f., but, because women generally go into the lower-paying occupations, the c.p.f. of the city is adversely affected.

We now can see how this same type of phenomenon accounts for the other "curious aspect of the negative correlations"—that of towns losing population but expanding their c.p.f. If a city loses population, and those persons lost are from the bottom half of the income and spending distribution, then their loss automatically brings up the c.p.f. of the city. This surely cannot be labeled an "expansion of their central-place functions."

In short, by choosing a measure of the central-place function which incorporates total population as its denominator, the authors have confounded any interpretation of their results. They have, in effect, chosen one variable such that its increase lowers the value of the second variable, other things being equal. Correlating two measures

which are already interrelated is, at best, a questionable venture.

The authors would do well to search out another indicator of what they call the "central-place function." This measure should be more independent of total community population than is the present indicator in order to avoid the above-mentioned problems of interpretation.

HOWARD E. ALDRICH
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The Authors Reply

In commenting upon our paper "Ecological Position, Urban Central-Place Function and Community Population Growth," Aldrich states our general conclusion correctly that community population growth is *not* a function of change in local central-place function. However, he goes on to argue to the contrary, that central-place expansion is by *necessity* a function of local population growth. His argument is based upon certain assumptions about the differential spending patterns of migrants and non-migrants. Basically, he suggests that the migrant from the small town or rural area to the larger city is a below-average spender. By moving to the city his absence in the rural area serves to increase the local per capita retail sales figure while his presence in the city serves to decrease the per capita retail sales there. Thus, the rapidly growing cities should have comparatively below-average increases in per capita retail sales while the declining small towns should have comparatively higher rates of growth in per capita retail sales. Although this line of reasoning is interesting, it is not supported by the data presented in our paper.

In developing his argument Aldrich appears to have ignored the correlation coefficient for *all* ninety-two urban places and, instead, focuses on the negative correlation for only the hinterland towns. The r for all of the towns is only .06, which indicates that less than 1 per cent of local population growth is statistically accounted for by expansion of local central-place function. It is upon this finding that we base our general conclusion about the lack of correlation between the two variables.

We should also point out that, if, as Aldrich asserts, "the very fact of a city growing rapidly would therefore tend to lower its c.p.f. relative to other cities," then we would expect the cities to be growing rapidly in population but to be comparatively below average in per capita sales while the hinterland towns would be growing slowly or losing population while they would have an above-average increase in per capita sales. Our data show that this is not the case. The central cities have higher rates of growth in both population and per capita sales. The average rate of population growth for the central cities is 10.9, while it is 3.9 for the hinterland towns (see Table 1). The average rate of increase in per capita retail sales for the central cities is 41.2, while it is only 33.1 for the hinterland towns (see Table 3).

If Aldrich shares our puzzlement about the negative correlations for the hinterland centers, he would do well to pursue a theoretical explanation that is consistent with the findings.

KENT P. SCHWIRIAN
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HAROLD MARK
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Comments on the Sociology of Georges Gurvitch

The review by Terry N. Clark of the last published work of the late Georges Gurvitch, *Les cadres sociaux de la connaissance* (*American Journal of Sociology*, LXXIII [March, 1968], 643–44), raised some important issues with regard to the relationships between European and American sociology, especially on the level of constructive mutual criticism. My chief concern is that by damning with faint praise an outstanding sociological theorist, the review may serve to perpetuate a long-standing and

lamentable neglect of the work of Gurvitch by American sociologists.

Contrary to Clark's judgment that there is a danger of Gurvitch's work being almost ignored in France in a few years' time, I would suggest that the more pressing danger is that it will continue to be ignored in America. Indeed, the reluctance to translate his many works into English will probably be strengthened by this latest negative review.

I find it difficult to accept the reviewer's criteria for describing this as Gurvitch's "worst book," especially as no comparisons are offered from his other works. The criteria utilized seem inappropriate in that they bear no relation to the stated objectives of the author and the format of the book (the reproduction of a course of public lectures). Thus, complaints about the shortness of the chapters and the paucity of empirical materials can be given weight only if we ignore the author's own statement in the Preface:

> The author is the first to realise that this book risks seeming too abstract and too schematic, particularly to the uninitiated. Obviously more abundant material could have been used. But if this work is limited to using certain choice examples, it is because it aims, not to be complete, but to indicate new directions for research, so often neglected or unappreciated by its predecessors. In other words, it aims *to formulate an original plan of research rather than to present definitive results* [p. viii, my translation].

Within these self-imposed limits, I would maintain that Gurvitch's qualities are conspicuous. They are particularly evident in his stimulating and creative discussion of the scope and method of the sociology of knowledge (especially in relation to philosophy). It is here that Gurvitch demonstrates to the full the value of his elaborate dialectical method as a tool for analysis and correction, especially in its role of clarifying and correcting philosophical preconceptions in the sociology of knowledge. The dialectical approach has the central role in enabling Gurvitch to suggest the dynamic relationships between the different types of sociability bonds—just as he had previously utilized it in describing the relationships of the spontaneous and the organized, the hierarchies of depth levels, or social times, or kinds of morality or law.

This brings me to the "hyperbolic *ad hominem* remarks" attributed to Gurvitch. Both examples given in the review depend for some of their force on being lifted out of context. In the case of the remark about Parsons, the context makes it clear that the reference is to Parsons' work seen as a product of "abstract culturalism," and it is this that "merits a special mention as a frightful example of the emptiness, *to which 'abstract culturalism' leads*" (my emphasis; the last phrase is omitted by Clark).

Certainly Gurvitch could be caustic in his criticisms of other people's theories, but this in no way invalidates the arguments that he uses. He saw criticism as an inescapable concomitant of his own dialectical method. In that sense he would no doubt have welcomed the critical review of his last work. But by the same token, both the dialectic and Gurvitch's reputation seem to me to necessitate critical response to that review. In normal circumstances this would come about naturally when a wide cross-section of sociologists arrived at their own judgments after reading the book, but in view of the relative inaccessibility of this book, due to the absence of an English translation, that possibility will be denied to many. It is all the more important, therefore, that every effort should be made to encourage sociologists either to temper or to suspend their judgments on both this book and Gurvitch's other work, until they are familiar to a wider professional audience.

KENNETH A. THOMPSON

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The Reviewer Replies

The concern shown in the comments of Kenneth A. Thompson for the reputation of the late Georges Gurvitch is laudable. But to attempt to enhance the reputation of Gurvitch, or anyone else, by a favorable re-

view of a work of dubious quality is likely to do more harm than good, at least in the long run.

It is certainly true that Gurvitch was a leading contemporary sociologist. I also retain a certain personal nostalgia for Gurvitch, for it was his seminar at the École Pratique des Hautes Études which introduced me to sociology. Given the importance of many of Gurvitch's contributions, I was especially sorry that his last book was so disappointing. But does the review not make this sufficiently clear in stating that "there is a danger that in a few years his works will be almost entirely ignored. This would be truly unfortunate as, particularly in earlier years, Gurvitch completed a number of studies which, although perhaps not brilliant, represented solid contributions" and then in cautioning that "Gurvitch's reputation in future years will no doubt have its ups and downs . . . but it is to be hoped that critics who seek to pass a judgment on his total contribution will not linger too long on the present volume. There are better elsewhere." It was also specifically recommended that "His *Vocation actuelle de la sociologie*, in two volumes, might be consulted."

Sorbonne professors occupied (past tense seems appropriate given the present uncertainties) a quasi-monopolistic position in the French intellectual marketplace, with the consequence that many—including Gurvitch—occasionally relaxed their intellectual standards both in lecture and in print. That Gurvitch recognized that these published lectures were weak, and apologized in the Preface, seems poor reason to consider them anything but weak. This is especially true because Sorbonne lectures have often been so outstanding—many of Émile Durkheim's volumes are slightly revised lecture notes, as are the more recent studies of Raymond Aron on industrial society and the history of social thought. Previously published lectures of Gurvitch were also often of value, although Gurvitch wrote far better than he lectured.

It is true that Gurvitch's remarks about Parsons and Lévi-Strauss occur within the context of criticisms of their more general ideas. But if statements that Parsons is "the sclerosis of American sociological theory [and] . . . a frightful example of emptiness" and "Lévi-Strauss . . . seeks in this work to avoid difficulties rather than to solve them" are not *ad hominem* attacks, what are they? It is certainly possible to criticize systems of thought in less directly personal fashion.

Still, these particular negative points should not be inflated to obscure the more important general issue: Gurvitch made a number of worthwhile contributions to sociological theory that too often have been ignored in the English-speaking countries. He deserves more careful attention, particularly in survey volumes and courses on sociological theory. Translating some more of his works, especially sections of the *Vocation*, would help. It would also help if English-speaking graduate students would learn to read French. But the last suggestion might be construed as an *ad communitem* remark, so perhaps it would be wiser not to make it.

TERRY N. CLARK

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A Rejoinder to Hans-Dieter Evers by the Reviewer

Hans-Dieter Evers has emotionally reacted (in the July, 1967, issue of this *Journal*) to my review of Hendry's *Small World of Khanh Hau*.[1] He has harped on two frivolous points and ignored the basic questions I raised about Hendry's book in my review. He has further impugned my "scholarly competence," which I consider to be obviously in bad taste.

First, about Evers' important revelation that the research was done in 1958–59 and not 1957–59. The blurb on the jacket of the

[1] James B. Hendry, *The Small World of Khanh Hau* (Chicago: Aldine Publishing Co., 1964), reviewed by Baidya Nath Varma in the *American Journal of Sociology*, LXXII (1967), 422–23.

book states that "Mr. Hendry conducted the research for this book during 1957–59," but Hendry in his Foreword notes that it was 1958–59. I noticed this discrepancy and then in reading through the book I found that much of the material was impressionistic, especially when Hendry made attempts "to assess its [the economic activity in the village of Khanh Hau's] meaning." Because he has devoted at least as much space to this evaluative enterprise as to his benchmark data, I looked up the period of his stay in Vietnam. The vita supplied by Hendry to the *American Men of Science* (10th ed.) states, and he corroborates it in the book, that he was in Saigon on the staff of the Vietnam Advisory Group of Michigan State University during 1957–59. I therefore used these dates for his effective knowledge of the world of the South Vietnamese people.

I know of the publications of L. W. Woodruff and Gerald C. Hickey, to which Evers refers, and I have said so in the review. It must have escaped Evers' hasty reading of my short review. The first two monographs were issued in mimeograph form in Saigon in 1960, and they never saw the light of commercial publication. Hickey's book *Village in Vietnam*[2] was published at the same time as Hendry's book and was understandably not available to me when I was writing the review. However, what is important is that Hendry has drawn heavily on the two earlier (1960) reports of Woodruff and Hickey in chapter xiii of his book. He was also in close touch with them during the research period and "in some ways, the members of the group participated together" (p. 273). When I am talking of "the smallness of vision of this team of social researchers," I have my feet firmly rooted in the facts of their constant consultation and co-operation and the field data which Hendry has delivered.

After having disposed of the two minor points of misunderstanding, I should like to challenge the statement of Evers that the book is "an important addition to our knowledge." The facts noted below in this connection may be considered as an extension of my remarks made in the original review.

Hendry's own account (and I may add that he is sincere in reporting his experience and impressions) makes it clear that the village of Khanh Hau is the type of showcase village, which has developed under governmental prodding in many underdeveloped areas. The location of a UNESCO-sponsored Fundamental Education Center in it had made the village "a popular place for official visitors from Saigon" (p. 272). One of the criteria established by the researchers in advance was that the village "be within reasonable commuting distance from the city" because "commuting from Saigon was inevitable" (p. 271). The reasons cited are "minor security incidents" and reluctance of local officials "to permit foreigners to stay overnight in the villages" (p. 271).

In this connection, it may be recalled that the period of 1958–59 was relatively more calm in Vietnam than its past or future. It may also be noted that this team of researchers was there with the blessings of the government. Obviously then, the researchers had either low capacity for establishing rapport or were opportunistic in the selection of the village. The latter impression is strengthened when one notes that "each member spent three to four days a week in the village and the rest of the time writing notes. *There were also other calls on the time of all members of the group for teaching, for administrative duties, and for other research carried on at the same time. The study of the rural community was the major research activity for all, but it was impossible to devote full time to it because of the nature of the other duties all were required to fulfill*" (p. 276) (italics mine). One wonders, during the eight months that the research was in progress, how much time Hendry spent with the villagers, especially when one notes that, except for some interviews in the beginning,

[2] Gerald C. Hickey, *Village in Vietnam* (New Haven, Conn.: Yale University Press, 1964).

his sample survey was mainly conducted by his two South Vietnamese assistants.

The questionnaire is so innocuous that Hendry did not even like to determine what the people were listening to on the radio or reading in the newspapers when he asked questions, "Do you listen to the radio?" or "Do you read a newspaper?" He was so anxious "to minimize questions which dealt with sensitive areas" that he has hardly any data on social attitudes, and yet the book is full of accounts of people's values and attitudes. A reviewer cannot be blamed for becoming skeptical of the whole study.

These and other serious shortcomings cannot be glossed over easily. I do not know how much experience Evers has in doing field research in the villages or of evaluating such research. I agree with him about the difficulties of doing research under present political conditions in Vietnam. But I do not share his attitude that this should interfere with the sociological vision or the methodological precaution which is necessary for conducting research even in a society "under stress." No doubt the tools of social research are not yet as advanced as one would like them to be, but the hundreds of social scientists engaged in cross-cultural research today have forged their way ahead under great odds and with satisfactory results. I cannot possibly say the same for the author of *The Small World of Khanh Hau*.

Cross-cultural research could be exciting and successfully executed in spite of the politics of the day in any country. I would refer Evers to my article, "Contributions of Qualitative Research for Macroscopic Quantitative Data,"[3] for an inside glimpse of this experience.

I hope Evers will join me in wishing social research to be scientifically significant rather than gratuitously opportunistic if it is to be of any value to the social scientist or the policy maker.

BAIDYA NATH VARMA

Columbia University

[3] Baidya Nath Varma, "Contributions of Qualitative Research for Macroscopic Quantitative Data," *Annals of the New York Academy of Sciences*, CXXXVI (1966), 523–46.

BOOK REVIEWS

Madness in Society: Chapters in the Historical Sociology of Mental Illness. By GEORGE ROSEN. Chicago: University of Chicago Press, 1968. Pp. x + 337. $5.95.

The author of this fascinating volume is a medical historian who applies a thoroughly sociological perspective in examining the social and cultural definition and the response to mental disorder over a long span of Western history. His painstaking scholarship and comprehensive documentation make this an impressive answer to many of the questions for which sociologists often seek answers.

The main portion of the book is a reconstruction of the social roles of the mentally disturbed, beginning with ancient Palestine, Greece, and Rome, and following developments in western Europe to the late nineteenth century. The basis for modern social psychiatry is isolated in the transitions from medieval Europe to the recent past. By meticulous and imaginative sleuthing in a variety of historical sources, Rosen formulates a convincing account for each era that includes the views of the nature and causes of mental disturbance of the era, the extent to which the presence of disturbed persons was taken for granted in the society, the general pattern of avoidance and tolerance and the attitudes of awe and hostility with which the disordered were regarded in society, and the systematic provisions for dealing with them. The author correctly indicates that he is not writing a history of psychiatry but an account of "the place of the mentally ill, however defined, in societies at different historical periods and the factors (social, psychological, cultural) that have determined it" (p. ix).

Rosen's approach can be nicely illustrated by citing a few of the recurrent themes that appear in his analysis. For instance, he documents the reappearance, throughout the period he reviews, of the "mad" role. There apparently was available, throughout the period, a "mad" role that could be evoked as the transitory response to unusual situations, deliberately simulated as a device to escape responsibility, or unwittingly learned as an appropriate feature of the role of prophet or divine. Another theme contradicts the assumption that modern enlightenment led to the conception of insanity as a form of illness. Rosen's historical evidence shows strikingly that treatment of the insane was accepted as the appropriate sphere of the medical practitioner in Greece and Rome and even to a limited extent in ancient Israel. Rosen shows that each era reveals the interplay between folk ideas of insanity as demon possession or as an essentially moral problem and more sophisticated and naturalistic ideas stressing physiological and, later, psychological causation. Another theme indicates that differential treatment by social class is a constant. Well-to-do families in every era provide private care of mentally disordered members while the poor are left to fend for themselves. Rosen also shows us that until quite recent times the mentally disordered were allowed to move freely about the community as long as they were not dangerous, although at the same time they were the constant butt of ridicule and harassment, especially by children. Rosen also documents how the development of modern conceptions of mental disorder depended upon the emergence in the late seventeenth century of a wholly new view of human nature, centering about the introduction of the idea of a personal self.

Two chapters deal in a comprehensive manner with collective psychopathology and psychic epidemics. Although these chapters rely somewhat more on familiar secondary sources than does his study of the role of the mentally ill, there is nevertheless enough that is original in Rosen's interpretations to warrant study by students of collective behavior. Again, the idea of a social role that is learned as the appropriate adaptation to certain stressful situations is central in his thought. Attempts to explain dancing manias, witchcraft delusions, and similar phenomena by assumptions of individual psychopathology are repudiated. He stresses the close ties between such collective behavior and religious ritual. Furthermore, "the bizarre behavior exhibited in dance frenzies and similar phenomena is instrumental. Such behavior may represent a critique of existing conditions, magical efforts to change these circumstances, symbolic

validation or expression of frustrated goals and repressed guilt, or transcendental compensation" (p. 224).

A final section of the book consists of three essays on more specific topics. A brief chapter on the psychopathology of aging and another dealing with some specific types of mental disorder are followed by a more extended essay reviewing the history of public health concern with mental illness.

Only a reviewer who is equally familiar with the wide range of primary documents so resourcefully employed by the author would be in a position to assess the accuracy of his inferences. But insofar as a sociologist interested in collective behavior and the social role of the deviant can judge, this volume is a superb work of historical scholarship, informed by a sociological perspective which would be a credit to any professional sociologist. The lucid and interesting style of writing also makes this excellent material for assignment to students in classes on deviance, the sociology of mental health, and collective behavior.

Ralph H. Turner

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Prestige and Association in an Urban Community: An Analysis of an Urban Stratification System. By Edward O. Laumann. New York: Bobbs Merrill Co., 1966. Pp. 218. $2.95 (paper).

This book is a revised version of the author's doctoral thesis. It is a fine exposition of an empirical survey that addresses theoretical questions important enough to deserve this wider distribution as a book, although the revision still bears some of the marks of graduate school culture: pedantic prose and conceptual caution.

Using a probability sample of interview responses from 422 adults in Cambridge and Belmont, Massachusetts, Laumann attempts with exceptional methodological resourcefulness to describe the structure of an American urban stratification system; to examine the relationship between macro- and micro-dimensions of the structure; and to consider the bearing of status-based attitudes on political and economic opinions. Chapter vi, a modified version of a paper by Laumann and Louis Guttman that appeared in the *American Sociological Review* (April, 1966), demonstrates the author's technical ingenuity to best advantage in its utilization of "Smallest Space Analysis."

This ingenuity helps resolve some of the issues clouding stratification theory and research in previous studies of urban communities. The technical refinements enable Laumann to go beyond verbal concepts of stratification on the one hand and valuable but strictly macro-structural analysis in the ecological tradition on the other. Having measured and graphed responses indicative of prestige and association with greater precision and conceptual relevance than other students of the problem, Laumann is then able to get on with the more fascinating topic of attitudinal linkages.

For the Cambridge-Belmont residential area, he is able to demonstrate that "class-like features best characterize the situation at the extremes of the occupational hierarchy, while a more fluid, differentiated situation obtains for the middle levels. . . . Yet . . . the middle levels are not completely unstructured." What is exciting is the precision with which this and other regularities are identified.

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The Sociology of Max Weber. By Julien Freund. Translated from the French by Mary Ilford. New York: Random House, 1968. Pp. ix+310. $6.95.

Twenty years ago this book would have been cheered: a useful supplement to the fragments of Weber's sociology that were then available in English translation and to the purposely truncated exposition of his ideas in Parsons' *Structure of Social Action*. The case for its utility at present is hard to make, now that nearly all of Weber's sociology has been translated and exegeses of such high caliber by Aron, Bendix, Martindale, Nisbet, and Roth supplement those by Parsons, Gerth, and Mills. Like Spykman's book on Simmel, it could have served as a stopgap; but why spend the time, or introduce students *that* way, when the real thing is available?

For those who seek a straightforward textbook on Weber, Professor Freund's volume can be commended as reasonably comprehensive, clear, and faithful. The first half presents Weber's main methodological ideas and his "vision

of the world," the latter half a circumspect selection of his ideas from the sociologies of economics, religion, politics, law, and art. The chapters on political and legal sociology contain some of the least frequently reworked elements of Weber's thought. And, in the section where Weber's concept of the "nation" is construed as "primarily the expression of power based on the passion for prestige," it is interesting to be reminded, that Weber covered essentially the same ground Rupert Emerson was to cover four decades later, in greater detail, of course, but with scarcely more analytic success. Only the chapter on Weber's putative sociology of art and technique is contrived and unconvincing.

The value of this book as a reference work is limited by its dimunitive index—only three pages—and by the fact that, while the author appropriately draws on diverse parts of Weber's writings to deal with each particular topic, these shifts in reference are not footnoted. This becomes particularly inconvenient where he sets forth the basic categories of social action, presenting the definitions from the 1913 *Logos* paper alongside the *Wirtschaft und Gesellschaft* concepts as though they were all of a piece.

A small number of misleading statements in the text should be noted. "Reciprocity" is *not* a "basic and characteristic element in Weber's thinking," nor does the ideal type equal "the sum total of concepts which the specialist in the human sciences constructs." The author's *validité de la légalité* is mistranslated as "that which is legitimate is valid." It is misleading to say that Weber regards interpretative sociology to be a mere adjunct to mainstream sociology, for Weber himself wrote that he considered the interpretative study of social action to be "decisive for [sociology's] status as a science." It is debatable that Weber was "the first in practice to place sociology on a strictly scientific basis" and not the case that Karl Mannheim was a student of Weber's, although he was of course greatly influenced by Weber's writings.

Freund's chief editorial contribution is to point out occasional gaps in the typologies—notably, Weber's neglect of the category of "religious reformer" in the *Religionssoziologie*. He also calls repeated attention to Weber's illustration of the notions of ideal types, the paradox of consequences, and ethical neutrality—the last with annoying frequency. For the most part, he follows his announced policy of refraining from "personal interpretation." This is regrettable, in view of the author's obviously thorough knowledge of the Weberian corpus. At any rate, his book is doubtless meeting a more urgent need for straight Weberian exposition on the Continent.

DONALD N. LEVINE

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To Make My Name Good: A Reexamination of the Southern Kwakiutl Potlatch. By PHILIP DRUCKER and ROBERT F. HEIZER. Berkeley: University of California Press, 1967. Pp. 160. $5.00.

In a bit of understatement, the final line of this book states that the potlatch "was, after all, a rather peculiar social institution." Efforts to make this native institution of the northwest coast appear less peculiar and more intelligible have engaged the ingenuity of several generations of eminent social scientists. Indeed, one could write an encapsulated account of modes and styles of social scientific theoretical orientations through refracted interpretations of the potlatch.

The basic ethnographic details of the potlatch were first presented to the scholarly world by Franz Boas in a series of publications commencing in the 1890's. While Boas did have occasion to witness several potlatches first-hand, the bulk of his data derives from informant testimony—particularly from an Indian named George Hunt, who collected reams of material at Boas' urging and mailed them to New York. Given the data of collection and Boas' scrupulous attention to detail, this body of material can never be matched. Nevertheless, Boas failed to provide a theoretical model sufficient to resolve discrepancies or fill lacunae in the data or to provide a clear image of "how the system really worked."

Marcel Mauss, in his seminal work *The Gift*, utilized ethnographic material on the potlatch to support his generalized model of reciprocity. The potlatch was seen as a form of gift exchange that ultimately functioned to bind together different groups and individuals. The profound ideas expressed in *The Gift* may be considered as lineally related to much of contemporary French structuralism, alliance theory, and transactional analysis. However, as applied to the potlatch, the necessary simplification of the model has probably generated as many problems as it has answered.

Ruth Benedict's portrayal of the Kwakiutl, based on Boas' data, in her widely read *Patterns of Culture* provided the principal path by which the potlatch penetrated the "pop culture" of social science. This picture, much of it overdrawn and even erroneous, underwent further distortion in introductory courses, through uncritical textbook ctiations and through the futile imagination of instructors who saw in potlatch giveaways and property destruction a parody of industrial capitalism. More essential in Benedict's approach was her attempt to draw cultural configurations coeval with specific psychological types. The Kwakiutl, in her shorthand designation, were characterized as possessing a megalomaniac paranoid trend (Theodore Schwartz has suggested that "paranoidoid" might be a more suitable label), and the potlatch was regarded as the key institution through which these behavioral qualities were expressed. Let he who is not stoned castigate Benedict's general mode of analysis or her specific interpretations of the Kwakiutl. Despite the horde of sniping critics she has attracted, her work endures as significant in the development of psychological anthropology and as influential in reinjecting humanistic values into anthropological description.

Several contemporary scholars have contributed notably to our understanding of the potlatch. Homer Barnett, on whom the present work under review leans heavily, stressed the status-defining attributes of the potlatch. Helen Codere, who carefully scrutinized Boas' published and unpublished material and did fieldwork of her own, demonstrated that the elaboration of the potlatch complex was a late phenomenon resulting from trade goods and surplus wealth made available by white contact; she also mollified Benedict's stereotype by pointing to the amiable side of Kwakiutl life. Wayne Suttles, Andrew Vayda, and S. Piddocke have tried to interpret the potlatch from an ecological point of view by perceiving the potlatch as a device for redistributing, through affinal links, resources unevenly distributed in nature.

The present volume builds upon past interpretations, contributes new primary data, and seeks to reinterpret the nature of the potlatch complex. The new material was collected by Philip Drucker among the Southern Kwakiutl (the same group to which most of Boas' data pertain) in 1937 and 1953 from two major informants (one of whom was the subject of Clellan Ford's life history, *Smoke from Their Fires*). The basic thesis underlying Drucker and Heizer's reinterpretation is that the potlatch system did not give or create new social positions but defined or reinforced the existing status system. To put the matter more directly, the potlatch was not an institution for status mobility; chiefs gave and attended potlatches because they were chiefs, and such activity was their prerogative and duty. Also, as the authors point out, the potlatch served to define lower, non-chiefly ranks, a fact obscured in the literature by the almost exclusive attention accorded the extravagant displays of chiefs.

This thesis is examined through discussion of several critical issues involved in the Southern Kwakiutl potlatch. The chapters on the postcontact development of the potlatch stress not only the importance of newly introduced forms of wealth, means of securing wealth, and methods of accounting, but also point up institutional changes that ensued when the potlatch was driven underground by the suppression of government agents and missionaries. Perhaps the best chapter in the book deals with the question of double return on potlatch gifts, a frequent stumbling block in previous analyses. The authors document their contention that double return was anomalous to Kwakiutl custom and was rarely practiced. Another chapter emphasizes the superordinate role of the council of chiefs in controlling potlatches and in serving as final authority in authenticating claims to status. Also discussed in separate chapters are rivalry potlatches (engaged in by different claimants to a hereditary position), rivalry gestures (reactions to real or imagined insult and conceptually distinguished from rivalry potlatches, though often acted out in a potlatch context), and the mortuary potlatch, which functioned to legitimize the rightful heir to the potlatch position of his deceased relative and to resolve any succession crisis. Drucker and Heizer also imply that the mortuary potlatch is prototypical to the other forms of the potlatch. Another chapter is devoted to criticizing Codere's hypothesis that the potlatch served as a war surrogate. While conceding that much of the verbal symbolism surrounding the potlatch had reference to war, Drucker and Heizer argue that this was a conscious metaphor representing figurative analogy rather than direct homology, since the ul-

timate function of the potlatch was socially integrative rather than divisive.

The "Conclusions" recap the main arguments of the book and introduce some new considerations. Given the assumption that the potlatch was not a mechanism for status mobility, the motivational mainsprings that made the system go demand explanation. As partial attempts to fill the motivational void, Drucker and Heizer briefly mention prestige and esteem accruing to adequate role performance and self-generating "gamesmanship" motives. These explanations are doubtlessly part of the story, but seem insufficient to account for either the excessive affect associated with potlatch behavior or the tenacity of the institution in the face of an erosive acculturative situation. In their treatment of possible functions and origins of potlatching, the authors dismiss too facilely the redistributive theories of the cultural ecologists. By emphasizing the area's absence of direct subsistence pressures that might necessitate redistribution, they thereby underestimate the possible significance of acquired appetitive wants that might serve equally well in stimulating intergroup exchanges. Disavowing economic determinism, Drucker and Heizer see the genesis of the potlatch as possibly deriving from a coalescence of functionally compatible customs. Starting with marital gift exchange that could develop into enduring relations between affinal kin, they posit the consequent emergence of a wealth system, hereditary rights associated with social status, and formal confirmation of these rights at mortuary ceremonies. This plausible, but unprovable, hypothetical sequence does not, as should be obvious, rule out economic factors as decisive in the development of the institution.

This book, then, contributes some valuable new data and sharpens and resolves some of the past controversies surrounding the potlatch, but falls far short of offering a final explication of the theoretical problems involved in understanding this "peculiar institution."

RAYMOND D. FOGELSON

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Religion and Society in Tension. By CHARLES Y. GLOCK and RODNEY STARK. Chicago: Rand McNally & Co., 1965. Pp. xii+316. $6.00.

Ever since Weber's day, sociologists have busied themselves researching and refining Weber's typologies of religious institutions without radically restructuring them. The present volume is a strikingly successful attempt to build a new typological structure of religious beliefs, practices, experiences, and institutions as a suitable starting point for scientific research on religion in the United States in the twentieth century. Recognizing that we are an increasingly secular society, the authors include humanism as a major type of "value orientation." Religion has a "supernatural referent" and humanism has none, but humanism commands a section parallel to religion in each theoretical chapter. Religious and humanist institutions are classified according to (1) formal organization or lack thereof and (2) differentiation from other social institutions. The authors then examine the emergence and transformation of different types of institutions and the consequences of contact between them.

Turning to the individual's religion, the authors suggest five "dimensions" under which they categorize religiosity for purpose of research: the experiential (feeling), ritualistic (practice), ideological (belief), intellectual, and consequential (ethical). This reviewer suggests that some of these terms could be simplified and that others overlap, but in terms of the cross-cultural research advocated by the authors, these five dimensions appear to be a good set of "operational definitions of religious involvement."

In presenting a "taxonomy of religious experience," the authors make the first substantial conceptual contribution to psychology of religion since William James's *Varieties of Religious Experience*. After defining religious experiences as occasions felt to be encounters with the supernatural, the authors outline four major types of experiences: the confirming (the most common type in America—a simple feeling of divine presence), the responsive (with salvational, miraculous, and sanctioning subtypes), ecstatic (very rare in America), and revelational (rarest of all). (There are four corresponding types of diabolic experiences.) These four general types may be ordered on the basis of complexity, frequency, and distribution within populations, and they supply an excellent working conceptual model for the researcher in psychology or sociology of religion, much more readily usable than James or Leuba.

The real brilliance of this book lies in the conceptual framework constructed in Part I. The authors continue for another 200 very readable pages to deal with specific questions, like the existence or non-existence of a religious revival in America (they conclude that "none of the work done to assess the state of religion in America meets even the minimum standards of scientific inquiry," particularly since each sociologist has used the term "religiousness" with a different meaning), the religious beliefs of church-going Americans (they conclude that the term "Protestants" is a statistical fiction, divisible into at least five generic camps, and cannot be compared with "Catholics"), and finally the parish church (conclusion: the minister's pastoral and preaching roles are most important to the parishioner, but neither the church nor the sociologists can say *what* preachers communicate to fifty million people every Sunday, or with what effect, or to whom). A later chapter on "The Social Context of Religious Experience" indicates that many religious experiences result from compliance with social norms.

Part III deals with religion and social change. The American churches are currently at the receiving rather than the contributing end when it comes to the creation of values; such values as they do promulgate are primarily harmony and opposition to change. Studies of political party and church membership in the United States, Britain, and France show that the churches are, increasingly, organizations comprised of conservatives and members of the upper and middle classes. The radicals, denied religious legitimacy, are likely to construct an alternative value system which for them takes the place of organized religion. The working classes show the greatest degree of religious alienation. Finally, in accordance with Weber's deprivation theory, the authors analyze different types of deprivation (psychic, economic, organismic, ethical, and social) as instigating the origins of corresponding types of religious groups (cult, sect, healing movement, reform movement, and church); each type of deprivation produces a different form of institution. A final section analyzes the behavioral conflicts between the natural and social sciences and religion.

This book is brilliant in its conceptualizations, which take the reader far beyond Weber and Troeltsch and which outline excellent suggestions for much-needed research projects in the field. It is a must for all serious sociologists of religion.

DOROTHY (CORBETT) WERTZ

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Les rôles masculins et féminins. By ANNE MARIE ROCHEBLAVE-SPENLÉ. Paris: Presses Universitaires de France, 1964. Pp. viii+364. Fr. 30.

This is the second of two volumes by the author dealing with role theory and its implications; the first, *La notion du rôle en psychologie sociale*, was published in 1962. Together they constitute the two dissertations, *principale* and *complémentaire*, required for the most advanced doctorate, the *doctorat d'état* at the University of Paris. Even a quick glance at these two publications indicates why this doctorate, a necessary prerequisite to being named full professor at a French university, demands so much more time and effort than does the usual Ph.D. in sociology or social psychology in the United States.

This study of masculine and feminine roles in contemporary Western society consisted of three distinct but interrelated empirical investigations. The first of these dealt with the stereotypes held by French and German men and women students about men and women, respectively, using the familiar checklist questionnaire (121 adjectives). The total of 245 subjects, all university students of psychology, were fairly equally divided between the two nations and the two sexes. One of the hypotheses tested was to the effect that Germans would make a more clear-cut distinction between "male" and "female" characteristics than would the French, somewhat along the lines of the Nazi *Kinder, Küche, Kirche* as the proper spheres of women's activity; the results went in the opposite direction! The author indicates a variety of specific but relatively unimportant differences between the two national groups, as well as wide individual variations in the responses, but there was fairly general agreement (as in comparable American studies) in ascribing traits of "dominance" to men and of "submission" to women. Both men and women subjects in both countries credited men with the possession of more approved or desirable traits (independently classified by a group of judges), and the au-

thor suggests that women are here reacting very much as do members of ethnic minorities.

The second investigation consisted of interviews with husbands (all engineers, to insure at least some degree of comparability) and their wives in France, Germany, and England; there were twenty-four pairs of subjects in each country. The questions this time were designed to determine the actual behavior of men and women, respectively, in the home in relation to children, to household chores, and to patterns of authority. Once more, individual variations were marked in all three countries, but there was definitely a greater tendency in England to stress a democratic partnership between men and women and a greater willingness on the part of men to participate in tasks which had previously been regarded as typically feminine; the Germans laid greater stress on masculine dominance and authority and on the idea that it was unreasonable to expect a man to become involved in domestic tasks. In fact, a number of German men referred critically to the American way of life, in which the man becomes a slave or a nursemaid, taking over all the household duties! The French occupied an intermediate position in this regard, but closer to the Germans than to the English.

The author makes a number of interesting points in this connection. In the first place, the role pattern is changing in all three countries, with much more equality now than that reported by the subjects for their own parents. Second, the description of actual behavior shows much more equality than is indicated by the stereotypes, and it is suggested that the latter are examples of a "social lag" which has not yet caught up with a significant social change. Third, the individual variations in accepted masculine and feminine roles are striking in all three countries, and all generalizations about *the* German or *the* English family fail to reflect the true situation. This last statement may seem self-evident, but it is important enough to deserve occasional re-emphasis.

The third investigation was an intensive clinical study of five cases of "intersexuality," of true hermaphrodism or pseudohermaphrodism. This was done in the hope that an understanding of interpersonal conflict with regard to one's own sex role might throw additional light on the nature of sex roles in general. This hope was not realized. The individual cases are interesting but too idiosyncratic, differing so much from one another that no generalizations are possible.

American sociologists interested in sex roles should find this volume of considerable interest. To some extent it duplicates what has already been done in the United States, but it deals with other populations—French, German, English—and therefore adds a significant cross-cultural dimension. The study, as the author herself indicates, might have been more interesting if the cultural differences had been greater, with subjects from Asia or Africa rather than Europe, but even so, the comparisons are worth making. The discussion of methodological problems is honest and sophisticated, raising pertinent critical questions about the research, the sample, the translation of terms from French to German, the precise meaning of the responses, and the relations to previous investigations. There is a useful bibliography of approximately three hundred titles, the majority in English, but with French and German publications amply represented.

Otto Klineberg

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La réussite sociale. By Alain Girard. ("Que sais-je?" Series, No. 1,277.) Paris: Presses Universitaires de France, 1967. Pp. 126.

This is a modest volume which summarizes the basic findings of an earlier monograph on social mobility in France and compares the results with other French and foreign studies. The "Que sais-je," series is oriented toward a general public, and the professional sociologist would do well to refer to Girard's earlier monograph (*La réussite sociale en France* ["Institut national d'études démographiques," Cahier No. 38; Paris: Presses Universitaires de France, 1961]), which appeared soon after the summary studies of Lipset and Bendix and Miller and has been somewhat neglected in both France and the United States.

The original data collected, in conjunction with the Institut National des Etudes Démographiques, are drawn from several of the most important French elites. Questionnaires were sent to persons listed in the *Dictionnaire biographique français contemporain* and to recent graduates of four *grandes écoles*. Analysis of the biographical articles in the *Petit La-*

rousse provided a basis for historical comparisons, which were supplemented by the studies of Odin and de Candolle. Also included are abbreviated studies of the social origins of *inspecteurs de finances*, Ministry of Foreign Affairs personnel, and graduates of the École Nationale d'Administration.

Generally, information was collected on date of birth, geographical origins, parents' occupations and education, size of family, birth order, and respondent's education. Such limited data restrict the scope of a study, and the merits of both volumes are clear organization and presentation rather than sophisticated analysis.

The most interesting results of mobility studies generally derive from comparisons across institutional sectors, national boundaries, and over time. Although the 1967 volume partially corrects the disappointingly small number of comparisons in the 1961 monograph, even these comparisons are often crude and unsystematic; the volume could have been easily strengthened considerably by confronting the results of other review studies with those of the 1961 monograph.

A number of interesting findings are reported, however, particularly those from the 1961 monograph. The high degree of inbreeding within certain institutional sectors—especially the military, the diplomatic corps, and the higher civil service—is carefully documented. Also suggestive is the relatively even distribution of geographical origins of *recent* elite members and the definite preponderance of the northern and eastern regions (especially Alsace and Lorraine) as birthplaces for *parents* of present elites. This last finding hints at the disproportionate number of Protestants and Jews among French elite groups, but, unfortunately, following the tradition of the French census (whch may have been mandatory for INED), no questions on religious background were posed. Since about 1800, Paris, with less than 10 per cent of the population, has produced roughly one-third of all elites. In social recruitment for big business, the military, the free professions, and the higher civil service, in France and abroad, Girard argues, a "two-thirds rule" holds: approximately 60–75 per cent of the members of these groups derive from *"la classe supérieure ou la classe possédante"* (p. 91)—a vague formulation at best.

Still, the shortcomings which mar the work for the professional—but less so for its intended audience—should not be allowed to detract from the significance of the findings contained in the 1961 volume. This monograph should enjoy a long life as a valuable reference for future students of elite recruitment.

TERRY N. CLARK

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The English Public Schools. By IAN WEINBERG. New York: Atherton Press, 1967. Pp. xix+250.

This book is about the contrarily named English public schools, that is, "prep," "private," or "independent" schools as they are variously called in the United States. Borrowing from Goffman's treatment of total institutions, Weinberg traces the historical development of the public school and how it has retained its unique position in contemporary English society. Although educational and cultural traditions of the public school are sensitive to external pressures, the author finds evidence of stability and conservative forces in operation.

To document his case, Weinberg relies largely on public and private records, plus the responses of headmasters to a short mail questionnaire. He undoubtedly relies, too, on his own experience as an English public school boy, which, except for a short note inside the book's jacket, is never acknowledged.

The author employs what appears to be just the right mixture of conceptual abstraction, with references to the appropriate literature, and descriptive data. The combination results in an orderly, interesting, and easily read book. Chapter titles illustrate the nature of some of the topics covered: "The Public School and the Elite," "The Boys and the Total Institution," "Careers and the Schools," "Public Schools and Parents," and "Stability and Change."

Much like its American counterpart, the English public school serves neither radical equalitarianism nor the meritocracy. Instead, it primarily serves an elite of blood who use social privilege to insulate their sons from the mass and to guarantee their own intergenerational replacement. As custodians of elite norms and values, the public school also is the pivotal point which insures that a significant number of elites share a common perspective.

More importantly, Weinberg explains *how* the public schools have survived despite the declared objective of the Labor government to eliminate them. One reason, among several, is that many of the public schools not only have been able to retain their technical superiority over competing forms of education, but they are the only schools whose total institutional structure makes it possible to socialize schoolboys to *act* like elites in the British context.

Weinberg generally avoids direct comparisons with the American prep school. However, the American reader might draw a number of parallels and distinctions. Private secondary schools in both countries, for instance, seem to be moving away from their classical curriculum with its overriding emphasis on the humanities. Yet, both still emphasize moral responsibility, Christian leadership, and ascriptive norms of self-conscious elitism. Both, too, now appear to educate a substantial number of the children of successful meritocrats and other members of the *nouveaux riches*.

Private education in England and the United States differs in other respects. The "prefect" system and "fagging" are distinctively British and reflect their concern with rules and discipline. The English public schools have shown far less receptivity to schemes such as our Independent School Talent Search program, to change recruitment policies which have long been a major source of strain. Also, in contrast to the English schools' strong orientation to the civil service and anticommercial values, American prep schools have produced relatively few political leaders.

The only serious reservation I hold for this book is whether Weinberg has always interpreted his data correctly. To give one example, I found his statements regarding a "dramatic shift to business" (p. 152) unconvincing, since the data he presented (p. 149) showed a fairly steady over-all *decline* in the proportion of public school graduates entering industry and commerce in recent years.

Nevertheless, this is a rewarding piece of work and is particularly topical in light of current upheavals in other forms of secondary education in Britain.

BRUCE K. ECKLAND

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Political Parties and the Canadian Social Structure. By FREDERICK C. ENGELMANN and MILDRED A. SCHWARTZ. Scarborough: Prentice-Hall of Canada, 1967. Pp. 277. $5.95.

Sociology and the behavioral approach to politics have yet to make extensive contributions to the study of Canadian politics. Though there is an increasing number of people studying Canada's political problems from these vantage points, the amount of published material remains relatively small.

This book, by Engelmann, a political scientist, and Schwartz, a sociologist, though in the form of a general text on Canadian political parties and not based on any immediate empirical research, constitutes an original contribution to the understanding of the functioning of the Canadian political system from these vantage points. Some might think that such an endeavor is premature, given the very small amount of monographic work the authors could rely on to write this text. And indeed, the basic weakness of the book is precisely linked to that lack of primary material, something the authors can in no way be held responsible for. Concerning many of the problems they raised in this book, there was literally no published research they could build on (e.g., the impact of the mass media on parties), and their account remains perforce relatively superficial at times.

But I personally think that such a book need not be written only to provide a synthesis on a given subject; it can also stimulate research in areas heretofore neglected. By providing an original conceptual framework for the analysis of Canadian parties, which departs from the approach usually found in such books, the authors will certainly open the eyes of political scientists and sociologists to avenues of research they had not thought of before.

This approach, simple as it is, is interesting because of the wide net it throws on social and political aspects of society likely to affect or be affected by political parties. The first part of the book examines a large set of factors which influenced the formation, the development, and the configuration of Canadian parties, moving from large societal influences (mainly the characteristic traits of Canada's social structure) to more specific ones (mass media, elites, interest groups, system of government). The chapters on social structure

and interest groups (as well as the summary chapter of that section) appeared to me as the most rewarding chapters. The second part deals with the political and social consequences of Canadian parties, starting with the effects on the political system (determination of leaders and the decision process) and moving to more widespread influences on public opinion and on social consensus and conflict. These last two topics were the most thoroughly examined.

The book also develops at the beginning a general typology of parties which is referred to occasionally in subsequent parts and which is used at the end primarily to offer a summary classification and interpretation of Canadian parties. The lack or weakness of most of the theoretically possible types other than the broad-based cadre parties of electoral success is in itself a clear indication of the social and political constraints on the development of parties in Canada. One wishes the authors had devoted more space to the analysis of this empirical configuration; in particular, the impact of the electorate in this regard is something we have learned a great deal about in voting studies, but is not dealt with extensively in the first part of the book (e.g., the role of ideology in the masses, the lack of political sophistication and involvement, etc.).

Yet, Engelmann and Schwartz have opened many avenues, and the first is that the literature on Canadian parties cannot but point the ways to more intensive studies in the future.

MAURICE PINARD
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Post-War Immigrants in Canada. By ANTHONY H. RICHMOND. Toronto: University of Toronto Press, 1967. Pp. 320.

Few scholars or even government officials have been interested in the immigrants who have returned home. And no one prior to Anthony Richmond has thought of using this return flow to upset our implicit if not explicit assumptions of the inevitability of the need to assimilate immigrants on this continent in such a way that they will turn into 100 per cent Americans or Canadians.

Richmond shows that the rate of return and remigration has been consistently high among all postwar immigrants to Canada, actually, nearly 25 per cent of those who migrate. This might be interpreted as an indication of Canada's inability to absorb new people in a satisfactory way, but, through his particular attention to those who have returned to the United Kingdom, Richmond makes a much more profound analysis of this trend. He proposes that the new "quasi-migrants" must be distinguished from the semipermanent or long-term migrants. Immigration policies in the past have been based on these two latter categories of migrants, who have been seen as permanent residents who must be assimilated to fit into the work and life of the new country. However, as all countries are now becoming highly industrialized, they are facing similar occupational needs. So, trained blue and white collar workers, business executives, and professionals are now able to move more and more freely around the world. One of the obstacles to this movement is still language, and this is probably why Richmond found that the highest rate of return from Canada was that of Americans, followed by immigrants from the United Kingdom and the other Commonwealth countries.

Supporting his idea of the trend to the "quasi-migrant" is the fact that 41 per cent of the returnees had not originally intended to settle permanently in Canada and that 29 per cent of those who went back to the United Kingdom were seriously considering going back to Canada after a short stay in Britain.

Other data supporting Richmond's analysis was that it was precisely the immigrants who were *most* satisfied with their experiences in Canada who returned home. They had fitted more easily into the occupational structure than other immigrants, as they were employed more quickly on arrival, and had also found jobs that were commensurate with those they had left in England. Their backgrounds showed better education and a higher level of training than those of the other immigrants.

Richmond claims that this new type of migration is quite different from the early pioneer movement of people to this continent, which was essentially a movement in one direction. It is much more like the recent movement of people from city to city within all highly industrialized countries. (In the five years preceding the 1961 census, 45.4 per cent of all Canadian residents over five years of age changed residence. Similar movements occurred in the United States and in England and Wales [p. 267], so the Canadian pattern

forms "part of a growing body of inter-urban migrants for whom distance and international boundaries were not major obstacles to movement" [p. 266].)

Besides this refreshingly new perspective on present-day migration, Richmond adds to our knowledge of this field by describing the economic and occupational background of migrants to Canada in the postwar years; the occupations and standards of living they achieved in the new country; the effect of family and kin support; the degree to which ethnic associations assist the process of assimilation; and their attitudes toward Canada.

Richmond sees this new trend to a global "floating population" as a movement that "makes possible the allocation of human resources in a way which is not only the most productive economically but which also enables the individual to achieve his own personal values and fulfill his goals with the minimum of frustration" (p. 269).

The significance of this new trend for the countries which will continue to need immigrant labor lies in their ability to adjust to this new kind of population. Its significance for the sociologist is that such well-used concepts as assimilation and integration may lose much of their sociological connotation.

This study is highly recommended, not only in its own right, but as one that opens up exciting new vistas and areas of research in that highly important field of ethnic relationships.

AILEEN D. ROSS

McGill University

Max Scheler: An Intellectual Portrait. By JOHN RAPHAEL STAUDE. New York: Free Press, 1967. Pp. xv+298. $6.95.

This is an unexciting book about an exciting personality. It suffers from its stated purpose: "to explain the development of Scheler's social and political ideas" (p. viii) and from the fact that it was written by a sociologically oriented historian. Scheler was not predominantly a politician, nor was he a sociologist interested in the manipulation of social reality. He was a speculative philosopher who sometimes applied philosophy to sociological theory. Only a philosopher could have done justice to Scheler's ideas on ethics, phenomenology, and metaphysics.

The author gives a detailed account of Scheler's life and presents Scheler's ideas within their biographical context. Scheler's opinions about comtemporary politics are overemphasized, while ideas expressed in his monumental "Der Formalismus in der Ethik und die materielle Werkethik" (1916) are insufficiently presented. However, chapters vi and vii give a detailed account of Scheler's later thoughts on metaphysics and on the sociology of knowledge. The author mainly tries to explain Scheler's thought as a result of the German political situation, especially during the 1920's, and as a reflection of Scheler's personal life. The greatest merit of this book lies in the fact that its author had access to an unpublished autobiography of Scheler's second wife (Maerit Furtwängler) and to many unpublished letters of Scheler to this second wife, and that he interviewed extensively Scheler's wives, relatives, and many friends. Professor Staude is right when he sees a close interrelation between Scheler's changes of thought and Scheler's personal life. Thus, Scheler's divorce from his second wife and his break with orthodox Catholicism are certainly connected, but the author should have learned from Scheler that personal substructure only releases ideas, and determines neither the content nor the validity of thought.

Staude's tendency to present Scheler as a political opportunist who changes his political philosophy in order to support the powers that be seems exaggerated. Karl Mannheim believed that it is to Scheler's credit "that he was more alive and more related to the present than others . . . and that he felt an obligation to react to the new intellectual realities of the world" and that "he was not satisfied with a once established relationship between eternity and contemporary history and that this forced him to try to master the new factors in the world" (*Wissensoziologie*, ed. Kurt Wolff [Berlin & Neuwied, 1964], p. 335, trans. mine). Scheler was basically a conservative elitist. This has motivated him to be in favor of "law and order" and in favor of the existing power structure (an attitude which is shared by the majority of today's academicians), but his antagonism against the individualistic, competitive, bourgeois middle-class ethos is not the essence of what is lasting in his ideas.

The peculiar bias of the author is clearest in the statement that "the story of Max Scheler's intellectual and personal life is one of a series of failures" (p. 254). By what standards can one

declare a man a failure who was professor at the best German universities of his period, who was at times the primary spokesman for German Catholic intellectuals, who fascinated the German public with his lectures (p. 217), who lectured to high Army officers at the request of both a former Reichschancellor and the Minister of the Army (p. 225), who was invited to lecture all over the globe, including the U.S.A., Russia, and Asia, whose "intellectual achievements were impressive" (p. 255), and who counted among his disciples a galaxy of famous German and French intellectuals (see the enumeration on pp. 255 ff.)? Is he to be considered a failure because he was torn by inner conflicts? But so were many great men, from Augustine to Freud. Was he a failure because he was married three times, because his divorces were accompanied by deep inner crises, or because he remained on close terms with his second wife even after he had married his third one? Or because he never held a professorship for very long due to his turbulent eroticism and his Faustian personality?

In a widely used German textbook on contemporary philosophy Scheler is called "an unusual personality, undoubtedly the most brilliant thinker of his time. . . . In the field of ethics his achievement is definitely the most outstanding and most original one of the first half of the 20th century" (I. M. Bochenski, *Europäische Philosophie der Gegenwart* [2d ed.; Bern, 1947], p. 151, trans. mine).

In addition to ethics, Scheler's contributions lie in the fields of metaphysics, philosophical anthropology, and sociology of knowledge. To characterize a complex system of thought by an oversimplified formula: Scheler tried to synthesize the absolute and the relative, essence and existence, in all these fields. This always was, and should still be, the aim of all philosophy, contemporary positivistic reductionism notwithstanding.

In the field of ethics, Scheler tried to establish a "materiale Wertethik," an ethics which does not abandon the idea of an absolute and which assumes that it can be grasped by intuition, which he restored as a valid source of knowledge.

In metaphysics, Scheler followed the Platonic tradition, retaining the belief in the reality of a realm of essences which are accessible through eidetic intuition (Husserl's *Wesensschau*). He philosophized about the ground of being in which he found the union of two fundamentally opposite, but dialectically interrelated, principles, *Geist und Drang* ("spirit and drive") whose gradual interpenetration forms the content of human history.

Scheler's sociology of knowlege centers around the interrelation of *real* and *ideal* factors, thus again reflecting his basically dualistic approach: ideal factors belong to the realm of essences and are immutable; their actualization in history, however, is relative and depends on the real factors, Scheler's substructure. He found them, not like Marx in the relations of production, but in certain psychological categories elaborated in his doctrine of drives: those rooted in family and clan relationships, in the will to power, but also in the profit motive. Thus Scheler prevented the surrender of the sociology of knowledge to total relativism, although he attributed to the real factors the power to release or to obstruct the realization of ideas.

In spite of the fact that he seems to reject most of Scheler's ideas as rationalizations of his political views, Staude has rendered a service to American sociology by telling the little-known story of Max Scheler (the bulk of Scheler's work, including his writings on the sociology of knowledge, still awaits translation into English). One may express the hope that this book will stimulate other studies on Scheler, hopefully by students who are more in sympathy with Scheler's ideas than Professor Staude.

WALTER A. WEISSKOPF

Roosevelt University

Rites of Modernization: Symbolic and Social Aspects of Indonesian Proletarian Drama. By JAMES L. PEACOCK. Chicago: University of Chicago Press, 1968. Pp. xxv+306. $11.00.

Modernization can be defined in terms of a shift in ideas and values, and drama is usually regarded as a powerful medium for the expression of new and revolutionary ideas. How does the theater work as a channel for ideas with the potential to change people's behavior? What is the role of the theater in the process of modernization?

James Peacock has attempted to answer these questions in his study of *ludruk*. One of the many kinds of theater found in Java, *ludruk* is performed by and for the urban

proletariat of eastern Java. Peacock's analysis illuminates the interplay of form and content, of substance and circumstance, and of theater and society. He constantly stresses that the effect of the theater depends on the form in which ideas are expressed as much as on the ideas themselves. Both form and content vary with the nature of the audience and the need to survive in a competitive atmosphere. *Ludruk* is both a model of Javanese society and a model for it; action on the stage is therefore symbolic action.

Ludruk is seen as a "rite of modernization." It expresses symbolically the contrast between modern and old-fashioned, and it provides models for "modern" behavior, thus facilitating the passage of proletarians from old to new. Further, some *ludruk* dramas are fantasies which encourage people to think in ways consonant with the modern world; specifically, they encourage people to think lineally and to be interested therefore in changing their situation by action directed at the goal of moving upward to elite status. This argument has a further twist; since most *ludruk*-goers are too old for upward mobility, Peacock hypothesizes that the effect of *ludruk* will be seen on their children, who will reflect their parents' changed attitude.

The success of *ludruk* as a purveyor of ideas to proletarian society is due to its spontaneous origin and to its reflection of the society around it One wonders how successful an elite theater, calculated as a propaganda instrument, would be. Certainly, attempts by the government and the Communist party to use *ludruk* to spread ideas deliberately have had at best an ambiguous success. On the whole, the ideas in *ludruk* which may cause changes in social action are ones that are as much derived from contemporary proletarian Javanese society as they are held up as a model for it. *Ludruk*'s importance lies in the way it reformulates those ideas, and, in its mass appeal for a change in the orientation of the masses, it is a prerequisite to further social and economic changes. Peacock's book provides us with an excellently documented case of how an institution such as the theater may serve as a filter in the process of transmuting ideas into action.

NICHOLAS S. HOPKINS

New York University

Society, Politics and Economic Development. By IRMA ADELMAN and CYNTHIA TAFT MORRIS. Baltimore: Johns Hopkins Press, 1967. Pp. 307. $8.95.

The major obstacles to the understanding of economic development have lain in the hinterland between the established academic disciplines. In each discipline, but perhaps especially in economics, great effort has been devoted to reducing the range of variables and to narrowing and sharpening the focus of attention. As a consequence, it has been possible to achieve greater theoretical elegance and also to produce operational hypotheses about matters of considerable public importance. The broader processes of development have not, however, proved amenable to this treatment. The parameters are key developmental variables, and development strategy is obscured rather than clarified.

There is a substantial measure of agreement among economists about this, although there are considerable differences of opinion about the usefulness of contemporary economic theory in dealing with short-run problems of allocation, but it has proved very difficult to widen the scope of inquiry without a serious loss of analytical precision: interdisciplinary work too often produces a collection of important but loosely related and broadly defined variables.

This book presents a framework for examining the interrelationship between economic, social, and political factors, which involves both a radically new conceptual design and a methodology that could test the hypothesis suggested by the analysis.

The authors select forty-one indicators of economic, social, and political development and provide scores for each of seventy-four countries. There are three types of indicators, ranging from those for which ordinal rankings are based on published statistics to those which are entirely matters of judgment. The authors interviewed area experts to get assessments of appropriate scores for qualitative indicators and to test their initial ratings of semi-qualitative indicators.

Using factor analysis—although frequently used by psychologists, it has rarely been employed by economists—the authors provide both a short-run and long-run analysis of their data. The long-run analysis is based on cross-section scores for 1957–62, with the assumption that different levels of development at a point

of time provide a reasonable approximation to the changes that would appear along a time path. The short-run analysis compares scores in 1950–51 with those for 1963–64.

A brief summary of their findings does justice neither to the sophistication of analysis nor to the subtlety with which authors discuss the implications of their work. Bearing that in mind, it can be said that the work supports and elaborates a theory of successive preconditions to the stage where rapid economic growth is feasible. At the lowest stage of development (represented mainly by African nations) the most significant differentiating factors are those which reflect the breakdown of traditional social structures. At the intermediate level, where some of the social obstacles have been diminished, differences in the nature of political institutions and the degree of participation become relevant and significant. And in the highest stage, on the threshold of rapid growth (where a majority of the countries were Latin American), factors such as leadership attitudes toward economic development become crucial.

Much more specific and operationally useful hypotheses can be drawn from this book. Since, however, the data have been used to produce the hypotheses, it will require different data to test them. A proper assessment of the authors' contribution must await further tests.

It would, in any event, be rash to put too much weight on the judgment of "about thirty . . . experts" (p. 13), however distinguished, about the social, economic, and political characteristics of seventy-four countries for many of which there is very little reliable information. The same experts might want drastically to revise their estimates in a few years' time, and others may give different judgments now. It would, moreover, be fortunate if estimates based on any short period provided a reliable indicator to long-term trends. The period 1957–62 was characterized, especially in Africa, by political and social turmoil, and at least some of the judgmental scores given by the authors clearly relate to a very short and very untypical phase.

A great deal more work needs to be done before one can be sure that the authors have made a significant breach in the obstacles that lie in the hinterland. In the meanwhile, one can safely say that this is an important book, it will probably be a seminal book, and it may be one of the great ones.

T. David Williams

University of Edinburgh

The Geography of Modernization in Kenya. A Spatial Analysis of Social, Economic and Political Change. By Edward W. Soja. (Syracuse Geographical Series, No. 2.) Syracuse, N.Y.: Syracuse University Press, 1968. Pp. xiv+143. $11.00.

Soja's study was his doctoral dissertation in geography at Syracuse University. Like all good theses it begins with a brief methodological introduction, this case focusing on Karl Deutsch's concept of *social mobilization* as the process which underlies modernization, and the notion that *social communications* provide the matrix over which modernization takes place. This matrix is given spatial expression in three succeeding chapters dealing, in turn, with the geography of Kenya before European penetration, the spread of Europeans and Asians and the onset of transition, and the development of the networks of transportation and communications. Content is provided in chapter v by a description of spatial variations in the social, economic, and political characteristics of the population. Chapters vi–viii then concentrate on twenty-five characteristics, measured for thirty-five enumeration areas in the country, detailing variations in demographic (five variables) and education (four) differences, communications and transport (seven), connectivity with Nairobi (two), economic development (five), and political development (two). The results of a principal components analysis of these data are discussed, along with an examination of the simpler structure resulting from normal varimax rotation. A final chapter draws the insights together into an overview of the geography of modernization in independent Kenya.

The principal conclusion of the book is that geographic proximity and accessibility to the major nodes and flow lines within the colonial circulatory system affected the degree to which the various peoples of Kenya were exposed to and transformed by the processes of change. Gaps between ethnic groups have been accentuated by the emergence of two modernization subsystems, one European-Asian, the other African. The extent of national political integration can be gauged by the geographic breakdown of Kenya into the national core area of Nairobi and a limited number of other national nuclei set within a limited number of "national participant areas" that are extending the effective national territory at the periphery. But fully three-quarters of the area of Kenya re-

mains outside effective national control, indifferent or anatagonistic to the tasks of nation building, and subject to regular Somali *shifta* raids.

The book is in many ways a first effort, a dissertation that satisfies professional certification requirements. Sections are written less than gracefully, and the verbalization of components analysis is clumsy and inexact. One is also bothered by vectors of component scores that are highly correlated—did something go wrong with the computations? On the other hand, the work promises to be an impressive steppingstone to a professional career that could revitalize geographers' interests in the political economy and political sociology of developing areas by injecting a much-needed dose of theory—an essential ingredient of locational analysis—and a willingness to transcend empirical myopia with well-founded speculation. This is not a book to be ignored by those concerned with modernization in the developing nations, for Soja has contributed a handsome first step in this direction.

BRIAN J. L. BERRY
University of Chicago

How To Learn an Unwritten Language. By SARAH C. GUDSCHINSKY. New York: Holt, Rinehart & Winston, 1967. Pp. 64. $1.95 (paper).

Field Methods in the Study of Culture. By THOMAS RHYS WILLIAMS. New York: Holt, Rinehart & Winston, 1967. Pp. 76. $1.95 (paper).

Visual Anthropology: Photography as a Research Method. By JOHN COLLIER, JR. New York: Holt, Rinehart & Winston, 1967. Pp. 138. $2.25 (paper).

The Craft of Social Anthropology. Edited by A. L. EPSTEIN. London: Tavistock Publications, 1967. Pp. 276. $10.50 (cloth); $5.75 (paper).

Fieldwork is probably the most firmly established and characteristic tradition in anthropology. It is an accepted initiation for a graduate student in anthropology to spend a year or two in the field in a remote society gathering material for a dissertation. When he becomes a full-fledged professional anthropologist, much of what he publishes will be the result of his first and successive trips to the field. And yet, anthropologists have been remarkably reluctant to publish much detail about the problems they encounter and the techniques they develop in their day-to-day gathering and ordering of material. For that reason these four books are interesting as attempts to deal systematically with the methods anthropologists use in the field. Many social scientists may find useful suggestions and techniques presented, and most readers will find this indication of the "state of the art" very interesting.

The first three books are part of a series called "Studies in Anthropological Method," under the general editorship of George and Louise Spindler. The aim of the series is both to show students the kinds of methods that stand behind formal, published monographs and to provide young anthropologists with a guide to field techniques. Sarah Gudschinsky's book will be most useful in conjunction with a course in linguistics. There are abundant practical suggestions for using a tape recorder and for ways to learn an exotic language. Her suggestions are lodged in a summary of linguistic analysis which demonstrates the relevance of this difficult and technical subject to anthropological training.

T. R. Williams discusses topics that have usually been considered part of the informal training of anthropologists. He treats such matters as composing a research proposal and preparing a budget for an extended stay in a field location. He also discusses the difficulties of gaining entry into a native community to carry out research there. One interesting suggestion he makes to young anthropologists is to distinguish between "culture shock" and "life shock." He points out that culture shock can only be dealt with in the field as the anthropologist draws on his own resources of perception and understanding. But life shock can be prepared for by, for example, seeing films of a baby's birth and by spending some time in the emergency ward of a large city hospital getting accustomed to emergencies and accidents and death, all of which the anthropologist will undoubtedly encounter in the field. Williams also includes a valuable Bibliography on various aspects of fieldwork method.

The first two manuals treat subjects which anthropologists widely recognize as important for successful fieldwork. In the third book under review, John Collier describes a research tool that only a few anthropologists have used very extensively. He so persuasively demonstrates the possibilities of "visual anthropology" that most anthropologists in the future will consider a camera an indispensable part of their kit. For example, he shows how long-range and aerial photographs can establish a general frame of

reference for the field study which would probably be unattainable by simple note taking and verbal description. He shows that certain serial events in a community, like a technological process, can be comprehensively documented with sensitive photography. Collier also describes in some detail a photographic research project he carried out in the San Francisco Bay area on the urban integration of American Indians there, and he discusses the considerations involved in taking the "right" photographs. Then he demonstrates his techniques for interpreting and analyzing the cultural material in his photographs. Perhaps most interesting is Collier's use of photographs in interviewing. He takes a number of photographs in a community and shows them to informants for "feedback." The responses of informants to his photographs, he discovers, adds another dimension to data elicited by questionnaires. With his informant's help he composes a photo-essay which he uses as a framework for interviews with other informants.

Collier distinguishes between the "public domain" of a community where one can photograph at will and the "private domain" where there might be objections to photography. And he suggests that the investigator can often proceed from the "public" to the "private" as he gains acceptance by the community. However, Collier has only discussed the use of photography in fieldwork where the camera was already well known or easily accepted. It remains for anthropologist-photographers to discover how to introduce the camera and photographs into communities where these objects might be regarded with considerable fear and suspicion. Many technical aspects of photography are not treated in much detail in this book. Readers who plan to use photography in research will have to seek technical instruction in another place.

The fourth book is a collection of essays by scholars who have worked together at the Rhodes-Livingston Institute in Lusaka, Zambia, or in the Department of Social Anthropology at Manchester University. This book is probably the most penetrating recent discussion of method in British social anthropology and as such seems destined to become an important textbook for university courses. As Max Gluckman points out in his Introduction to the volume, two lines of argument, reflecting two different types of techniques, run through the essays. On one hand, there is an attempt to quantify variables and to treat them with statistical calculations. On the other hand, there is an attempt to present detailed descriptions of "extended cases" or "situations" from which the analysis of social processes develops. The use of statistics and attempt at rigor by anthropologists seems to result from the influence of techniques developed by sociologists. "Situational analysis" or the "extended-case method" is a refinement of method more in the tradition of social anthropology.

The writers who stress the statistical approach—E. Colson, J. C. Mitchell, J. A. Barnes, T. S. Epstein, and M. G. Marwick—show the problems an anthropologist faces in areas where no public records are maintained. They discuss the techniques of census taking, genealogy making, and schedule following which are necessary to describe a population statistically. J. A. Barnes presents a fairly complicated mathematical treatment of such information in "The Frequency of Divorce." It is Barnes's essay that best shows the use that can be made of statistical data in anthropology as well as the limitations of the method. He constructs a rather elegant model for computing the rate of divorce in an African community that makes it comparable with data from other cultures. And he shows that, once the rates have been established, a number of questions are raised which lead to a demand for further research. However, as Barnes points out, such a study is limited by the definitions of the variables treated. For example, Barnes defines divorce in this way: "For purposes of calculation I assume that anyone at anytime is either married or not; and that a marriage can end only by divorce or by the death of one or the other partner" (p. 49). He cautions that a definition like this avoids consideration of the social and cultural context of marriage and divorce. Indeed, he concludes, "Divorce is not one 'thing' and cannot be described by a single index or a single adjective. It is a social process that has many aspects; we can measure some but cannot measure others" (p. 98).

J. Van Velsen stresses the second approach presented in this volume in an essay called "The Extended-Case Method and Situational Analysis." He observes that structural anthropologists, following Radcliffe-Brown, have been more concerned with abstracted structures in society than with the actual behavior of individuals. A structural anthropologist would record only the events that illustrated the struc-

tures he discerned, using himself as the research instrument. This suggests why so little has been published on the methods of investigation used in the field.

Van Velsen proposes that the structural frame of reference is only the starting place for analysis and that it "should be supplemented and enlivened by an account of the actions—both 'normal' and 'exceptional' of the individuals who operate the structures, i.e. the processes going on within the structure" (p. 141). Van Velsen, following Gluckman, challenges the structuralist assumption that the social system is homogeneous and stable and that individual variations can be ignored in favor of broad structural regularities. He argues that people live with conflicting norms and that such conflict may contribute to social cohesion. He suggests that the social processes leading to cohesion can be elucidated with detailed treatment of specific cases showing "the way in which individuals actually handle their structural relationships and exploit the element of choice between alternative norms according to the requirements of any particular situation" (p. 148).

Following the same approach, A. L. Epstein in "Case Method in the Field of Law," discusses the dynamic process of social control. He argues that, by giving details of an event like a dispute, especially by giving the "facts" from the points of view of several informants, "the case takes on a multidimensional aspect: the parties themselves are no longer just names, but characters in the round whose motives become intelligible not only in terms of temperament but also in terms of the multiple roles they occupy simultaneously or at different times" (p. 228).

Sociologists and anthropologists share many of the same problems in different places and in different ways. The series, "Studies in Anthropological Method," aims to make explicit certain techniques anthropologists use; sociologists may find these useful as well. *The Craft of Social Anthropology* presents two seemingly distinct methods of field study, the merits of which can perhaps best be judged in the light of continued studies of the techniques that anthropologists employ in their research.

WM. A. COLLINS

University of California, Berkeley

Party Democracy: Politics in an Italian Socialist Federation. By SAMUEL H. BARNES. New Haven, Conn.: Yale University Press, 1967. Pp. xii+279. $7.50.

This study of the post–World War II politics of one provincial unit of the Socialist party of Italy will be especially interesting to students of power and the polity. The author links his empirical findings to some of the general literature (especially the work of Robert Michels and of S. M. Lipset) on the nature, conditions, and feasibility of democratic rule.

One of Barnes's more interesting contributions is the notion that the "strategic factor" of democracy is the presence of "multiple autonomous channels of communication." This idea, unfortunately, is not elaborated to any substantial degree, but nevertheless merits attention and can be pondered most profitably by way of comparison with Lipset's "institutionalized opposition" and Harry Eckstein's "congruent authority structures."

Another interesting aspect of Barnes's *Party Democracy* is the radical discrepancy between his concluding note and the logically appropriate inference. In his concluding sentence, "Michels' cruel game continues," Barnes suggests that his investigation tends to confirm anew Michels' judgment that oligarchy is the "preordained form of the common life of great social aggregates" and that history's "democratic currents" are destined to follow an even ebb and flow. Such a conclusion is hard to reconcile with Barnes's previous testimony. Barnes testifies that (1) the operative policies of the Arezzo Federation of the PSI coincide rather closely with the policies favored by most of the members; (2) a heavy majority of the members voice approval of the policies which are operative; (3) the principal forces favoring oligarchy (non-responsive policies) are poverty, meager education, and (consequently) low rates of political knowledge and self-confidence among the ordinary members; (4) the postwar history of the federation is a history of democratization, thanks largely to improvements in the members' incomes, education, and other socioeconomic resources; and (5) the federation's incumbent leaders cannot stray far from the policies preferred by the members, even though the members are quite poor and ill-educated by comparison with most people in modern states. This apparently occurs because there are two well-organized factions within the federation,

with other proximate groups which discontented members can easily join. The members can prevent the enactment of unpopular policies by refusing to comply with leaders' directives.

On this showing, Barnes surely ought to voice an optimistic prognosis for democracy. He finds very little oligarchy in an association which is substantial in size, is bureaucratized, and is populated by exceptionally poor, ill-educated members. He finds a trend toward democracy coinciding with gains in size and organizational complexity. He does not say, or even hint, that the Arezzo Federation's democracy (or lack of gross oligarchy) is attributable to extraordinarily rare, ephemeral conditions.

In *Party Democracy*, then, we find a curious phenomenon which may be described as biased pessimism (on the part of a pro-democratic social scientist) concerning the feasibility of democracy. The case, I believe, is not an isolated one. Michels, too, was spuriously pessimistic about democracy (see my "Democracy, Organization, Michels," *American Political Science Review* [June, 1965]). A fulsome study of this phenomenon may be in order.

JOHN D. MAY

University of Chicago

The Social Context of Premarital Sexual Permissiveness. By IRA L. REISS. New York: Holt, Rinehart & Winston, 1967. Pp. xiv+256. $7.95.

Love Match and Arranged Marriage: A Tokyo-Detroit Comparison. By ROBERT O. BLOOD, JR. New York: Free Press, 1967. Pp. xv+264. $7.95.

Comparative Perspectives on Marriage and the Family. Edited by H. KENT GEIGER. Boston: Little, Brown & Co., 1968. Pp. 222. $2.95 (paper).

In 1960 Reiss published a comprehensive analysis of premarital sexual standards in the United States. In *The Social Context of Premarital Sexual Permissiveness*, he empirically tests, refines, and builds upon the material set forth in the earlier work. First, he attempts to develop effective measures of premarital standards and permissiveness. He then uses these measures on student and adult samples to examine sociocultural correlates of permissiveness and offers theoretical propositions based upon his findings. His treatment of the data is exceptionally thorough. This book provides a valuable basis for future empirical and theoretical work in this area.

The main emphasis in the measurement of permissiveness is given to the development of a Guttman scale. The two components included are the type of physical behavior (kissing, petting, and coitus) and the degree of affection accompanying each activity (engaged, love, strong affection, and no affection). There are two reversals in the scaling, both of which should lead to further theoretical refinement. Alternative measures are also discussed including ways of modifying the basic Guttman scale and the categorization of respondents according to the types of sexual standards developed by Reiss in his previous book.

The relationships between permissiveness and a wide variety of independent variables are integrated into seven propositions which in turn are summarized by a general theoretical statement. These propositions provide a useful framework for interpreting the large amount of data included in the study. Reiss's final theoretical proposition reads as follows: "The degree of acceptable premarital sexual permissiveness in a courtship group varies directly with the degree of autonomy of the courtship group and with the degree of acceptable premarital sexual permissiveness in the social and cultural setting outside the group" (p. 167). His discussion of the implications of the theoretical statement includes an analysis of the influence on permissiveness of the changing roles of individuals in the family and courtship institutions.

A complex relationship is reported between permissiveness and race, sex, social class, and liberal-conservative setting. For instance, social factors, such as church attendance and liberal setting, show a stronger relationship to permissiveness among groups with low traditional levels of permissiveness (whites, females, and upper classes) than among groups with traditionally higher levels of permissiveness. Race and sex are related to permissiveness; social class is not. It appears that the bases for permissiveness may differ among social classes—in the lower classes, tradition and socioeconomic forces are important, while among the upper classes, general liberalism is an important factor. Further work should enrich the implications of these relationships, especially since lower-lower-class respondents may have been underrepresented in the samples used in this analysis.

In *Love Match and Arranged Marriage*, Blood attempts to answer two questions: "(1) What are the differential consequences after marriage of the new and old systems of mate-selection in Japan? (2) Are the effects of internal and external forces on marriage the same in Tokyo as they are in Detroit?" (p. vi).

In the first section of the book, Blood compares the two types of marriage in Japan—love match and *miai* (arranged to the extent that the partners met through a formal introduction or *miai*)—on several dimensions of marital satisfaction. The results are broken down by length of time since marriage. Blood reports that the degree of satisfaction on all measures decreases over time for both types of marriage. However, the "satisfaction curves" differ. On some measures the *miai* marriages begin at a slightly higher level than love matches and decrease to a lower level by the end of nine or more years of marriage. Blood interprets this as "delayed courtship," which compensates for the limited courtship preceding *miai* marriages. "Delayed honeymoons" are reported in which *miai* marriages begin at a lower level than love matches, surpass love matches temporarily, and then decline to levels lower than love matches. For respondents married nine or more years measures of love intensity and aggregate marital satisfaction show that satisfaction is highest among *miai* husbands and lowest among *miai* wives. Love-match husbands and wives fall between these extremes. This result is interpreted in the context of the benefits for the man in a *miai* marriage and the awareness of changing patterns on the part of *miai* wives.

There is some ambiguity in Blood's analysis of the two courtship systems. He states that so many people participate in both that "to some extent, the system one marries by is a matter of chance" (p. 46). These two systems are also characterized as "*alternative* systems of mate-selection despite the overlap" (p. 45). Again, "Arranged marriages today are largely a last resort for young people who fail to contract a successful love match. Only the more old-fashioned segments of the younger generation (less educated, less emancipated girls) are apt to rely on this system" (p. 58).

In the second section, Blood compares marriages in Detroit and Tokyo. These comparisons effectively show the influence of differences between the two societies on husband-wife relationships. In this section, as previously, most differences appear to be small but fairly consistently patterned. The ways in which indexes have been constructed and presented make it difficult to interpret the size of differences. The analysis throughout the book is perceptive and creative although rather informally and loosely connected with the data.

Comparative Perspectives on Marriage and the Family, edited by H. Kent Geiger, contains a well-balanced selection of articles and an integrating introduction. The book is divided into the following sections: "Aspects of Change and Continuity"; "Relations of the Family with Larger Groupings"; "The Patterning of Emotions"; "Perspectives on the Quality of the Marital Tie"; "Contexts for Early Learning Experiences"; "Sources of Important Personality Components"; and "Current Social Problems." The articles cover a wide variety of societies with some emphasis on Israel and Japan. The articles by Norman Dennis and Peter Marris are especially worthwhile.

Geiger's Introduction builds a framework around the articles by pointing out common themes and posing questions arising from the selections. Especially interesting is his discussion of the ways in which the family and society interact on each other in modern mass and modernizing societies. In modern mass society, the family has become "a 'haven' from the pressures and change of the outer world" (p. xx). This retreat contributes to the decreased possibilities for a sense of community in a wider context. In modernizing societies, loyalty to the family can obstruct development.

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American Kinship: A Cultural Account. By DAVID M. SCHNEIDER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968. Pp. 117. $5.50 (cloth); $2.50 (paper).

The distinction between "cultural" and "social" systems may have found a more congenial home in anthropology than in sociology. David Schneider specifies his subject as a cultural system—a system of symbols.

His problems, then, include what *distinguishes* a "relative" from a non-relative; what a distinctively kinship relationship is; how the relative as a person is constructed out of kinship elements combined with elements drawn from

other systems (e.g., age role, sex role, stratification); how certain variations are intrinsic to the nature of the system; and how kinship relates to other symbolic systems.

He indicates that kinship in America means a biogenetic relationship, a "code of conduct" of "enduring, diffuse solidarity," or both. There are thus relatives in nature, relatives in law ("law" as a more general term for the "code of conduct element"), and relatives in both nature and law. The unitary term for enduring, diffuse solidarity is "love," differentiated into erotic (between spouses) and cognatic (between parent and child).

The distinction between involuntary substance and a voluntary code for conduct accounts for the presence of variance where there are multiple terms for the same "genealogical category" (father as "pop," "dad," "my old man"), and for variation in whether certain people (e.g, uncle's wife) are considered relatives at all.

Schneider writes: "The fundamental symbol out of which the system of symbols of the American kinship system is differentiated is . . . that of coitus. In the whole universe, which is divided into an order of nature and an order of law [or custom, reason], it is the symbol of coitus which relates the kinship system to the universal system. The two parts of the symbol of coitus which are differentiated are, first, that of the substantive outcome—the child who shares the biogenetic material of its parents—and, second, the relationship (conduct) of the two parents to each other. The word for this latter aspect is 'marriage': it stands for the unity of the husband and wife, their unity in a sexual relationship, and a unity which is opposed to the unity of parent and child" (p. 96).

A *symbolic* function of kinship is the mediation of this relationship between nature and law or reason. Thus one could predict from Schneider's analysis that, when Americans (correctly or incorrectly) apprehend something askew in the general order of things, they will correlate it with something being askew in the structure of the family, and vice versa.

In a provocative view, Schneider states that the study of a highly differentiated society such as our own may be more revealing of "the nature of kinship" than the study of more anthropologically familiar, less differentiated societies.

American Kinship is a brilliant book and will become a paradigmatic example of cultural analysis—especially because it taps an area which is often posited as dealing with the given, most basic "facts of life." That very assumption is part of our culture and one of the reasons why the symbols are such powerful ones.

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Toward a Society of Leisure. By Joffre Dumazedier. Translated from the French by Stewart E. McClure. New York: Free Press, 1967. Pp. x+307. $6.95.

In his Foreword, David Riesman suggests that "*Toward a Society of Leisure* [should] be read with a double vision: on the surface for what it has to say about its topic, and on a second level, for what it has to say about the state of French social science in the person of a dedicated and energetic spokesman" (p. x). The book, which originally appeared in 1952 as *Vers une civilisation du loisir?* presents Joffre Dumazedier's very personal statement of the relation between leisure and life, based on inquiries that he has conducted over the past decade to describe and to illuminate the problems in a leisure society. Dumazedier, an action-oriented man, holds little regard for theory or facts that do not help in shaping policy. This orientation makes the book particularly significant.

Because the sociological approach to leisure has not yet moved from taxonomy to process theory, Dumazedier was forced, necessarily, to review and describe a great many of the mundane facts of leisure—the development of the mass media, of mass audiences, of mass leisure. His own, optimistic orientation, however, makes these descriptions meaningful and lively. Thus, a discussion of changes in the role of holidays concludes with the recognition that the "opposition between the technical and the natural environments takes place according to an evolutionary and dialectical rhythm in which the mixture of tradition and progress in daily behavior is highly complex" (p. 61) and recommends the subject to ethnographers and folklorists. Dumazedier is ever cognizant of the fact that all human activity has personal symbolic meaning, while at the same time it contributes to the social reality and the definition of society.

He is equally sensitive to the ties that bind leisure to work. "The humanization of work

through the values of leisure is inseparable from the humanization of leisure through the values of work" (p. 87). He extends this *Struktur-zusammenhang* to an analysis of the free time of housewives, and traces the relationship between the amount of free time a housewife has to the value she places upon having it. "How she makes use of her time is then determined by the choices she makes among . . . leisure opportunities" (p. 95).

The first part of the book deals with leisure and the social system, the second with leisure and the cultural system. Three components are stressed in particular: vacationing and tourism, the mass media (i.e., the cinema and television), and self-improvement. The most useful discussion for me was the first. Aside from Hans-Joachim Knebel's *Soziologische Strukturwandlungen im modernen Tourismus* (Stuttgart: Ferdinand Enke Verlag, 1960), there has been no sociological analysis of this phenomenon. Dumazedier is necessarily confined to rather descriptive discussions of the rise of travel, but he manages to consider the problem in the context of cultural exchange.

Dumazedier has grasped the difficulties inherent in a pluralistic democracy, and his work is particularly relevant in the contemporary struggle for the decentralizations that various radical and occupational groups have been waging. Students in Nanterre as well as parents in Ocean-Hill Brownsville have a place in the pluralistic model from which Dumazedier has developed his vision of the "Society of Leisure."

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Educational Climates of High Schools: Their Effects and Sources[1]

Edward L. McDill, Leo C. Rigsby, and Edmund D. Meyers, Jr.

ABSTRACT

This contextual analysis demonstrates that (1) a number of dimensions of the educationa climates of schools have moderate effects on the mathematics achievement and college plans oi students, with relevant individual attributes controlled; (2) several indicators of "intellectual" or "cultural" facilities of the community and measures of school curriculum and facilities do not qualify as sources of variations in climate effects; and (3) the degree of "parental involvement in and commitment to the school" is the one contextual variable which is a source of climate effects. The educational implications of the results are discussed.

THE PROBLEM

In a recent article, Benjamin Bloom[2] has contended that there are few schools in the United States which actually constitute consistent and powerful educational environments. Research evidence by social scientists on the impact of differing contexts, climates, or environments of both high schools and colleges supports such a position. The findings from a number of studies[3] in this area in the last two decades indicate that those school environments in which intellectualism and academic achievement are positively valued and stressed by teachers and students have a positive but only modest impact on the cognitive development of students. Moreover, in the high school studies there has been a tendency to infer the normative climate of schools from the "dominant social class character" of the student body (i.e., the average socioeconomic composition). In other words, there is a conspicuous lack of direct measures for the characteristics of the school environment and their influence on the academic achievement, values, and aspirations of students. As Bloom[4] has noted, steps should be taken to assess school environment more directly in order that policy decisions which will promote the desired academic growth in students may be made.

This paper reports the results of a large-scale study relevant to this concern. More specifically, it focuses on three interrelated problems: (1) the identification of a number of dimensions of educational and social climates of high schools; (2) the assessment of the contextual effects of these environmental dimensions on the academic performance and college plans of students;[5] (3)

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[2] Benjamin S. Bloom, "Stability and Change in Human Characteristics: Implications for School Reorganization," *Educational Administration Quarterly*, II (Winter, 1966), 35–49.

[3] For a list of references which present extensive reviews of research in this area, see Edward L. McDill, Edmund D. Meyers, Jr., and Leo C. Rigsby, "Institutional Effects on the Academic Behavior of High School Students," *Sociology of Education*, XL (Summer, 1967), 181–82.

[4] Bloom, *op. cit.*, p. 47.

[5] In order to demonstrate contextual effects on the behavior of individual students, it is necessary to separate the consequences of school conditions from those of the individual's own characteristics for his behavior. Stated differently, one has to demonstrate an impact of school environment on the dependent variables with individual "input" factors such as scholastic aptitude, family socioeconomic status, and their internalized academic orientations con-

the investigation of sources of climate effects on the achievement and college plans of students by controlling formal organizational properties of the school and community level variables.

After presenting the results relevant to the above three problems, a discussion will be presented of possible ways of modifying the influence of those factors which seem to be the sources of the effects of school climate and which thereby strengthen the impact of the school environment on the academic growth of students.

METHOD[6]

Twenty public, coeducational high schools selected in a three-stage, non-random manner from seven geographical areas and eight states comprise the sample.[7] This design resulted in a sample of institutions which exhibit marked heterogeneity with respect to demographic, socioeconomic, and community characteristics, and also considerable variation in academic achievement (as measured by one of the standardized achievement tests used in Project Talent) and in rates of college attendance.

Data were collected in 1964 and 1965 from students, teachers, and principals of the twenty schools, using the following instruments.

1. Self-administered questionnaires to 20,345 students in which the subjects were treated as both respondents and observer-informants about the school environment.
2. A self-administered questionnaire completed by 1,029 faculty members in the twenty institutions. In completing the questionnaire, the teacher, just like the student, was acting as both respondent and observer-informant.
3. Principal's questionnaire. Each of the twenty principals completed a questionnaire, providing data on a number of social, demographic, and academic characteristics of the school.
4. Two academic tests from Project Talent administered to the student bodies of the twenty schools. These were (*a*) Aptitude for Abstract Reasoning (AR), a fifteen-item, multiple-choice test designed to measure ability to determine the relationship among patterns of diagrams. Scores on the AR test provide one indication of a student's intellectual potential which is relatively independent of curriculum content, since formal instruction is not generally provided on this specific reasoning task at any grade level.[8] (*b*) Achievement in Mathematics (MATH), a twenty-four-item multiple-choice test constructed to provide an indicator of achievement through the ninth-grade level (other than arithmetic computation and reasoning).[9]

trolled. These three variables are among the most important predictors of academic achievement and educational plans. Furthermore, to isolate a contextual effect requires that a relationship between the group level attribute and a dependent attribute at the individual level be demonstrated while the corresponding characteristic for individuals is controlled. In this study, students' academic orientations or values are used as the individual counterpart of the measure of school environment. For an explication of the logic and methodology underlying contextual analysis, see James S. Coleman, "Relational Analysis: The Study of Social Organization with Survey Methods," *Human Organization*, XVII (1958), 28–36; and Peter H. Blau, "Structural Effects," *American Sociological Review*, XXV (1960) 178–93.

[6] A more extensive presentation of the method is found in Edward L. McDill, Edmund D. Meyers, Jr., and Leo C. Rigsby, "Sources of Educational Climates in High Schools" (final report to the Office of Education, U.S. Department of Health, Education, and Welfare under Contract OE-3-10-080, December, 1966). This document is available from the ERIC Document Reproduction Service (Accession No. ED 010 621).

[7] The schools were chosen with the goal of obtaining considerable variation on (1) various "output" measures, such as college-going and achievement levels; (2) a number of "input" measures, such as I.Q. and socioeconomic composition; and (3) demographic and social factors which were expected to relate to school climates.

[8] John Flanagan *et al.*, "Project Talent, Studies of the American High School" (Monograph No. 2, University of Pittsburgh, 1962), p. 6-5.

[9] More complete descriptions of these two academic tests may be found in John T. Dailey and Marion F. Shaycoft, *Types of Tests in Project Talent* (U.S. Department of Health, Education, and Welfare, Office of Education, Cooperative Research Monograph No. 9). Washington: U.S. Government Printing Office, 1961.

The response rates of subjects to each of the instruments were high. Consequently, bias due to non-response cannot have an appreciable effect on the validity of the findings presented below.[10]

Measures of school climate.—The measures of the academic and social climate of the schools were obtained using a modified version of Selvin and Hagstrom's procedure[11] for classifying formal groups in terms of a large number of variables so that contextual effects on the variation in behavior of members can be assessed. In the present research, thirty-nine aggregative characteristics[12] of the schools based on data from both student and teacher questionnaires were factor analyzed using the principal component solution, and then orgthogonally rotated to simple structure using the varimax method.

Using the eigenvalue criterion, six interpretable factors were extracted, which summarize with a relatively high degree of precision (82 per cent of the total variance) the information contained in the thirty-nine variables. Estimates of factor scores were computed for the schools on each factor, and these estimates were then used to rank the schools for the contextual analysis.

The measures of these various components are considered one of the most important accomplishments of the study since they represent comprehensive, stable, and *direct* indicators of the normative influence of school environment. A number of earlier studies in this area have inferred the level of school climate from indirect indicators such as the average socioeconomic composition of the student body or of the neighborhood in which the school is located—indicators whose overall adequacy has recently been questioned.[13]

Limitations of space do not permit a detailed description of the dimensions of school environment. However, this information is readily available to interested readers.[14] The six dimensions were interpreted and labeled using those variables which have statistically significant loadings ($.05 > p$) on the corresponding rotated factors:[15]

- Factor I—academic emulation
- Factor II—student perception of intellectualism-estheticism
- Factor III—cohesive and egalitarian estheticism
- Factor IV—scientism
- Factor V—humanistic excellence
- Factor VI—academically oriented student status system

Measures of individual level variables.—The AR test is employed as the measure of mental aptitude. Measures of sex, grade in school, and family socioeconomic status (SES) were obtained from single items in the student questionnaires. The measure of family SES is provided by information on father's education.

Students' academic orientations or values are measured by a scale derived from six items contained in the student questionnaire, each tapping a different component within the broad realm of intellectualism-achievement; for example, "learning as much as possible," the importance of "good grades," the degree of satisfaction received from "working hard on studies," and the degree of admiration the subject has for "bright students." The responses to each item were dichotomized, and each respond-

[10] For a systematic treatment of this problem, see McDill *et al.*, "Sources of Educational Climates in High Schools," pp. III-17–III-27.

[11] Hanan C. Selvin and Warren O. Hagstrom, "The Empirical Classification of Formal Groups," *American Sociological Review*, XXVIII (1963), 399–411.

[12] Aggregative characteristics are summarizing measures based on smaller units (in this case, individuals) within formal groups. Twenty-seven of the thirty-nine variables are scales; the remaining twelve are single-item indicators.

[13] William H. Sewell and J. Michael Armer, "Neighborhood Context and College Plans," *American Sociological Review*, XXXI (1966), 159–68; and McDill *et al.*, "Institutional Effects. . . ."

[14] McDill *et al.*, "Institutional Effects," p. 187 (Table 2).

[15] Harry H. Harman, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1960), p. 177; and William W. Cooley and Paul R. Lohnes, *Multivariate Procedures for the Behavioral Sciences* (New York: John Wiley & Sons, 1962), p. 172.

ent was assigned a score based on the number he answered in the positive direction. The reliability estimate of .59 obtained from the KR-20 formula indicates that the scale has an acceptable level of internal consistency.

The measure of one of the two dependent attributes, students' plans regarding college attendance, is inferred from a single item in the student questionnaire. Students' scores on the twenty-four-item MATH test serve as the measure of academic achievement.

The statistical technique and categorization of variables.—To minimize the problem of obtaining spurious climate effects, which is a matter of special concern in contextual analysis, a statistical procedure for use with attribute data was employed using as many categories as possible on the individual level attributes.[16] In this multiple-regression technique, the "effect parameter" for each explanatory attribute may be viewed as closely analogous to an unstandardized regression coefficient. In particular, when the dependent variable is dichotomous (as here), this parameter can be operationally interpreted as follows: It gives the change in probability of being "high" on the dependent attribute, given that the person changes from one level to the next higher level on the independent attribute under consideration, but that his level on all other attributes stays unchanged.

RESULTS

EFFECTS OF SCHOOL CLIMATE AND PERSONAL CHARACTERISTICS ON MATH ACHIEVEMENT AND COLLEGE PLANS

As noted above, contextual analysis requires that the effects of the environment on the dependent attributes be demonstrated

[16] For a discussion of the problem of statistical artifacts in contextual research and suggestions for ways to cope with these problems, see Arnold S. Tannenbaum and Gerald G. Bachman, "Structural versus Individual Effects," *American Journal of Sociology*, LXIX (1964), 585–95. The multivariate technique is a modified version of Coleman's stochastic model for the multivariate analysis of attribute data (see James S. Coleman, *Introduction to Mathematical Sociology* [London: Free Press of Glencoe, 1964], chap. vi). Boyle, by slightly modifying Coleman's technique, has demonstrated that it yields parameters for the effects of dichotomous independent attributes on dichotomous dependent attributes which are mathematically equivalent to unstandardized regression coefficients obtained from multiple regression of dummy variables (see Richard P. Boyle, "Causal Theory and Statistical Measures of Effect: A Convergence," *American Sociological Review*, XXXI [1966], 843–51). The model has been formally extended by Coleman to make it applicable to the case of polytomous independent attributes, either ordered or unordered, on dichotomous dependent attributes. The analogy to multiple-regression analysis is approximate for polytomous attributes, however. Nevertheless, Boyle has shown that the procedure yields effect parameters which are close estimates of the coefficients obtained from multiple-regression analysis of dummy variables which are polytomous.

The following classification scheme was used for the variables. (The primary criterion dictating the classification was to retain a sizable number of cases in each cell of the tables which are used in the multivariate analysis. This procedure results in highly reliable estimates of the effect parameters of each independent attribute on the dependent attributes.) Father's education was divided into four categories, approximating a quartile classification. Raw scores on the AR and MATH tests were standardized (using the C-scale technique) by grade and sex, since there were systematic differences in performance by each sex and grade category. The standardized scores for the AR tests were then collapsed into the four categories, which made the number of cases in each of them as near equal as possible. The distribution of scores on the scale measuring students' achievement orientations was also divided into approximately equal quartiles. The two dependent attributes were dichotomized as follows. Students who indicated that they planned to enrol as full-time in college immediately upon completion of high school were classified as having college plans. All other students were considered as not having firm intentions. For the other attribute, standardized scores on the MATH test were dichotomized as closely as possible to the median. Finally, the contextual measures were dichotomized. This was accomplished by ranking the schools on each contextual dimension and then collapsing them as closely as possible to the median. Obviously, a larger number of categories for all contextual variables would have resulted in more precise measurement of the characteristics. However, use of more refined categories was not feasible because in the analysis presented in a later section the measures of school climate and some of the potential sources of school climate effects are introduced simultaneously. The relationships among these characteristics are sufficiently pronounced that using a larger number of categories would have resulted in empty cells in the tables and produced unreliable effect parameters.

while relevant personnel attributes are controlled, since any purported effects of the group level attributes could be attributable to systematic uncontrolled differences in the individual characteristics among the different groups. This is the primary task of this section of the analysis. A second and related task is to assess the relative effects of each of the individual characteristics.

Each row in Table 1 gives the estimated effect parameters for a model in which one of the climate dimensions and three individual level attributes are used as explanatory variables in an attempt to account for variation in MATH achievement. Table 2 gives corresponding results using college plans as the dependent attribute.[17] Thus, in each row of Tables 1 and 2 the same three

[17] Coleman, *Introduction to Mathematical Sociology*, pp. 218–19, presents a formula for standardizing effect parameters for polytomous, ordered, independent attributes to make them comparable to measures of effect for dichotomous attributes. However, for such adjusted effect parameters to be identical to those obtained from actual dichotomies, the sample has to be rectangularly distributed over the ordered polytomous categories. In all tables in this paper the effect parameters for polytomous independent attributes are standardized to dichotomous form.

TABLE 1

INDEPENDENT EFFECTS OF EACH OF SIX CLIMATE DIMENSIONS OF SCHOOLS AND OF THREE PERSONAL ATTRIBUTES ON MATH ACHIEVEMENT*

Climate Dimensions	Weighted Effects of Climate Dimensions	Weighted Effects of Father's Education	Weighted Effects of Student's Academic Values	Weighted Effects of Student's Ability
I. Academic emulation	.110	.119	137	.299
II. Intellectualism-estheticism...	.072	.130	.136	.305
III. Cohesive and egalitarian estheticism............ ...	.048	.132	.135	.307
IV. Scientism......	.033	.138	.136	.309
V. Humanistic excellence......	.042	.133	.136	.308
VI. Academically oriented status system..................	.046	.134	.136	.308

* All effect estimates are standardized to dichotomous form and are significant at the 01 level.

TABLE 2

INDEPENDENT EFFECTS OF EACH OF SIX CLIMATE DIMENSIONS OF SCHOOLS AND OF THREE PERSONAL ATTRIBUTES ON COLLEGE PLANS*

Climate Dimensions	Weighted Effects of Climate Dimensions	Weighted Effects of Father's Education	Weighted Effects of Student's Academic Values	Weighted Effects of Student's Ability
I. Academic emulation...	.112	.200	.190	.151
II. Intellectualism-estheticism...	.071	.212	.188	.156
III. Cohesive and egalitarian estheticism.	.031	.219	.190	.159
IV. Scientism..................	.002†	.225	.190	.160
V. Humanistic excellence.......	.025	.219	.190	.159
VI. Academically oriented status system..................	.046	.216	.188	.157

* All effect estimates are standardized to dichotomous form. Unless otherwise noted, effect estimates are significant at the .01 level.

† Not significant at the .05 level.

individual level characteristics are being statistically controlled.

The results for the two dependent attributes will be discussed separately. The most important finding in Table 1 is that, when three relevant individual level attributes are held constant, each of the six climate dimensions has a significant positive effect on MATH achievement. These effects are in the direction which would be expected, given the content of the factors. The effects of dimension I are the strongest of the six dimensions, which is probably attributable to its being the most comprehensive and reliable measure of school environment. In fact, its effect is almost as strong as that of father's education and indicates that the more emphasis on academic performance, competitiveness, and intellectualism by both faculty and students, the more likely students are to achieve "high." The effect of the second dimension indicates that the more the school atmosphere encourages an intrinsic value of knowledge and the more teachers are emotionally supportive of students, the more likely students are to achieve on the MATH test. Each of the four remaining climate components has less than half the explanatory power for academic performance than does the first. The effect of the third dimension indicates that the greater the degree of social cohesion, democratic values, and intellectual standards for recognition among students, the higher the achievement level of individual students. The small effect parameter for Factor IV indicates a tendency for those schools exerting a strong press toward scientism to have a higher proportion of their students with high MATH achievement than those institutions which do not encourage excellence in this broad substantive area. Likewise, the effects of the fifth construct reveal that the greater the value placed on the humanities by teachers and students and the greater their emphasis on achievement in general, and on an intrinsic value of knowledge, the greater the proportion of students with achievement in a specific substantive area. Finally, the small positive effects of Factor VI suggest that the more the student social system of the school rewards intellectualism and achievement, the greater the tendency for students to achieve. This result is consistent with the widely accepted hypothesis among social scientists that adolescent subcultures of high schools have an impact on the values, aspirations, and achievement of individual students.[18]

Each of the three individual attributes in Table 1 has a sizable effect on achievement in the expected direction, with ability having by far the greatest magnitude. The high degree of association between ability and performance is consistent with the findings of numerous studies which document the considerable predictive power of intellective factors for performance.[19]

The substantial effect of father's education is certainly not unexpected, since family SES is the social background factor which

[18] Sarane S. Boocock, "Toward a Sociology of Learning: A Selective Review of Existing Research," *Sociology of Education*, XXXIX (1966), 27–32 and 41. For the present sample it should be noted that, in nineteen of the twenty schools, both "leadership in activities" and "athletics" ("cheerleader" for girls) are viewed as more important for status among other students than "high grades." Furthermore, in all of the twenty schools, both leadership in activities and athletics are considered more important for prestige than "knowing a great deal about intellectual matters." With such evidence it is obvious that the label "academically oriented status system" is not applicable to any of the twenty schools in absolute terms, but only relative to each other. These results are consistent with those of Coleman in *The Adolescent Society*. In each of the ten high schools he studied, scholastic achievement was less valued by students than other activities such as athletics, popularity, and leadership in activities (see James S. Coleman, *The Adolescent Society* [New York: Free Press of Glencoe, 1961]).

[19] An excellent summary of this research is found in David E. Lavin, *The Prediction of Academic Performance* (New York: Russell Sage Foundation, 1965), chap. iv, who notes that the correlation is higher at the high school level than at the college level, which can be explained by the more restricted range of ability of college students. Lavin estimates, based on his survey of the literature, that the average zero-order correlation between ability and *grades* for high school students is .60. In the present research, the zero-order, product-moment correlation between AR scores and MATH scores is .52.

has been most consistently demonstrated to be related to academic performance.

The third personal attribute in the table, students' academic values, also has an appreciable independent influence on their achievement level. This attribute may be viewed properly as an indicator of students' commitment to learning and achievement, and its effect suggests the importance of such a personal value system to the academic development of youth who are in constant demand as the educational and scientific entrepreneurs of modern society

The results of Table 2 reveal that, in general, the effects of the climate dimensions on college plans are not appreciably different from those on achievement. The single exception is for Factor IV, scientism, which has a small positive effect on MATH but no effect on college plans. One possible explanation for this lack of effect is that the items used in the faculty and student "presses" for scientism, which are the variables with the highest loadings on Factor IV, are inadequate measures of the degree of scientific emphasis in the schools.[20] However, this does not appear to be a completely satisfactory explanation, since Factor IV has a small positive effect on MATH, as shown in Table 1. This result suggests an alternative explanation: The degree of scientific ferment in the high school, although related to MATH performance, is not directly related to college plans. In other words, for the kind of scientific enthusiasm that may prevail in a high school, achievement in mathematics is useful, but college plans are irrelevant. It is not science as a career, but science in the here and now that is being tapped. Two pieces of evidence to support this interpretation are offered. First, the evidence in the study shows that the great majority of students with college plans do not mention "understanding science" as a primary purpose in attending college. In fact, out of a diversified list of ten purposes it ranks next to lowest in importance. Second, only 12 per cent of the students with college intentions indicated they planned to major in science in college.

Turning to the independent effects of the three personal attributes on college plans as shown in Table 2, it can be seen that each has a considerable effect on students' intentions, with father's education having more impact than any of the other attributes. Especially noteworthy is the point that, on the average, father's education has an effect parameter approximately .05 larger than that for student ability. Michael,[21] in his analysis of a nationally representative sample of seniors in 500 public high schools reports similar results; that is, students' socioeconomic background exerted a slightly greater influence on their college plans than did ability. Both of these findings seem consistent with the conclusion reached by Sibley[22] twenty-five years ago that the intelli-

[20] There are only five variables with significant loadings on Factor IV. These variables with their loadings are as follows: student perceptions of faculty press for scientism (.901), student perceptions of student press for scientism (.735), faculty perceptions of faculty press for scientism (.883), faculty perceptions of student press for scientism (.756), and faculty perceptions of faculty press for independence (.628). This latter variable is conceptually consistent with a strong emphasis on scientism since it measures the extent of teachers' encouragement of independent and creative work by students. ("Press" refers to the characteristic emphases or pressures of an environment as perceived by the collectivity of informants who constitute its membership.)

[21] John A. Michael, "High School Climates and Plans for Entering College," *Public Opinion Quarterly*, XXV (1961), 594.

[22] Elbridge Sibley, "Some Demographic Clues to Stratification," *American Sociological Review*, VII (1942), 330. One could argue that a comparison of college plans of high school students with actual college attendance is tenuous, since an unknown number of students are unrealistic about enrolling in college in their responses to the item in the questionnaire. However, the measure of college plans employed here seems not to be an invalid indicator of college attendance: Only those students with definite plans to enroll immediately after graduation from high school were classified as having plans. Those who gave any of the following responses were categorized as not planning to attend: "no, never"; "yes, but not right after high school"; "yes, as a part-time student right after high school"; and "undecided." A comparison in each school of the percentage of the preceding year's graduates who attended college did not yield any substantial differ-

gence of the student was more important than family SES in determining whether he would finish high school, but the opposite was true regarding the likelihood of attending college. The fact that the effect parameters for father's education and for students' academic values are larger for college plans than for MATH can be best explained in terms of the qualitative difference between the two dependent attributes. College plans and aspirations belong to a class of social-psychological phenomena which are highly susceptible to the constraints of significant others in the immediate interpersonal environment (e.g., parental pressures, of which father's education is an indicator) and to personal motivation and values (as measured by the respondent's academic values). Although the pressures of significant others and personal motivation can enhance students' achievement, such effects are limited simply because there is an upper bound to the ability of the student. Stated differently, it is a truism that the student cannot achieve higher on a standardized test than his ability level permits.

Before turning to a discussion of the sources of school climate effects on achievement and college plans of students, it is important to return to a critical problem in all contextual analyses: the adequacy of the controls for the individual attributes of the respondents. With respect to the dependent attributes under consideration here, there could be concern with the adequacy of father's education as a measure of family background. Consequently, two additional measures of family SES (father's occupation and mother's education) are introduced into the analysis.[23] Table 3 presents the effects of the most important measure of school climate, Factor I, on the two dependent attributes with father's education, mother's education, father's occupation, student's academic values, and ability simultaneously controlled. Thus, the first row of Table 3 is comparable to the first row of Table 1, and the second row of Table 3 is comparable to the first row of Table 2. Holding constant the two additional measures of family background reduces only a negligible amount the effects of the climate dimension on the two dependent attributes. This finding offers further support for the climate effects reported in Tables 1 and 2. In fact, the only noteworthy impact made by the simultaneous introduction of mother's edu-

TABLE 3

INDEPENDENT EFFECTS OF ONE CLIMATE DIMENSION AND FIVE INDIVIDUAL LEVEL ATTRIBUTES ON MATH ACHIEVEMENT AND COLLEGE PLANS*

Dependent Attribute	Weighted Effects of Factor I	Weighted Effects of Father's Education	Weighted Effects of Mother's Education	Weighted Effects of Father's Occupation	Weighted Effects of Student's Academic Values	Weighted Effects of Student's Ability
MATH achievement	.097	.050	.062	.061	.139	.290
College plans......	.091	.081	.114	.084	.179	.139

* All effect estimates are standardized to dichotomous form and are significant at the .01 level.

ences when compared with the percentage of seniors in this sample who expressed college plans. In a study of Wisconsin high school students it was shown that more than 90 per cent of the seniors with college plans actually enrolled in college the following year. These results are reported in J. Kenneth Little, *A State-wide Inquiry into Decisions of Youth about Education Beyond High School* (Madison, Wis.: School of Education, 1958), cited in William H. Sewell, "Community of Residence and College Plans," *American Sociological Review*, XXIX (February, 1964)

[23] The other potential measure of family SES available in the data—annual family income—could not be used because more than 38 per cent of the students were unable to provide reliable responses to the questionnaire item dealing with this family attribute.

cation and father's occupation on the results of Tables 1 and 2 is to reduce by more than 50 per cent the independent effects of father's education on both MATH achievement and college plans. These reductions in the effect parameters reflect the high correlations among father's education, mother's education, and father's occupation.

SOURCES OF CLIMATE EFFECTS ON THE DEPENDENT ATTRIBUTES

A number of educational researchers and practitioners have asserted that characteristics of the community environment—primarily socioeconomic and "cultural" resources—are important determinants of academic "output." Community factors such as amount of financial support for education and presence of intellectual facilities such as libraries and museums have been viewed as outside-school sources of variation in student achievement and educational aspirations. However, as noted by Boocock, the evidence that such factors have important educational consequences is far from conclusive.[24] Given the lack of consistency of findings in this area, it is especially important to assess the importance of those community characteristics for which measures are available in the present study.

A second set of factors which will be introduced as potential sources of climate effects are formal organizational properties of the school. These are school characteristics which, for the most part, were included in the original Project Talent survey—characteristics which reflect a few of the many curriculum innovations and organizational approaches being explored in the last two decades. The single most comprehensive piece of research dealing with the relationship of curriculum and school facilities measures to student achievement is the U.S. Office of Education's survey, *Equality of Educational Opportunity*,[25] often referred to as the Coleman report. Among the numerous important findings produced in the highly provocative and controversial work,[26] one is especially relevant to the present discussion: Most of the variation between students' performance on a standardized test of verbal achievement was not explained by school characteristics and resources, such as per-pupil expenditures, number of books in the library, and student-teacher ratio. That is, despite the great diversity of school facilities, curricula, and resources, the variation in achievement among pupils in the same school was roughly four times as large as the variation among schools.[27] However, in a review article, Bowles and Levin[28] seriously question the validity of these results in the Coleman report on a number of grounds. Data are available in the present research on a num-

[24] Boocock, *op. cit.*, p. 38; e.g., Coleman, *The Adolescent Society*, p. 65, found no relationship between per-pupil expenditure and achievement in ten Illinois high schools when ability of students was controlled. He also cites a state-wide study of Connecticut high school students which failed to reveal such a relationship when ability was controlled. Finally, at the college level, he cites (p. 329) results from the classic study by R.H. Knapp and H.B. Goodrich, *Origins of American Scientists* (Chicago: University of Chicago Press, 1952), which revealed that the undergraduate institutions which were most productive of scientists were not the most affluent colleges. One large-scale study which showed sizable relationships between community characteristics and test scores was William G. Mollenkopf and Donald Melville, "A Study of Secondary School Characteristics as Related to Test Scores" (Princeton, N.J., Educational Testing Service, 1956 [mimeographed]). However, as noted by Boocock, *op. cit.*, his findings are questionable, since the response rate from principals was less than 50 per cent.

[25] James S. Coleman *et al.*, *Equality of Educational Opportunity* (Washington: U.S. Government Printing Office, 1966).

[26] Christopher Jencks, "Education: The Racial Gap," *New Republic* (October 1, 1966), pp. 21–26, has made the unqualified statement that the report incorporates the most important piece of educational research conducted in recent years. On the other hand, Bowles and Levin challenge the adequacy of the data, the statistical analyses, and the validity of the interpretation of the findings (see Samuel Bowles and Henry M. Levin, "The Determinants of Scholastic Achievement—an Appraisal of Some Recent Evidence," *Journal of Human Resources*, III [Winter, 1968], pp. 3–24).

[27] Coleman *et al.*, *op. cit.*, chap. iii.

[28] Bowles and Levin, *op. cit.*, pp. 8–12

ber of school resources and curricular variables similar to those which were used in the Coleman report. Thus, it should be possible to present additional evidence on the debate concerning the importance of school facilities and resources on student output.

a) Factors in the community as potential sources of climate effects.—Turning first to community characteristics as potential sources of climate effects, a number of "cultural" facilities can be summarily dismissed because they do not vary across communities. In every school, students had access to a public community library; in fifteen of the twenty, "concerts" were readily available to them, and the same holds true for "community theater" in sixteen of the twenty schools. Although the communities showed sufficient heterogeneity on four other cultural resources to justify consideration as potential sources of school environment effects—museum, art gallery, opera, and professional stage[29]—investigation of these facilities failed to produce any consistent relationships with the climate measures and/or the dependent attributes. Thus we conclude that in the present sample the presence or accessibility of a number of community cultural facilities has no impact on the relationship of the educational climates of schools to students' academic performance and therefore cannot be defended as sources of school environmental effects.[30]

However, one community level factor which does appear to function as a source of climate effects is the extent of involvement and interest by parents in school policies and in their children's academic performance. This variable is labeled "Parental Involvement in the High School" (P.I.H.S.), and consists of a summated binary rating scale constructed from three items in the teacher questionnaire.[31] This attribute is introduced as an indicator of the extent to which norms and values regarding academic excellence in the school are shared by the parents and thus the community or neighborhood served by the school. The underlying proposition is that the more prevalent these norms and values are, the more likely the school is to develop an atmosphere which encourages students to higher achievement and educational aspirations. The data of Table 4 lend support to this proposition. School ranks on P.I.H.S. are significantly and strongly correlated in a positive direction with their ranks on factor scores for each of the six dimensions of school climate.

In introducing P.I.H.S. into the analysis as a potential climate source variable, the

[29] All of the data on community resources considered thus far were obtained from items in the principal's questionnaire. These measures of the cultural or intellectual atmosphere of the community are crude in the sense that they are merely indicators of the presence or absence of such facilities. However, it is reasonable to assume that the quality of these facilities is positively correlated with a number of socioeconomic resources of the school and/or community presented below for which ordinal or interval data are available, and which are shown *not* to be sources of climate effects on the dependent attributes. Thus, there is no reason to believe that data on the quality of these three facilities would produce different results as potential sources of school climate effects.

[30] Implicit in this conclusion is a statistical truism: For a given variable to be a source of the effects of climate dimensions on students' academic behavior, the variable has to be related to both the climate dimensions and the dependent attributes.

[31] The three true-false items comprising the P.I.H.S. scale are: (1) Most parents in this school are apathetic to school policies. (2) Parents of students here seem interested in their children's progress. (3) Parents often ask for appointments with teachers to discuss their children's schoolwork. The KR-20 reliability coefficient of .64 is high for a measure containing such a small number of items. It is noteworthy that Neal Gross *et al.*, in a 1966 study of the correlates of academic productivity of urban elementary school pupils from low socioeconomic backgrounds, used a similar approach in constructing a contextual measure of parental interest in the academic performance of their children (see Neal Gross *et al.*, "Some Sociological Correlates of the 'Academic Productivity' of Urban Elementary Schools with Pupils from Families of Low Socio-economic Status" [paper presented at the American Sociological Association Meetings, Miami Beach, Florida, August, 1966]). That is, the measure was based on an average of *teachers' perceptions* of parents' interest in their children's academic activities. The present authors were unaware of Gross's study when this analysis was undertaken.

schools were ranked according to their median values and then dichotomized at the median of the distribution.

The effects of P.I.H.S. on MATH and college plans—based on data from a representative, 10 per cent subsample of the students in each school[32]—are .194 (.01 > p) and .193 (.01 > p), respectively. Thus, it meets the first criterion as a source of climate effects. The effects of P.I.H.S. on the two dependent attributes for the entire sample, with ability and father's education controlled, are .099 for MATH (.01 > p) and .111 (.01 > p) for college plans. Stated differently, holding constant these two personal attributes reduces the zero-order effects of P.I.H.S. about 50 per cent; however, its impact on each dependent attribute is still significant and substantively meaningful, demonstrating that it meets the second criterion as a climate source variable.

TABLE 4

SPEARMAN RANK-ORDER CORRELATIONS BETWEEN P.I.H.S. AND SIX CLIMATE DIMENSIONS FOR TWENTY SCHOOLS

Climate Dimensions	r's with P.I.H.S.
I. Academic emulation	.79**
II. Intellectualism-estheticism	.62**
III. Cohesive and egalitarian estheticism	.78**
IV. Scientism	.68**
V. Humanistic excellence	.76**
VI. Academically oriented status system	.48*

* Significant at the .05 level.
** Significant at the .01 level.

Tables 5 and 6 offer further evidence that P.I.H.S. is functioning as a source of the climate effects on both dependent attributes. First, the effects of the climate dimensions on both dependent attributes, with ability and father's education controlled, tend to disappear when P.I.H.S. is introduced as an additional control. In fact, none of the climate effects in either of the two tables is statistically significant at the .01 level. On the other hand, the significant effects of P.I.H.S. on MATH and college plans persist with the climate dimensions controlled.[33]

A discussion of the substantive importance of P.I.H.S. as a source variable will be postponed until a number of other potential sources have been considered. However, it should be noted here that these results are

[32] In the search for source variables, zero-order effects of such variables on the dependent attributes are based on this 10 per cent subsample in order to minimize computer costs, and the .05 level of significance is chosen as the one beneath which a relationship is discounted. For those potential source variables which show a significant relationship with the dependent attributes for the 10 per cent sample, the effects of such variables on the dependent attributes are then computed for the total population with ability and father's education simultaneously controlled. That is, no community or school characteristic can qualify as a source of climate effects unless the characteristic has a significant effect on a dependent attribute with the ability of the students and family SES both held constant.

[33] In fact, the positive effects of P.I.H.S. on college plans are slightly increased when climate components II, III, and V are controlled, and its effects on MATH are very slightly enhanced when component II is held constant. Furthermore, in every instance in Table 6 and in one case in Table 5 the effects of the climate dimensions are slightly negative. No substantive significance is attached to the fact that in certain instances the effects of P.I.H.S. in Tables 5 and 6 are slightly larger than those when only ability and father's education are controlled and that in six instances out of ten in Tables 5 and 6 the climate effects acquire negative signs. In a recent scholarly article on multivariate techniques, Robert Gordon explains how the distribution of the predictive values of two independent variables (as measured by regression coefficients) can be "tipped" or altered in favor of one or the other by changes in the correlations among a set of independent variables (see Robert A. Gordon, "Issues in Multiple Regression," *American Journal of Sociology*, LXXIII [March, 1968], 610–11). The more highly correlated the predictors, the more susceptible they are to being tipped. In fact, as the predictors become very highly correlated, the tipping effect can take the following form: One of the regression coefficients assumes a *negative* value and the other a *higher positive* value even though the predictors and the dependent variable are all positively correlated at the zero-order level. Gordon also carefully documents how erroneous substantive conclusions can be reached from indisdiscriminate use of multiple regression and partial correlation procedures for explanatory variables which are highly correlated *and* not conceptually distinct. Certainly, in the present investigation P.I.H.S. and the climate constructs are both conceptually and operationally distinct.

TABLE 5

INDEPENDENT EFFECTS OF CLIMATE DIMENSIONS, P.I.H.S., ABILITY, AND FATHER'S EDUCATION ON MATH ACHIEVEMENT*

Climate Dimensions	Weighted Effects of Climate Dimensions	Weighted Effects of P.I.H.S.	Weighted Effects of Student's Ability	Weighted Effects of Father's Education
I. Academic emulation........	†	†	†	†
II. Intellectualism-estheticism..	−.006‡	.103	.320	.132
III. Cohesive and egalitarian estheticism.............	.016‡	.092	.319	.131
IV. Scientism.................	.022§	.093	.320	.130
V. Humanistic excellence......	.007‡	.095	.319	.132
VI. Academically oriented status system..................	.000‡	.098	.319	.134

* Results are based on total sample of students, not the 10 per cent subsample. All effect parameters are significant at the .01 level unless otherwise noted.

† The relationship between P.I.H.S. and climate dimension I is sufficiently pronounced that there are no schools *low* on P.I.H.S. and *high* on academic emulation. Consequently, the effects of P.I.H.S. on MATH with academic emulation, ability, and father's education simultaneously controlled cannot be computed. Conversely, the effects of academic emulation on MATH with P.I.H.S., ability, and father's education simultaneously controlled cannot be calculated. (As shown in Table 4, the rank correlation between median school scale scores on P.I.H.S. and factor scores on academic emulation is .79.)

‡ Not significant at the .05 level.

§ Significant at the .05 level.

TABLE 6

INDEPENDENT EFFECTS OF CLIMATE DIMENSIONS, P.I.H.S., ABILITY, AND FATHER'S EDUCATION ON COLLEGE PLANS*

Climate Dimensions	Weighted Effects of Climate Dimensions	Weighted Effects of P.I.H.S.	Weighted Effects of Student's Ability	Weighted Effects of Father's Education
I. Academic emulation........	†	†	†	†
II. Intellectualism-estheticism...	−.018‡	.143	.175	.216
III. Cohesive and egalitarian estheticism.............	−.011‡	.115	.173	.219
IV. Scientism.................	−.009‡	.107	.176	.215
V. Humanistic excellence......	−.021§	.119	.174	.219
VI. Academically oriented status system..................	−.012‡	.110	.174	.217

* Results are based on total sample of students, not the 10 per cent subsample. All effect parameters are significant at the .01 level unless otherwise noted.

† The relationship between P.I.H.S. and climate dimension I is sufficiently pronounced that there are no schools *low* on P.I.H.S. and *high* on academic emulation. Consequently, the effects of P.I.H.S. on college plans with academic emulation, ability, and father's education simultaneously controlled cannot be computed. Conversely, the effects of academic emulation on college plans with P.I.H.S., ability, and father's education simultaneously controlled cannot be calculated. (As shown in Table 4, the rank correlation between median school scale scores on P.I.H.S. and factor scores on academic emulation is .79.)

‡ Not significant at the .05 level.

§ Significant at the .05 level.

consistent with those of a recent large-scale study by Gross *et al.*[34] on a number of correlates of the "academic productivity" of urban elementary schools. One of the variables most positively correlated with the criterion was the faculty's assessment of the extent of parental interest in the academic performance of their children.

b) *Resources and organizational properties of schools as sources of school climates.*—As noted above, one of the most controversial findings of the Coleman report is that economic resources of schools explained only a very small proportion of variance in the verbal achievement of children. Bowles and Levin are highly critical of this finding, arguing that the measurement of variables and the statistical techniques used are "biased in a direction that would dampen the importance of school characteristics."[35] For example, they contend that the measure of per-pupil expenditure used is biased in that it was averaged for an entire school district and therefore did not reflect the variation among schools within a system. They also indicate that their further analysis of data in the Coleman report leads to the implication that another measure of economic resources of schools—teachers' salaries—is positively related to student achievement. Fortunately, rigorous measures of these two variables are available for each of the twenty schools in the present investigation—average per-pupil expenditure and annual starting salaries for teachers. These data permit the consideration of these two variables as climate sources, which can provide further evidence on this important controversy. It should be emphasized that the twenty schools show a great deal of variation on these two characteristics; per-pupil expenditure ranges from $365 to $1,000 per year, and starting salaries for teachers vary $1,000. Consequently, any failure of these two measures of capital investment to account for variation in climate effects could not be explained in terms of restricted range.

[34] Gross *et al.*, *op. cit.*

[35] Bowles and Levin, *op. cit.*, p. 8.

The zero-order effects of these two characteristics on the two dependent attributes for the representative subsample of students are:

Effect of per-pupil expenditure on MATH = −.008
Effect of teachers' salaries on MATH = .008
Effect of per-pupil expenditure on college plans = .054
Effect of teachers' salaries on college plans = .016

Both input resources can be eliminated as sources of climate effects since neither is significantly related to the two dependent attributes. Thus, Bowles and Levin's criticisms of the Coleman report notwithstanding, the results for the twenty schools in this sample certainly do *not* contradict Coleman's conclusion that the variance in achievement which is accounted for by a school facilities measure (which included per-pupil expenditure) is of little consequence.[36]

Ten different formal organizational properties of the schools were also examined as possible sources of climate effects. Three facilities measures were dismissed immediately because the schools do not show sufficient variation on them. These were: (1) use of teaching machines as instructional devices;[37] (2) volumes in school library;[38]

[36] Coleman *et al.*, *op. cit.*, p. 312.

[37] None of the schools used teaching machines regularly "in many instances," and only four used them regularly "in a few instances."

[38] The adequacy of this measure of library facilities is open to question because the response alternatives for the question measuring library facilities were not presented in sufficient detail. A broader range of response categories would probably have produced sufficient variation for the characteristic to be considered as a source. However, in the Coleman report, *op. cit.* (n. 25 above), p. 316, which showed large variation in library facilities, it was demonstrated that the number of volumes per student had only small and inconsistent relationships with verbal achievement for both Negroes and whites in different geographical regions. Furthermore, the Project Talent survey (Flanagan *et al.*, *op. cit.*, p. 6-14) produced correlations of only .203 and .253 between number of volumes in the library and performance on comprehensive tests of mathematics achievement and reading achievement. It should be

(3) percentage of students on half-day sessions (i.e., double shifts).[39]

Table 7 lists seven curricular and facilities characteristics on which the schools were sufficiently heterogeneous to permit their consideration as climate sources. Each

TABLE 7

ZERO-ORDER EFFECTS OF SEVEN CURRICULAR AND FACILITIES CHARACTERISTICS OF SCHOOLS ON MATH ACHIEVEMENT AND COLLEGE PLANS*

INDEPENDENT ATTRIBUTES	DEPENDENT ATTRIBUTES	
	MATH Achievement	College Plans
Size of math and science classes†	−.058‡	−.002‡
Size of classes in non-science courses†	.023‡	.032‡
Accelerated curriculum for superior students§	.065**	.065**
Opportunity to obtain advanced placement and/or credit in college§	.105	.114
Homogeneous grouping of students by ability§	−.032‡	−.041‡
Acceleration policy for graduation§	.142	.149
Percentage of teachers with more than bachelor's degree†	.076	.124

* Results are based on a representative 10 per cent subsample of the students in each school ($N = 2,053$). All effect estimates are significant at the .01 level unless otherwise noted.

† Effect estimates for these attributes are unweighted and obtained from school quartile ranks standardized to dichotomous form.

‡ Not significant at the .05 level.

§ Effect estimates for these attributes are unweighted and obtained from dichotomies, not school quartiles.

** Significant at the .05 level.

of these characteristics is based on a single-item indicator in the principal's questionnaire. The zero-order effect parameters for each of these characteristics on both dependent attributes for the 10 per cent sample are also given. (Whenever there is sufficient variation across schools on these characteristics, their effects are based on quartile ranks standardized to dichotomous form.)

Three of the characteristics (average size of math and science classes, average size of classes in non-science courses, and homogeneous grouping of students by ability) are not related at the .05 level to either dependent attribute. (The effects of class size in non-science areas are not in the "expected" direction.) The failure of the two measures of class size to have appreciable predictive power on MATH achievement is highly consistent with Project Talent results,[40] which used average math achievement scores for *schools* as the unit of analysis, and those of the Coleman report,[41] which used verbal achievement scores of *students* as the unit. Bowles and Levin[42] are highly critical of Coleman's conclusion on this point because they feel the measure of pupil-teacher ratio he used, which was obtained by dividing school enrollment by number of teachers, is an inadequate measure of class size, given the fact that unpublished data in the Coleman report *suggest* great heterogeneity in teaching loads within schools. Nevertheless, the results of the Coleman report, Project Talent, and the present investigation are consistent with the general conclusion of numerous studies at both the high school and college levels to the effect that class size shows no clear relationship to learning.[43] Furthermore, at the elementary level the evaluation of the More

emphasized that these two correlations were based on school means, not on individual student scores. Only under most unusual circumstances can correlations based on individuals be as large as those based on schools (Flanagan *et al.*, *op. cit.*, p. 5-1). Usually, in the Project Talent survey the correlations based on school means were substantially larger.

[39] Only one of the twenty schools had half-day sessions.

[40] Flanagan *et al.*, *op. cit.*, p. 6-17.

[41] Coleman *et al.*, *op. cit.*, p. 312.

[42] Bowles and Levin, *op. cit.*, p. 12. In both the Project Talent survey and the present study the measures of class size in science and math and in non-science courses were based on responses to the following type of item: "What size is your *average* instructional class in science and math [non-science courses]?" Thus it is highly likely that measures in both studies are "defective" in the manner described by Bowles and Levin.

[43] Boocock, *op. cit.*, p. 11.

Effective Schools Program in New York City for disadvantaged students (with one of its most distinguishing characteristics being small classes) has failed to show greater academic growth for these students than for the students in the control schools where there were substantially larger student-teacher ratios and larger average class sizes.[44]

The non-significant effects of homogeneous ability grouping are also in the direction opposite to that predicted by the rationale typically offered by educators who advocate this mode of classroom organization: Teachers can achieve better academic results when teaching a group of students who are relatively similar in learning ability. The measure of ability grouping in this study is admittedly weak because it is based on a single-item indicator which classified the schools into two crude categories—those which group for "many" or "all" courses and those which utilized it for only a "few" or "no" courses. However, the results using this measure are consistent with the findings of the most rigorous and comprehensive study of ability grouping ever undertaken. This is the experimental investigation of elementary school children in New York City recently completed by Goldberg, Passow, and Justman, which produced the following generalization:

> The general conclusion which must be drawn from the findings of this study and from other experimental grouping studies is that, in predominantly middle-class schools, narrowing the ability range in the classroom on the basis of some measure of general academic aptitude will, by itself, in the absence of carefully planned adaptations of content and method, produce little positive change in the academic achievement of pupils at any ability level.[45]

TABLE 8

SUMMARY EFFECTS OF FOUR DICHOTOMIZED CURRICULAR AND FACILITIES CHARACTERISTICS OF SCHOOLS ON MATH ACHIEVEMENT AND COLLEGE PLANS WITH FATHER'S EDUCATION AND SCHOLASTIC ABILITY SIMULTANEOUSLY CONTROLLED*

Independent Attributes	Weighted Effects of Independent Attributes on MATH†	Weighted Effects of Independent Attributes on College Plans‡
Accelerated curriculum for superior students.	.020	.028
Opportunity to obtain advanced placement and/or credit in college.	.043	−.003§
Acceleration policy for graduation.	.032	−.017§
Percentage of teachers with more than B.A. degree.	.046	.083

* Results are based on total sample of students, not the 10 per cent subsample. All effect parameters are significant at the .01 level unless otherwise noted.

† Father's education and ability have approximately constant effects on MATH with each of the four independent attributes controlled. The effects of father's education vary from .142 to .151 and those of ability from .325 to .327.

‡ Father's education and ability have approximately constant effects on college plans with each of the four independent attributes controlled. The effects of father's education vary from .237 to .243 and those of ability from .178 to .184.

§ Not significant at the .05 level.

The four remaining characteristics in Table 7 have significant effects in the expected direction on both dependent attributes, and Table 8 presents the effects of these four on the dependent attributes for

[44] David J. Fox, *Expansion of the More Effective School Program* (New York: Center for Urban Education, September, 1967), p. 121. In this evaluation study, a distinction was made between average class size and pupil-teacher ratio (see p. A-1). The former was defined as number of pupils in school divided by number of organized classes, whereas pupil-teacher ratio was obtained by dividing the number of students in school by the total number of authorized teaching positions. To obtain an indication of the large differences between the experimental and control schools on these two measures, consider the following data for October, 1966, the termination date of the evaluation: The average class size in the control schools was 28.5 and only 20.1 for the More Effective schools. Pupil-teacher ratio in the former was 22.2 and only 12.3 in the latter.

[45] Miriam L. Goldberg, A. Harry Passow, and Joseph Justman, *The Effects of Ability Grouping* (New York: Teachers College Press, 1966), p. 167. This conclusion is also supported at the national level by the results of Coleman *et al.*, *op. cit.*, p. 314, who found that ability grouping at the school level accounted for almost no variance in verbal achievement with family background of students controlled.

the total sample of students with ability and family SES both controlled. A comparison of these data with those of Table 7 indicates that, although all four characteristics exert a significant effect on MATH (and the same holds true for two of them with respect to college plans), much of their apparent explanatory power is attributable to family status and ability level of students. Consequently, they have only very limited substantive influence on students' academic behavior.[46] The one exception to this statement is the effect of teachers' educational level on college plans and, to a lesser extent, on students' MATH performance. The level of formal education of teachers may be viewed as one indicator of the academic competence of the staff, a variable which previous research has shown to be related to student performance.[47]

Rather than presenting several tables which show (*a*) the independent effects of each of the four curricular and facilities measures listed in Table 8 on the two dependent attributes when the climate dimensions, father's education, and ability are simultaneously controlled and (*b*) the independent effects of each of the climate dimensions on the dependent attributes with each of the four curricular measures, father's education and ability simultaneously controlled, the important results may be summarized as follows.

1. The small, statistically significant effects of accelerated curriculum on college plans disappear when each of the six climate dimensions is held constant. However, the effects of each of the climate dimensions are unaffected by controlling accelerated curriculum.

2. The significant effects of educational level of teachers on college plans are unaffected by controlling each climate dimension. Likewise, the effects of each climate dimension are almost totally independent of the educational level of faculty.

3. The small, significant effects of accelerated curriculum on MATH disappear when each of the climate dimensions is controlled. On the other hand, the effects of the six climate dimensions on MATH are not reduced when accelerated curriculum is held constant.

4. The statistically significant effects of advanced placement in college on MATH scores disappear when climate dimensions II, III, V, and VI are held constant; although they remain statistically significant when dimensions I and IV are controlled, they are reduced by approximately 50 per cent. On the other hand, the effects of each climate dimension on MATH are reduced only a minute amount when the effects of advanced college placement are removed.

5. The small, statistically significant effects of an acceleration policy for graduation on MATH disappear when each of the six climate dimensions is controlled. However, the effects of all the climate dimensions remain almost totally intact when graduation policy is controlled.

6. In general, the effects of average educational level of teachers on MATH remain intact when each climate dimension is held constant. Likewise, the influence of each factor dimension on MATH is not appreciably reduced when teachers' educational level is controlled.

These findings, taken together, strongly suggest that none of these four organizational properties of schools is an important source of variation in climate effects on students' academic behavior, since controlling them has no appreciable influence on the magnitude of the relationships between the six climate dimensions and the dependent attributes. However, controlling

[46] These findings are in general accord with those of Coleman *et al.*, *op. cit.*, pp. 312–16. Twelve different characteristics of school facilities (similar to those considered in this section) accounted for only a small amount of variance in individual students' achievement when their family background differences were controlled.

[47] Coleman *et al.*, *op. cit.*, p. 318, found that quality of teachers, as measured by scores on a standardized test measuring verbal skills, had a substantially stronger effect on students' achievement than did physical facilities and curricular measures.

the effects of the climate dimensions tends to result in the disappearance of the limited effects of these characteristics on the dependent attributes. The one exception to this generalization is the effect of teachers' educational level on both MATH achievement and college plans.

Perhaps the small effects of curricular and facilities characteristics on achievement and educational plans are a *consequence* of variation in community involvement and interest in academic excellence of the schools. That is, schools located in neighborhoods or communities with a strong *social* commitment to quality education for their children are more likely to institute pedagogical innovations and to attract highly competent teachers than communities lacking such a social investment in the quality of education. Although there are no comprehensive measures of community interest available in the present study to test such a proposition, the scale measuring parental involvement in the high school, P.I.H.S., can serve as an indicator of this phenomenon. To test the proposition adequately would require a protracted longitudinal study of communities and their schools, rather than the cross-sectional approach employed here. However, a necessary condition for the proposition to have validity is that there be positive correlations between these curricular and resource characteristics of the schools and P.I.H.S. Each of the four characteristics (indicated in Table 8) has a significant relationship ($.05 > p$) with P.I.H.S.:

	Product-Moment Correlation
Accelerated curriculum..........	.423
Advanced placement............	.494
Accelerated graduation..........	.635
Educational level of teachers....	.470

Of course, it is possible that P.I.H.S. is generated by school policy and quality and is, therefore, a consequence of such characteristics rather than a source of them. However, it seems, for example, more plausible to argue that competent teachers (as indicated by level of formal education) are attracted to schools in communities where the residents (especially the parents) and school officials are socially committed to quality education than the converse.[48] Of course, each of these statements is undoubtedly an oversimplification of the complex causal process involved, with a two-dimensional or "feedback" causal relationship being more accurate.[49]

More evidence to suggest the validity of the argument that the extent of the collective parental and community support is one source of variation in the small influence of various indicators of curriculum and facilities on students' achievement and educational plans is found in Table 9, which is identical to Table 8 except that P.I.H.S. is also held constant. A comparison of the effect parameters in the two tables indicates that holding constant P.I.H.S. (1) "washes out" the small effects of an acceleration policy for graduation on MATH achievement, (2) reduces the small effects of advanced college placement on MATH scores, and (3) reduces the small effects of teachers' education

[48] That economic investment on the part of the community (as contrasted to *social* investment) is not important in this sample in recruiting quality teachers is evidenced by the fact that the product-moment correlation at the school level between beginning teacher salaries and percentage of teachers with more than the B.A. degree is $-.11$ ($.50 > p > .30$).

[49] Stated in different terms and at a more general level, school environment and community support interact: communities or neighborhoods with a strong, collective social investment in quality education tend to generate school environments conducive to high educational aspirations and achievement, and these schools attract families to the community who have a strong commitment to quality education. For examples of two-directional relationships involving research on school environments and academic behavior, see Edward L. McDill and James S. Coleman, "High School Social Status, College Plans, and Interest in Academic Achievement: A Panel Analysis," *American Sociological Review*, XXVIII (1963), 905–18; and Jerome Kirk, "Cultural Diversity and Character Change at Carnegie Tech" (a report on the Carnegie Tech Campus Study, Carnegie Institute of Technology, Pittsburgh, 1965), p. 40.

on MATH to a point of little substantive significance even though the parameter remains statistically significant.

In sum, the extent of parental and community interest in the school generally functions as a factor accounting for the small net impact of curriculum and facilities on academic behavior of students.

EDUCATIONAL IMPLICATIONS OF FINDINGS ON SOURCES OF CLIMATE EFFECTS

The results of the preceding section, indicating that the critical factor in explaining the impact of the high school environment

TABLE 9

SUMMARY EFFECTS OF FOUR DICHOTOMIZED CURRICULAR AND FACILITIES CHARACTERISTICS OF SCHOOLS ON MATH ACHIEVEMENT WITH FATHER'S EDUCATION, SCHOLASTIC ABILITY, AND P.I.H.S. SIMULTANEOUSLY CONTROLLED*

Independent Attributes	Weighted Effects of Independent Attributes on MATH†	Weighted Effects of Independent Attributes on College Plans‡
Accelerated curriculum for superior students	§	§
Opportunity to obtain advanced placement and/or credit in college	.031	**
Acceleration policy for graduation	.018††	**
Percentage of teachers with more than B.A. degree	.026	.061

* Results are based on total sample of students, not the 10 per cent subsample. All effect parameters are significant at the .01 level unless otherwise noted.

† The effects of father's education, ability, and P.I.H.S. on MATH are almost invariant with each of the three independent variables controlled in this column. The effects of father's education vary from .128 to .132, those of ability from .316 to .319, and those of P.I.H.S. from .092 to .097.

‡ The effects of father's education, ability, and P.I.H.S. with the measure of teachers' level of education controlled are .217, .171, and .096, respectively.

§ The positive relationship between accelerated curriculum and P.I.H.S. is sufficiently pronounced that there are no schools with an accelerated curriculum and *low* on P.I.H.S. Consequently, the effects of the former attribute on MATH and college plans with P.I.H.S., father's education, and ability simultaneously controlled cannot be computed.

** Effects of these independent attributes on college plans were not computed because there is no significant relationship between them and college plans with ability and father's education simultaneously controlled (see Table 8).

†† Not significant at the .05 level.

on the achievement and educational aspirations of students is the degree of parental and community interest in quality education, would appear to have policy implications. The results seem to support a plea recently made by the U.S. Commissioner of Education:

> In all communities—rural and suburban, but especially inner-city—the principal needs to take the initiative in tailoring his school to the character of the community. He needs to solicit parent participation and to help parents understand what kinds of contributions they can make. The principal ought to be welcoming parents and letting them see how the school is run and explaining to them its policies and programs. He should at the same time be converting the school into a community resource that offers adults a center for community activities, for instruction in practical subjects as well as leisure-time activities.[50]

At present there are innovations under way of the type advocated by the commissioner—innovations which need to be carefully evaluated over long periods by educational researchers for their potentially positive benefits. The results of the present investigation suggest that these innovations, if kept free of social tensions, might set in motion the feedback mechanism of "parental involvement–intellectually viable school environment" discussed above.

One such innovation is the "community school," which is designed to serve as a community service center where neighborhood residents may obtain health services, counseling services, legal aid, and employment information. In short, the school is conceived as one of the prime loci of community or neighborhood life.[51]

[50] Harold Howe II, "Picking Up the Options" (address to the Annual Meeting of the Department of Elementary School Principals, National Education Association, Houston, Texas, April 1, 1968), p. 13.

[51] Baltimore City Public Schools, School-Community Relations Division, Bulletin No. 1 (August, 1967). See also, "Reconnection for Learning: A Community School System for New York City" (report of the Mayor's Advisory Panel on Decentralization of the New York City Schools). New York: The Advisory Panel, 1967.

A concept, related to the community school, is the *proposed* experimental program "Family Opportunities for Reaching Goals through Education" (FORGE), currently being designed by the Office of Special Programs, Franklin and Marshall College, Lancaster, Pennsylvania.[52] Under this proposed program, poverty neighborhoods and their accompanying schools would be defined and then used as the basic units of the program. The purpose of the program is to encourage the parents of selected elementary school children to become closely involved in their children's education and their local schools. In addition to long-term advisory and support services, the project staff would guarantee total five-year college expenses for each child accepted by a college upon completion of high school. The primary locus of the program would be the neighborhood under the leadership of a resident director. His chief responsibilities would be to provide long-term advice and counsel to parents regarding resources and limitations of neighborhood schools, to promote student and parental involvement in the schools' policies and programs, and to counsel students individually regarding their educational needs and how they can be met. Hopefully, such a program would create the intellectual and social camaraderie between schools and families which appears to be the hallmark of schools with strong academic climates.

As documented by Bloom in his major work, *Stability and Change in Human Characteristics*,[53] highly consistent home environments have more potent effects on cognitive development than those lacking internal consistency. He generalizes this to the relationship between schools and homes: school and home environments which are mutually reinforcing are likely to achieve greater academic growth of students than those lacking such consistency.[54] It would seem that "community schools" and similar organizational innovations in public institutions could perhaps be one mechanism for obtaining support of parents which in turn could provide data for both parents and school officials to achieve consistency between the two environments.

CONCLUSIONS

In their summary of the follow-up study of Project Talent high school seniors in 1963 who were ninth graders in the original survey in 1960 (conducted by Shaycoft[55]) the authors conclude that there is a substantial amount of academic growth by students during the high school years and that the schools are of importance in accounting for varying rates of growth. They state:

> In summary, schools do vary in effectiveness, but the specific school characteristics that produce results are somewhat elusive. One reason they are so resistent to identification may be that they are elusive *inherently*, not just in the present context. In other words, one of the crucial differences between an effective school and an ineffective one may be something as vague as the *school's atmosphere* [italics supplied]. A school may provide an atmosphere where the motivation to learn is stimulated or it may provide one that produces students whose goal is to "get by." This sort of information cannot be gathered through a questionnaire survey.[56]

The present authors find themselves in agreement with the first of the two major points in the above quote. The evidence from the present research indicates that the educational and social environment of the school does have a moderate effect on the

[52] Project FORGE, Office of Special Programs, Franklin and Marshall College, Lancaster, Pa., 1968 (mimeograph).

[53] Benjamin Bloom, *Stability and Change in Human Characteristics* (New York: John Wiley & Sons, 1964).

[54] Bloom, "Stability and Change . . . " (see n. 2 above), p. 46.

[55] Marion F. Shaycoft, *Project Talent, The High School Years: Growth in Cognitive Skills* (American Institutes for Research and School of Education, University of Pittsburgh, 1967).

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In sum, the extent of parental and community interest in the school generally functions as a factor accounting for the small net impact of curriculum and facilities on academic behavior of students.

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The results of the preceding section, indicating that the critical factor in explaining the impact of the high school environment on the achievement and educational aspirations of students is the degree of parental and community interest in quality education, would appear to have policy implications. The results seem to support a plea recently made by the U.S. Commissioner of Education:

> In all communities—rural and suburban, but especially inner-city—the principal needs to take the initiative in tailoring his school to the character of the community. He needs to solicit parent participation and to help parents understand what kinds of contributions they can make. The principal ought to be welcoming parents and letting them see how the school is run and explaining to them its policies and programs. He should at the same time be converting the school into a community resource that offers adults a center for community activities, for instruction in practical subjects as well as leisure-time activities.[50]

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academic behavior of students. However, they cannot accept the second point that adequate measures of school environment cannot be obtained through survey techniques. At the college level there is a substantial body of research—based to a considerable extent on survey techniques—focusing on the kinds of college environments which are conducive to academic achievement and aspirations. The results on climate effects presented in this paper are consistent with the general tenor of findings from other studies at both the high school and college levels, using both survey techniques and other approaches, such as the interview and both participant and non-participant observation.[57] Of course, none of the studies to date has presented conclusive information on the nature of the academic environment of schools which would form the basis for incontrovertible policy prescriptions to school administrators as how to promote particular types of cognitive development in students. Nevertheless, the results of this and other recent studies offer substantial evidence that there are "overachieving" and "underachieving" schools. They point also to the need for more intensive studies of such deviant institutions. Such research should produce, in the foreseeable future, systematic evidence on "the realities of the teaching-learning process as they actually are and as they might be."[58]

Johns Hopkins University
Vanderbilt University
Dartmouth College

[57] See Boocock, *op. cit.*, pp. 24–31, for some of the more important work in this area.

[58] Henry S. Dyer, "School Factors and Equal Educational Opportunity," *Harvard Educational Review*, XXXVIII (Winter, 1968), 55.

Schools and the Stratification Process[1]

Robert M. Hauser

ABSTRACT

The influence of socioeconomic origins on three outcomes of secondary schooling (academic achievement, course marks, and aspirations) is interpreted using path analysis and covariance analysis for six grade-cohorts of students in the Nashville Standard Metropolitan Statistical Area (SMSA). While we have had some success in constructing recursive interpretations of background influences on educational outcomes, the proposed models do not account for the common content of similarly determined outcomes. Intelligence plays a key role in mediating the influence of social origins on educational outcomes. Further, there is little evidence that teachers discriminate by socioeconomic origin. School differences in educational outcomes are small and may be artifactual consequences of differences in student-body composition. High school quality is less persistent over time than student-body composition, and plays a minor role in the determination of ultimate educational attainment.

Schooling may be interpreted as one stage in the lifelong stratification process or "socioeconomic life cycle."[2] For an individual enrolled in school, the process extends back in time to antecedents in school, neighborhood, and home, and forward in time to further schooling and to social and economic achievement. The central role of schooling in the stratification process gives educational opportunity its theoretical and practical importance.

In some studies of the role of schooling in the stratification process, educational performance at maturity is interpreted as a mechanism of intergenerational status transmission.[3] In others, the influence of origin status on educational performances is assessed at some fixed point in the schooling process.[4] In the former, a single index of performance in the school system, usually educational attainment (years of schooling completed), is treated as a variable which intervenes between origin status and adult achievement. In the latter, a variety of indexes of educational performance are examined and there is a greater interest in the operation of the school system. While the two approaches use different indicators of educational performance and treat different portions of the socioeconomic life cycle, we should expect them to lead to complementary conclusions. It is disconcerting, then, that a definitive study of occupational stratification pictures education as loosening the ties between status origins and destinations, while recent school studies present the opposite interpretation.

[1] The author expresses his thanks to Albert J. Reiss, Jr., and Albert Lewis Rhodes for making available the data for this analysis and to Otis Dudley Duncan, Albert J. Reiss, Jr., Edward O. Laumann, Jerald G. Bachman, G. F. Peaker, and C. Arnold Anderson for their criticism and advice. I also want to express my thanks to the editors and staff of the *Journal* for their editorial assistance. Tabulations were carried out at the University of Michigan and Brown University with the help of Mike Coble and Roy Kass. An earlier version of this paper was read at the sixty-third Annual Meeting of the American Sociological Association, August, 1968.

[2] Otis Dudley Duncan, "Discrimination Against Negroes," *The Annals* (January, 1967), pp. 87–89.

[3] An early and still valuable presentation is found in Pitirim A. Sorokin, *Social Mobility* (New York: Harper & Bros., 1927) pp. 169–72 and 187–93. A recent and comprehensive treatment is given in Peter M. Blau and Otis Dudley Duncan, *The American Occupational Structure* (New York: John Wiley & Sons, 1967).

[4] The most recent and massive of such studies is described in James S. Coleman, Ernest Q. Campbell, *et al.*, *Equality of Educational Opportunity* (Washington: U.S. Office of Education, 1966), which is generally known as the *Coleman-Campbell Report.*

INTERGENERATIONAL MOBILITY AND SCHOOL STUDIES

The knowledge we have obtained from the first approach may be summarized by reference to the "basic model" of occupational achievement proposed by Blau and Duncan.[5] In the model it is assumed that a father's educational and occupational statuses influence his son's educational attainment and occupational achievement and that the son's educational attainment also influences his occupational achievement. For the cohorts covered in the 1962 survey, "Occupational Changes in a Generation," the influence of socioeconomic origin on the son's occupational achievement is mainly indirect, via educational attainment. That is, a father's educational and occupational statuses are only a minor handicap or advantage, once schooling has been completed. Moreover, most of the variation in the son's educational attainment must be explained by factors unrelated to his social origins. Thus, the effect of educational attainment on occupational achievement nearly exhausts the influence of social origins on occupational achievement. At the same time, it is largely an indication of the impact of educational opportunity on occupational achievement. With some variations in detail, the same pattern holds as additional indicators of socioeconomic background are introduced, and it holds for cohorts completing school throughout the present century.[6]

While the Blau and Duncan model interprets education as both mediating and diminishing the effects of social origins on adult achievement, recent school studies emphasize the role of the educational system in restricting intergenerational mobility. Indeed, since the publication of the *Coleman-Campbell Report*—which documented racial and ethnic differentials in academic achievement in elementary and secondary schools—criticizing the failures of the schools has become a minor industry. Although the persistence of racial differentials in academic achievement at given grade levels cannot be ignored, the prospective gain in social and economic achievement resulting from their elimination cannot be great. It cannot be measured in the absence of a model relating grade-level performance to adult social and economic achievement.[7] Still, a typical assessment is that by Clark, who states, "American public schools have become significant instruments in the blocking of economic mobility and in the intensification of class distinctions rather than fulfilling their historic function of facilitating such mobility."[8]

In part, the conflict of these interpretations may be explained by the plurality of issues which are treated in the studies of schools. Critics of the schools are variously concerned with (*a*) the distributive function of schooling in promoting social mobility for individuals; (*b*) the content of the educational experience, without regard for the differential performance of individuals or groups; and (*c*) the control of the schools as a symbolic or tactical political objective. More important, restricting our interest to the first of these concerns, which is the most relevant to a distributive interpretation of

[5] Blau and Duncan, *op. cit.*, chap. v.; C. Arnold Anderson, "A Skeptical Note on Education and Mobility," in A. H. Halsey, Jean Floud, and C. Arnold Anderson (eds.), *Education, Economy, and Society*, (New York: Free Press, 1961), pp. 164–79; Otis Dudley Duncan and Robert W. Hodge, "Education and Occupational Mobility: A Regression Analysis," *American Journal of Sociology*, LXVIII (May, 1963), 629–44; Otis Dudley Duncan, David L. Featherman, and Beverly Duncan, *Socioeconomic Background and Occupational Achievement: Extensions of a Basic Model*, in Final Report, Project No. 5-0074 (EO-191) (Washington: U.S. Department of Health, Education, and Welfare, Office of Education, Bureau of Research, 1968), chap. iv; Beverly Duncan, "Education and Social Background," *American Journal of Sociology*, LXXII (January, 1967), 366–68.

[6] Beverly Duncan and Duncan, Featherman, and Duncan, *op. cit.*

[7] Otis Dudley Duncan, "Inheritance of Poverty or Inheritance of Race," in Daniel P. Moynihan (ed.), forthcoming; Samuel Bowles, "Towards Equality of Educational Opportunity?", *Harvard Educational Review*, XXXVIII (Winter, 1968), 88–99.

[8] Kenneth B. Clark, "Alternative Public School Systems," *Harvard Educational Review*, XXXVIII (Winter, 1968), 101.

the stratification process, there are several inadequacies of the school studies which may help to account for the conflicting interpretations. These include (1) the absence of a theoretical rationale for the indicators of educational performance; (2) the absence of criteria for adequate explanation; (3) a misleading preoccupation with school quality; and (4) the absence of linkage with later events in the stratification process. Each of these points is developed briefly below.

1. There is no measure or set of measures of educational performance at a given grade level whose relationships with origin status provide unequivocal evidence that there is more of less equality of educational opportunity. Among the heterogeneous measures which have been used in this way are intelligence quotients; academic achievement test scores, course marks; educational and occupational aspirations, expectations, or plans; and a variety of less easily described attitudinal variables.[9] There has been relatively little effort to order these indicators in a model of the relevant portions of the achievement process. In the absence of such a model, interpretation of the indicators is equivocal.

2. Analyses of educational opportunity have usually stopped with the demonstration that differentials in educational performance are associated with one or more socioeconomic background characteristics. There are, however, great differences in the interpretations which can legitimately be made from given findings: (*a*) that some postulated relationship exists; (*b*) that it is of a given magnitude; and (*c*) that it is a sufficient explanation of the phenomenon of interest. While interpretations of the influence of social origins on educational performance frequently require evidence of the third kind, they are usually based on evidence of the first or second kind.[10] For example, we do not know whether socioeconomic background would account for the observed similarity of a student's scores in more than one measurement of an educational performance, but the assumption that this is the case is implicit in the statement that socioeconomic background "explains" educational performance.

3. There has been a preoccupation with the effects of the quality of the school,[11] due,

[9] Consider, for example, the shotgun approach to measurement of educational inequalities of the *Coleman-Campbell Report* and its argument for using a verbal component of ability as a criterion of academic achievement (pp. 292–95).

[10] Classic overinterpretations of such relationships are contained in Patricia Cayo Sexton, *Education and Income* (New York: Viking Press, 1964) and A. B. Hollingshead, *Elmtown's Youth* (New York: John Wiley & Sons, 1949), pp. 163–203. Or consider the statement of Warner, Havighurst, and Loeb: "The American school . . . reflects the socioeconomic order in everything that it does; in what it teaches, whom it teaches, who does the teaching, who does the hiring and firing of teachers, and what the children learn in and out of the classroom," in *Who Shall Be Educated?* (New York: Harper & Bros., 1944), chap. xii. Comprehensive reviews of the literature pertaining to this approach have been undertaken by David E. Lavin, *The Prediction of Academic Performance* (New York: Russell Sage Foundation, 1965), pp. 122–34, and by Sarane S. Boocock, "Toward a Sociology of Learning: A Selective Review of Existing Research," *Sociology of Education*, XXXIX (Winter, 1966), 1–46.

[11] The search for such effects was a primary goal in the *Coleman-Campbell Report*, esp. pp. 290–325. For a discussion of the findings of the *Coleman-Campbell Report* in this regard, see Samuel Bowles and Henry M. Levin, "The Determinants of Scholastic Achievement—An Appraisal of Some Recent Evidence," *Journal of Human Resources*, III (Winter, 1968), 3–24, and Coleman's "Reply to Bowles and Levin," *Journal of Human Resources*, III (Spring, 1968), 237–46. See also Richard P. Boyle, "The Effect of the High School on Students' Aspirations," *American Journal of Sociology*, LXXI (May, 1966), 628–39; Richard P. Boyle, "On Neighborhood Context and College Plans (III)," *American Sociological Review*, XXXI (October, 1966), 706–07; James S. Coleman, *The Adolescent Society* (New York: Free Press, 1962); Irving Krauss, "Sources of Educational Aspirations among Working Class Youths," *American Sociological Review*, XXIX (December, 1964), 867–79; Edward L. McDill, Edmund L. Meyers, and Leo C. Rigsby, "Institutional Effects on the Academic Behavior of High School Students," *Sociology of Education*, XL (Winter, 1967), 181–99; John A. Michael, "High School Climates and Plans for Entering College," *Public Opinion Quarterly*, XXIV (Winter, 1961), 585–95; John A. Michael, "On Neighborhood Context and College Plans (II),"

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Investigations of school quality have been unproductive for at least two reasons: (*a*) School effects are small; there is a great deal of overlap among student bodies in educational performance and the processes determining educational performance are much the same in any school. School effects contribute little to the explanation of individual differences in educational performance which are presumably to be explained in distributive approaches to educational stratification.[14] (*b*) Differences in educational performance from school to school are not necessarily indicative of differences in school quality because student bodies differ in composition on factors which are operative prior to educational performance. Net school differences may be identified with school quality after composition on prior factors has been controlled statistically. The imposition of such controls is tantamount to the adoption of a model for the determination of educational performance within schools. While the necessity to control for student-body composition has been recognized in many studies of school quality, the substantive meaning of such controls has often been ignored.

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[13] Daniel P. Moynihan, "Sources of Resistance to the Coleman Report," *Harvard Educational Review*, XXXVIII (Winter, 1968), 30.

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One way to link performance in school with adult achievement would be to supplement or supplant the measure of educational attainment in the Blau and Duncan model with a direct measure of the quality of educational performance at maturity: mental ability, an achievement test score, or the like. For such a modification to alter the initial interpretation, it would have to be demonstrated that (*a*) more of the influence of origin status on adult achievement would be mediated by a measure of educational quality than by one of educational attainment and that (*b*) a measure of educational quality would have a larger effect on adult achievement than does educational attainment. It is already known, however, that origin status has only a modest direct effect on adult achievement when educational attainment is included in the model. Moreover, there is a sufficient correlation between performance in school and continuation in school for the quality and quantity of education to be redundant predictors of adult social and economic achievement.[17] Differential performance at a given grade level is obviously not related to educational attainment at that grade level, where the latter is constant by definition. But the same observation does not apply to the relationship between educational attainment and performance measured at maturity. Thus, it is unlikely that the inclusion of additional measures of educational performance would alter the interpretation offered by Blau and Duncan.

In short, we do not have an interpretation which is consistent with the basic interpretative framework of the Blau and Duncan model for the relationships between educational performance at given grade levels and its antecedents or consequences in the stratification system. The remainder of this paper describes some efforts to develop consistent interpretations of the influence of socioeconomic origins on three outcomes of secondary schooling: academic achievement, course marks, and aspirations. Two measures of each outcome are available, and the investigation takes the form of an effort to construct recursive multivariate causal models which can account for their common content. The use of hypothetical variables in the evaluation of alternative interpretations is illustrated. Gross and net school effects are examined and problems in their interpretation are discussed. Finally, an explicit speculation is offered about the relationship between high school quality and ultimate educational attainment.

DATA AND METHODS

The analysis is based on data for nearly 17,000 white public school students enrolled in grades seven to twelve in Davidson County, Tennessee, in 1957.[18] Davidson County includes Nashville, Tennessee, and its suburbs. Coverage of the population was reported to be excellent. Thirty-four schools are represented at the seventh- and eighth-grade levels, twenty-one at ninth-, and seventeen each at tenth-, eleventh-, and twelfth-grade levels.

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based on simple arithmetic averages of statistics which were originally computed separately for subgroups defined by sex or grade level. While there is no formal justification for this procedure, the results probably differ little from those which would have been obtained using more complex averaging methods.[19] The use of averages provides a parsimonious basis for the evaluation of alternative models with stable data, and it does not distort any major results. Still, the results are not directly applicable to either sex at any grade level, and there are undoubtedly real differences among the subgroups.

The reduction and interpretation of the data have been carried out using covariance and path analysis. In the analysis of covariance, each variable is partitioned into orthogonal within-school and between-school components. Under the assumption that school effects are additive, it is possible to develop models for the determination of educational outcomes within schools and to assess the implications of those models for the determination of school differences in educational outcomes.[20] Path analysis is a method for the quantitative interpretation of multiple causal assumptions about linear and additive relationships among variables that is more general than multiple correlation and regression analysis.[21]

A tentative ordering of the variables included in the analysis is shown in Figure 1. It is assumed that the variables on the left side of the diagram may influence variables lying to the right either directly or through intervening variables. No variable is assumed to influence a variable on its left, and, with a single exception, relationships among variables in the same column are not analyzed in causal terms. At some points in the discussion it is assumed that

X = father's occupation (24.4)	M = Stanford Mathematics Grade Equivlent (19.6)	A = arithmetic mark (6.2)	
V = father's education (24.2)	Q = intelligence quotient (16.5)	E = English mark (6.1)	T = educational aspiration (11.4)
S = number of siblings (8.7)	W = Stanford Reading Grade Equivalent (15.8)	O = membership in school organizations (8.3)	
		Z = perceived father's educational expectation (24.6)	J = occupational aspiration (10.3)

FIG. 1.—Tentative causal ordering of variables in educational achievement process. Tentative causal ordering runs from left to right. Numbers in parentheses below descriptions of variables are percentages of variance which lie between schools averaged over grades 7–12 separately by sex. There is reason to believe that the figures for M and W are erroneously high. The figures for T are for students in the Davidson County Ring surrounding Nashville and those for J are for boys in the Ring.

[19] Helen M. Walker and Joseph Lev, *Statistical Inference* (New York: Holt, Rinehart, & Winston, 1953), p. 301.

[20] Walker and Lev, *op. cit.*, chap. xv; James Fennessey, "The General Linear Model: A New Perspective on Some Familiar Topics," *American Journal of Sociology*, LXXIV (July, 1968), 1-27; Karl Schuessler, "Covariance Analysis in Sociological Research," in Edgar F. Borgotta (ed.), *Sociological Methodology, 1969* (San Francisco: Jossey-Bass, 1969) pp. 219–44.

[21] Otis Dudley Duncan, "Path Analysis: Sociological Examples." *American Journal of Sociology*, LXXII (July, 1966), 1–16.

the three socioeconomic background variables (X, V, and S) influence intelligence (Q). Elsewhere, the correlation between socioeconomic background and intelligence is not analyzed, and intelligence is assumed to influence academic achievement (M and W).

Course marks (A and E), academic achievement (M and W), and intelligence (Q) were obtained from school records. For all students except those in the seventh grade, marks and achievement test scores were obtained in the eighth grade. The intelligence quotients were based on Otis or Kuhlman-Anderson group administered tests; the small variation in the age at which ability was measured does not affect the results in any important way. The remaining variables come from group-administered questionnaires. Father's education (V), perceived father's educational expectation (Z), and educational aspiration (T) were coded in "years of schooling." Father's occupation (X) and occupational aspiration (J) were expressed as scores on Duncan's index.[22] Organizational membership (O) is the number of extracurricular organizations to which the student belonged. The two aspiration measures (T and J) were ascertained only for the two-thirds of the students who attended schools in the ring surrounding the city of Nashville.

GROSS SCHOOL EFFECTS

The figures in parentheses below each variable in Figure 1 are squared correlation ratios (E^2_{YX}), or the proportions of variance which lie between schools. They indicate the degree to which student bodies are segregated by socioeconomic background, ability, and educational outcomes; they are upper limits on the effects of the school attended. About one-fifth of the variance in academic achievement, one-ninth of the variance in aspirations, and one-sixteenth of the variance in course marks may be attributed to the gross effect of the school attended. One may suspect the size of these figures because they apply to a single metropolitan area population; however, they are comparable to findings for larger populations in which school and racial differences are not confounded.[23] It may be noted in passing that the limited size of the Nashville figures is apparently not a consequence of student mobility from school to school, for they are essentially the same when recalculated for students who did not move within four years.

The interpretation of school effects as unit organizational properties with cumulative impact on the student implies that they should persist in time. The cross-sectional Nashville data do not permit a direct test of this hypothesis. It is possible, however, to construct synthetic grade-cohorts by pairing cohorts in different grades who attended schools in the same feeding channel, for example, to match the tenth-grade class in a high school with the ninth-grade classes in its junior high feeder schools. Each pair of observations was weighted by the number of students in the lower grade-cohort.

The first column of Table 1 contains the averages of the intercorrelations of school means for the five adjacent pairs of grade-cohorts (7–8, 8–9, 9–10, 10–11, and 11–12). The intercorrelations for student background characteristics (father's occupation, father's education, and number of siblings), intelligence, father's educational expectation, and student's educational and occupational aspirations, might be classified as moderate to high. Those for achievement test scores and course grades are lower. In the case of achievement test scores, the synthetic intragrade-cohort correlations are probably low because of an error in the data. Some indication of the size of the true intercorrelations for achievement test scores may

[22] Albert J. Reiss, Jr., with the collaboration of Otis Dudley Duncan, Paul K. Hatt, and Cecil C. North, *Occupations and Social Status* (New York: Free Press, 1961).

[23] Coleman *et al.*, *op. cit.*, pp. 295–99; and Robert M. Hauser, *Family, School, and Neighborhood Factors in Educational Performances in a Metropolitan School System* (unpublished Ph.D. dissertation, Department of Sociology, the University of Michigan, 1968), chap. iv.

be obtained by a comparison of the first and second columns of Table 1. The second column contains the averages, over grades in school, of the correlations between variable means for boys and for girls attending the same schools. These are generally larger than the synthetic intragrade-cohort correlations. The intersex correlations for academic achievement are about as large as the intersex and synthetic intragrade-cohort correlations for intelligence.

The persistence of school effects cannot be taken for granted from the results in the first two columns of Table 1. If intracohort change in student characteristics could be described by a simple causal chain, then the intercorrelation between school means from grade seven to grade twelve would be equal to the product of the intervening intracohort correlations. For example, if each correlation for adjacent grade levels were equal to 0.9, the seventh- to twelfth-grade intercorrelation would be $(0.9)^5 = 0.59$.

At the same time, additional analysis suggests that there is more persistence over long periods than one might infer from the intercorrelations for adjacent grade-cohorts. The observed intercorrelations from grade seven to grade twelve are shown in the third column of Table 1, and the correlations expected from a simple causal chain model are shown in the fourth column. The latter figures are taken to be the fifth powers of the entries in the first column. Using these "smoothed" figures, we obtain higher expected values than would be generated by the actual products of the correlations for adjacent cohorts. The causal chain model provides a reasonably good fit only for organizational membership and student aspirations. In every other instance, the observed correlations from the seventh to the twelfth grade are larger than the (artificially inflated) expected values. This conclusion holds for student background characteristics as well as for educational outcomes. Thus, we can hypothesize that the persistence of school means results from a persistence of student-body composition rather than any property of the schools' internal organization. It will be possible to return to this issue at a later stage of the analysis.

TABLE 1

STABILITY OF SCHOOL MEANS[a]

Variable	Average Intergrade Correlation	Combined Intersex Correlation	Stability from Grade 7–12	
			Actual	Expected
X	0.913	0.924	0.760	0.634
V	0.915	0.924	0.772	0.641
S	0.786	0.821	0.508	0.300
Q	0.749	0.848	0.619	0.236
M	0.400	0.912	0.043	0.010
W	0.369	0.894	0.167	0.006
A	0.428	0.742	0.164	0.014
E	0.644	0.805	0.141	0.111
Z	0.903	0.886	0.745	0.600
O	0.624	0.851	0.055	0.095
T: county	0.834	0.717	0.406	0.403
J: county	0.707	0.375	0.191	0.177

NOTE.—Item identifications: *X* = father's occupation; *V* = father's education; *S* = number of siblings; *Q* = intelligence; *M* = Stanford Mathematics Grade Equivalent; *W* = Stanford Reading Grade Equivalent; *A* = arithmetic mark; *E* = English mark; *Z* = father's educational expectation; *O* = organizational membership; *T* = educational aspiration; and *J* = occupational aspiration.

[a] See text for explanation.

If relationships among variables differed from school to school, the unique effect of each school could not be expressed by an additive constant as is required in covari-

ance analysis. That is, if such interacts with the other variables influencing educational performance, the statistical adjustment of school performance levels for student-body composition is equivocal. Some evidence on the size and persistence of non-additive school effects on several relationships is presented in Table 2. The assumption that each school has a unique relationship adds roughly two or three percentage points to the within-school variance explained by the average within-school regression lines. The size of these interactions does not appear to depend on the strength of the relationship examined. They are relatively large in the weaker relationships and relatively small in the stronger relationships.

The extent to which non-additive school effects can be interpreted is of as much interest as their size; the amount of information lost in the calculation of covariance adjustments using average within-school regression lines would not be large if departures from additivity were random. On the argument used above, persistence is a prior condition for the interpretation of school effects, and for that reason average intergrade-cohort and intersex correlations of regression coefficients were examined. These are reported in the last two columns of Table 2. There is little evidence of intersex consistency or of intercohort persistence of the within-school regression coefficients. On this evidence we assume that no substantial distortion of results is introduced by the calculation and interpretation of covariance adjustments.

TABLE 2

NON-ADDITIVITY OF SCHOOL EFFECTS

Relationship	Within-School Variance Explained by:		Average Correlation of Slopes	
	Average Regression (%)	Added by Interaction (%)	Intercohort	Intersex
W on *V*	2 79	3.10	0.008	0 108
W on *Q*.......	31.67	2 84	−0 103	0 071
E on *V*.......	2.95	1.76	−0.004	0 133
E on *Q*.... .	19.51	2.49	0 055	0 185
T on *V*........	6 33	1 81	0.167	−0 021
T on *Q*........	5.20	2 62	0 070	−0 065

Note.—Item identifications: *W* = Stanford Reading Grade Equivalent; *E* = English mark; *T* = student's educational aspiration; *V* = father's education; and *Q* = intelligence.

WITHIN-SCHOOL MODELS

Little more can be said about school effects without considering the determination of educational outcomes within schools. The entries below the diagonal of Table 3 are "average within-school" correlations among the variables (averaged, as described above, over all sex and grade level subgroups). The latter averaging process should not be confused with the use of average within-school correlations, which are computed from the deviations of individual scores from sex and grade level specific school means. Where school effects are additive, average within-school correlations provide an adequate description of the relationships among the variables without regard to school attended.

We begin by assuming that both *M* and *W*, the indicators of academic achievement, are influenced by the three socioeconomic background variables: father's occupation (*X*), father's education (*V*), and number of siblings (*S*). These assumptions are shown explicitly by the path diagram in Figure 2. The straight single-headed arrows represent

TABLE 3

WITHIN-SCHOOL CORRELATION MATRIX FOR ALL STUDENTS IN NASHVILLE SMSA AND BOYS IN DAVIDSON COUNTY RING

	X	*V*	*S*	*Q*	*M*	*W*	*A*	*E*	*Z*	*T*	*J*	*O*
X...		0.491	−0.148	0.209	0.146	0.210	0.126	0.167	0.246	0.195	0.210	0.136
V....	0.471		0.193	0.215	0.172	0.226	0.152	0.179	0.299	0.254	0.213	0.170
S....	−0.127	−0.187		−0.134	−0.112	−0.171	−0.082	−0.125	−0.189	−0.177	−0.173	−0.034
Q....	0.200	0.207	−0.146		0.529	0.530	0.400	0.432	0.212	0.235	0.282	0.174
M...	0.149	0.164	−0.123	0.560		0.613	0.502	0.483	0.233	0.268	0.252	0.169
W....	0.189	0.209	−0.169	0.574	0.644		0.386	0.458	0.283	0.267	0.276	0.183
A....	0.105	0.119	−0.116	0.361	0.487	0.368		0.630	0.180	0.202	0.238	0.138
E....	0.134	0.151	−0.141	0.416	0.486	0.463	0.569		0.219	0.272	0.292	0.153
Z....	......	......	......	......	......	......	......	......		0.642	0.342	0.184
T....	......	......	......	......	......	......	......	......	......		0.456	0.215
J....	......	......	......	......	......	......	......	......	......	......		0.185

Note.—Item identifications: *X* = father's occupation; *V* = father's education; *S* = number of siblings; *Q* = intelligence; *M* = Stanford Mathematics Grade Equivalent; *W* = Stanford Reading Grade Equivalent; *A* = arithemetic mark; *E* = English mark; *Z* = father's educational expectation; *T* = educational aspiration; *J* = occupational aspiration; and *O* = organizational membership. Correlations above the diagonal are for boys in the Davidson County Ring. Correlations below the diagonal are for all students in the Nashville SMSA. Missing correlations below the diagonal were not computed.

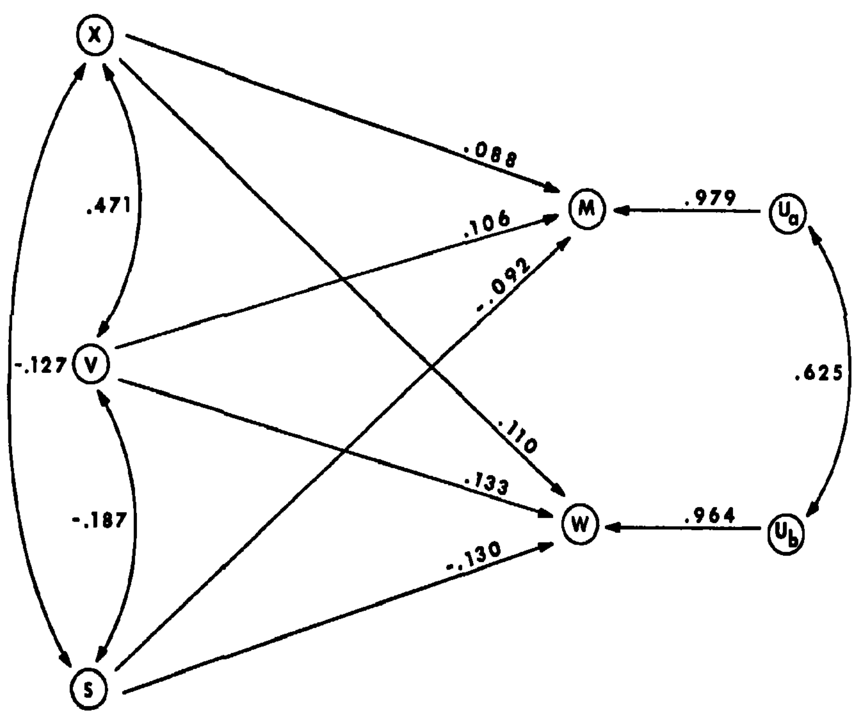


FIG. 2.—Determination of academic achievement by student background. Item identifications: *X* = father's occupation; *V* = father's education; *S* = number of siblings; *M* = Stanford Mathematics Grade Equivalent; *W* = Stanford Reading Grade Equivalent; U_a = unmeasured determinants of *M*; and U_b = unmeasured determinants of *W*.

direct causal influence. The curved two-headed arrows represent unanalyzed relationships; for example, the model in Figure 2 does not attempt to explain the relationships among X, V, and S. U_a and U_b are disturbances or residual determinants of M and of W, respectively. They are assumed to be uncorrelated with the prior determinants of M and of W, and, in combination with the prior variables, they completely determine M and W. The disturbances represent the effects of unmeasured variables, measurement error, statistical interaction, and nonlinearity.

The assumptions of Figure 2 can be expressed formally by the expressions

$$M = p_{MX}X + p_{MV}V + p_{MS}S + p_{Ma}U_a \quad (1)$$

and

$$W = p_{WX}X + p_{WV}V + p_{WS}S + p_{Wb}U_b, \quad (2)$$

where it is also assumed that

$$r_{Xa} = r_{Va} = r_{Sa} = r_{Xb} = r_{Vb} = r_{Sb} = 0 ,$$

and each of the variables is expressed in standard form (as the ratio of the deviation of a score from its mean to its standard deviation). The path coefficient p_{jk} is read as the net effect of variable k on variable j, while the order of subscripts on correlation coefficients is arbitrary. Path coefficients are obtained by repeated application of the basic theorem of path analysis to equations (1) and (2). The theorem states that

$$r_{ij} = \sum_k p_{ik} r_{kj} ,$$

where i and j are any two variables in the system and k runs over all variables from which direct paths lead to variable i.[24] Expressions for the six correlations of M and of W with the socioeconomic background variables may be solved simultaneously for the path coefficients for the socioeconomic background variables. Expressions for r_{MM}, r_{WW}, r_{Ma}, and r_{Wb} may be solved for the path coefficients of the residual variables. These expressions serve the dual function of permitting the identification of the path coefficients and providing an interpretation of the observed correlations among the variables in the system.

Under the stated assumptions, each of the socioeconomic background variables has a small direct effect on M and on W. The path coefficients (which are, in this case, identical with the beta coefficients of multiple correlation analysis), are interpreted as the net change in the dependent variable effected by a one standard deviation change in a predetermined variable. For example, $p_{MX} = 0.088$ means that a 1σ change in X is followed by a 0.088σ change in M when the effects of V and S are also taken into consideration. Since $r_{MX} = 0.149 = p_{MX} + p_{MV}r_{VX} + p_{MS}r_{SX}$, the gross relationship between father's occupation and mathematics achievement may be decomposed into a direct effect (0.088) and an indirect effect (0.061), which is attributable to the association of X with the other two measured variables influencing M. The path coefficients for the disturbances are coefficients of alienation; that is, $p^2_{Ma} = 1 - R^2_{M \cdot XVS}$. It is clear that X, V, and S account for little of the within-school variance in M and W. For example, $p_{Ma} = 0.979$ means that X, V, and S account for only 4.2 per cent of the within-school variance in M.

A more important result can be obtained using the basic theorem to interpret the correlation between mathematics and reading achievement, which was not used in solving for coefficients of variables influencing M and W directly. We can write

$$\begin{aligned} r_{MW} = {} & p_{MX}(p_{WX} + p_{WV}r_{VX} + p_{WS}r_{SX}) \\ & + p_{MV}(p_{WX}r_{XV} + p_{WV} + p_{WS}r_{SV}) \\ & + p_{MS}(p_{WX}r_{XS} + p_{WV}r_{VS} + p_{WS}) \\ & + p_{Ma}r_{ab}p_{Wb} . \end{aligned} \quad (3$$

Substitution of known path and correlation coefficients in equation (3) implies $r_{ab} = 0.625$, which is barely less than the observed value of $r_{MW} = 0.644$. It may be noted that r_{ab} is just the conventional third-order partial correlation, $r_{MW \cdot XVS}$, and the deriva-

[24] Duncan, "Path Analysis: Sociological Examples," *op. cit.*, p. 5.

tion above makes clear the rationale for its calculation. To state the substantive result in a slightly different way, under the assumption that $r_{ab} = 0$, the "explanation" in Figure 2 would lead us to predict that $r_{MW} = 0.054$. Clearly, socioeconomic background alone does not explain academic achievement.

would add little, however, to the explanation of the correlation between *M* and *W* because they are highly intercorrelated with the predetermined variables already entered in the system.[25]

The introduction of intelligence is a more interesting possibility. Intelligence is not a redundant measure of origin status, and,

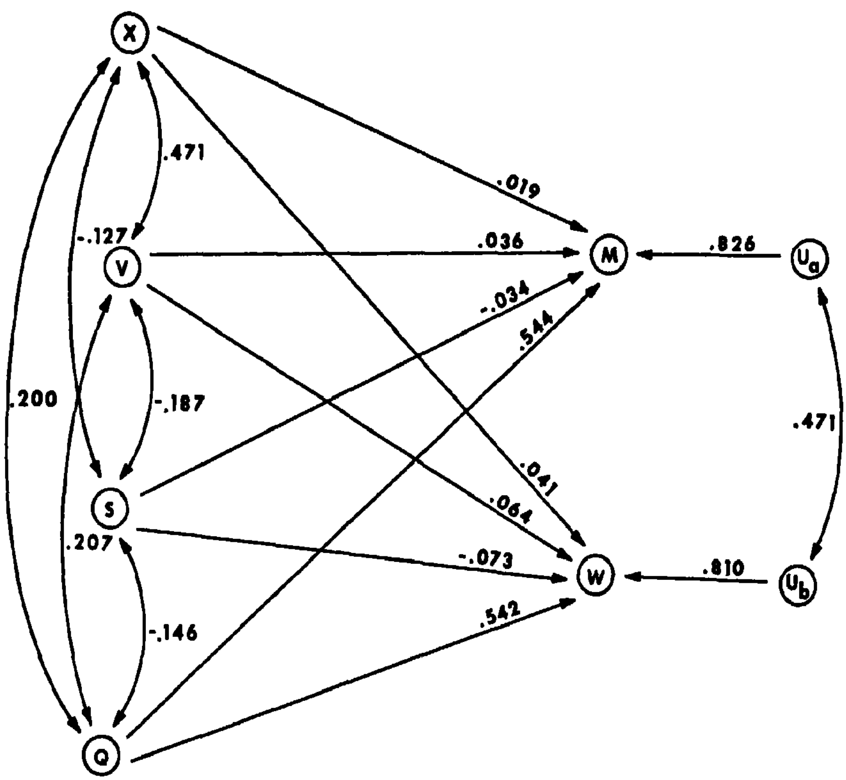


FIG. 3.—Determination of academic achievement by student background and intelligence. Item identifications: *X* = father's occupation; *V* = father's education; *S* = number of siblings; *Q* = intelligence quotient; *M* = Stanford Mathematics Grade Equivalent; *W* = Stanford Reading Grade Equivalent; U_a = unmeasured determinants of *M;* and U_b = unmeasured determinants of *W*.

What variables would add substantially to the explanation of *M* and *W* shown in Figure 2? One obvious possibility is the introduction of more measures of origin status and family structure; for example, mother's education is frequently cited as an important factor in the determination of a child's achievement. The introduction of more variables of this sort might well modify the values of the path coefficients for the predetermined variables in Figure 2. They

irrespective of its social definition, sociologists have recently become more willing to accept the idea that there is a genetic component in intelligence.[26] In Figure 3 the initial model is altered by the inclusion of intelligence among the predetermined variables. Intelligence adds substantially to the

[25] Blau and Duncan, *op. cit.*, pp. 188–94, 203.

[26] Bruce K. Eckland, "Genetics and Sociology: A Reconsideration," *American Sociological Review*, XXXII (April, 1967), 173–94.

explanation of achievement by socioeconomic background, as can be seen by comparison of the residual paths in Figures 2 and 3. The correlations of the socioeconomic background variables with academic achievement are generated largely by their joint effects with each other and with intelligence, rather than by their direct influence on achievement. For example, $r_{MX} = 0.149$ may be decomposed into a direct effect of $p_{MX} = 0.019$, and an indirect effect of $r_{MX} - p_{MX} = 0.130$. Since $p_{MQ}r_{QX} = 0.544$ (0.200) = 0.109, some four-fifths of the indirect effect of X on M is attributable to the association of X with Q. As similar results hold for the other socioeconomic background variables, we could almost accept the interpretation that their effects on academic achievement are mediated entirely by intelligence.

With the introduction of intelligence as an explanatory factor, the correlation between the two measures of academic achievement is still largely unexplained. The value of r_{MW} implied by the determination of academic achievement by X, V, S, and Q is only 0.329, and in Figure 3, $r_{ab} =$ 0.471. These results are summarized on the first two lines of Table 4.

Similar efforts have been made to account for the relationships between the two course marks, A and E, and the two aspiration measures, T and J. These results are summarized on the third and fourth lines of Table 4. For aspirations, the results are based on the average correlations for boys from the suburban ring (presented above the diagonal of Table 3). The results for course marks and for aspirations are much like those already reported. Socioeconomic background accounts for little of the relationship between the two course marks.

TABLE 4

WITHIN-SCHOOL RELATIONSHIPS AMONG EDUCATIONAL OUTCOMES IMPLIED BY ALTERNATIVE MODELS

Dependent Variables	Predetermined Variables	Implied Correlation	Observed Correlation
M and W[a]	X, V, S	0.054	0.644
M and W[a]	X, V, S, Q	0.329	0.644
A and E[b]	X, V, S	0.032	0.569
T and J[b]	X, V, S, Q	0.116	0.456
A and E[a]	M, Q, W	0.260	0.569
T and J[b]	Z, O, E	0.258	0.456
A and E[a]	X, V, S, M, Q, W	0.263	0.569
T and J[b]	X, V, S, Q, Z, O, E	0.268	0.456

Note.—Item identifications: M = Stanford Mathematics Grade Equivalent; W = Stanford Reading Grade Equivalent; A = arithmetic mark; E = English mark; T = student's educational aspiration; J = student's occupational aspiration; X = father's occupation; V = father's education; S = number of siblings; Q = intelligence; Z = father's educational expectation; and O = organizational membership.

[a] For all students in Nashville SMSA.

[b] For boys in Davidson County Ring.

Even with the addition of intelligence to the model, the background variables explain no more than one-quarter of the relationship between educational and occupational aspirations. In summary, socioeconomic background, even when supplemented by intelligence, does not account for the common content of similarly determined educational performances.

The results of parallel analyses involving more proximate causes of course marks and aspirations are reported on the fifth and sixth lines of Table 4. We assume that (*a*) each course mark is affected by mathematics and reading achievement and by intelligence, and (*b*) each aspiration is affected by perceived father's educational expectation and student's organizational membership

and English mark. Even here, we account for only about one-half of the correlations r_{AE} and r_{TJ}. Moreover, in the case of aspirations, the completeness of the explanation is probably overestimated, since there is evidence that Z, the report of father's educational expectation, is affected by the student's own educational aspiration.

The data are not inconsistent with the assumptions that M, Q, and W mediate the influence of socioeconomic background on course marks, and Z, O, and E mediate the influence of socioeconomic background and intelligence on aspiration. This can be seen by comparing line five with line seven and line six with line eight, in Table 4. When X, V, and S are added to the effort to explain r_{AE} by M, Q, and W, or when X, V, S, and Q are added to the effort to explain r_{TJ} by Z, O, and E, there are negligible increments in the correlations implied by the model.

All of the implications of these last two findings cannot be discussed here, but one observation may suggest their substantive importance. There is no evidence of discrimination by teachers in the evaluations which are measured by course marks. The association of course marks with socioeconomic background is effected by the influence of ability and achievement on marks and by association of ability and achievement with socioeconomic background. We shall consider shortly whether there is evidence of discrimination by teachers in the determination of academic achievement.

The results up to this point suggest that more sophisticated interpretations of social factors in the schooling process are prerequisite to a full analytic description of the role of schooling in the stratification process. We have had some success in developing recursive systems in which some causal paths may be dropped, but little progress has been made toward the primary goal of explaining the common content of the indicators of educational performance. While substantial improvements in the explanations offered above may have to await the discovery of new and powerful predictors which are not highly correlated with socioeconomic background, it is possible to examine some alternative interpretations without introducing more measured variables.

QUALITY OF TEACHING AND ACADEMIC ACHIEVEMENT

It was noted above that most, but not all, of the relationship between socioeconomic background and academic achievement may be interpreted as a consequence of the direct effect of intelligence on achievement and of the association of intelligence with socioeconomic background. We now consider the possibility that the remaining influence of background on academic achievement results from teacher discrimination, that is, socioeconomic differentials in the quality of teaching. The situation which actually existed in this regard is unknown, but it is possible to consider a range of possibilities which are compatible with the data. It is convenient to begin with the assumption that teaching quality, denoted by K, is the missing variable which accounts for that portion of the common content of M and W which is not explained by intelligence.

We assume that the relationships of X, V, S, and Q with M and W are identical in order to simplify the computations. We use the values $r_{QM} = r_{QW} = 0.560$; $r_{XM} = r_{XW} = 0.169$; $r_{VM} = r_{VW} = 0.186$; and $r_{SM} = r_{SW} = -.146$. It is also assumed that K, like the background variables, has identical gross relationships with M and W, that is, $r_{KM} = r_{KW}$. In the two interpretations shown in Figure 4, socioeconomic background is presumed to influence intelligence and teaching quality, and the latter variables act directly on academic achievement, completely accounting for $r_{MW} = 0.64$ with $r_{ab} = 0$. The conceptual distinctions among Q, K, and M and W are now rather fine; Q is what the student *can learn*, K is what the student *is taught*, M and W are what the student *has learned*.

In the top model in Figure 4 it is assumed that there is no causal relationship and no covariation between Q and K. Because intelligence is ignored by teachers, the model can be described as one of equal access to

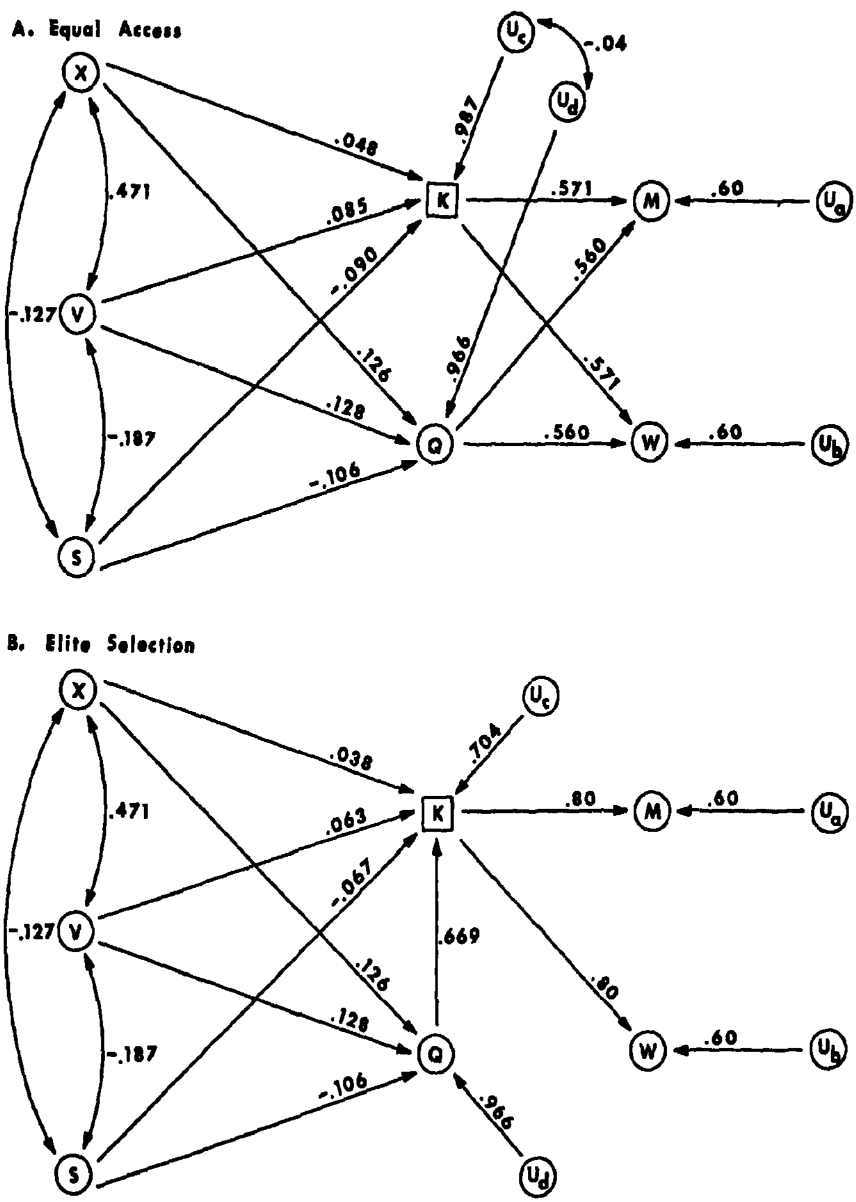


FIG. 4.—Two hypothetical models of the achievement process within-schools. Item identifications: X = father's occupation; V = father's education; S = number of siblings; K = effects of teaching, M = Stanford Mathematics Grade Equivalent; W = Stanford Reading Grade Equivalent; Q = intelligence; U_a = unmeasured determinants of M; U_b = unmeasured determinants of W; U_c = unmeasured determinants of K; and U_d = unmeasured determinants of Q.

educational facilities. The path coefficients and correlations involving M may be obtained from the equation

$$r_{MW} = p_{MK}r_{KW} + p_{MQ}r_{QW} . \quad (4)$$

Because it has been assumed that $r_{KW} = r_{KM}$ and $r_{QW} = r_{QM}$, equation (4) can be rewritten as

$$r_{MW} = p_{MK}r_{KM} + p_{MQ}r_{QM} . \quad (5)$$

Because $r_{KQ} = r_{Ka} = r_{Qa} = 0$, it can be shown that $r_{KM} = p_{MK}$ and $r_{QM} = p_{MQ}$. Thus, equation (5) can be rewritten as

$$r_{MW} = r_{KM}^2 + r_{QM}^2 , \quad \text{or}$$
$$r_{KM}^2 = 0.64 - (0.56)^2 = 0.326 . \quad (6)$$

Taking the positive root, we find $r_{KM} = p_{MK} = 0.571$. Because the right side of equation (5) is also an expression for the proportion of variance in M explained by K and Q, the residual path can be found as $p_{Ma} = \sqrt{1 - r_{MW}} = 0.60$. The solutions for the path coefficients and correlations involving W are analogous.

Given the assumed values for the correlations of X, V, and S with the indicators of academic achievement and the assumption that the background factors are uncorrelated with U_a and U_b, values for the correlations of X, V, and S with M and W are implied by the equations

$$r_{MX} = r_{WX} = p_{MK}r_{KX} + p_{MQ}r_{QX} , \quad (7)$$

$$r_{MV} = r_{WV} = p_{MK}r_{KV} + p_{MQ}r_{QV} , \text{ and} \quad (8)$$

$$r_{MS} = r_{WS} = p_{MK}r_{KS} + p_{MQ}r_{QS} . \quad (9)$$

By substitution of known path coefficients and correlations, we find $r_{KX} = 0.099$, $r_{KV} = 0.124$, and $r_{KS} = -0.112$; the values of the path coefficients for the socioeconomic background variables can be found in the usual fashion.

Under these assumptions the implied correlations between socioeconomic background variables and teaching quality are two-thirds to three-fourths as large as the original correlations between socioeconomic background and academic achievement. While background does have an effect on teaching quality, that effect need not be large to account for the relation between background and academic achievement; nearly all of the variation in teaching quality must be explained by variables unrelated to socioeconomic background.

The solution just obtained depends on the assumption that teaching quality is unrelated to the student's intelligence, but we do not know whether this was the case. We can test the conclusion by adopting an extreme alternative assumption: Suppose measured intelligence as well as the background factors affects teaching quality, while the latter alone accounts for the common content of the achievement measures. We again allow the background variables to *influence directly both K and Q, but not M or W*. This second set of assumptions seems more congruent than the first with the interpretation offered by critics of intelligence testing. For example, Sexton argues, "One very destructive function of the IQ score is that it serves as a kind of cement which fixes students into the social classes of their birth. . . . Typically, the lower-income child comes to school and sooner or later he learns that he cannot compete with upper-income students. . . . For these reasons he does poorly on IQ tests. The teacher learns that he has a low IQ rating and puts him into a slow-moving group where he is not expected to do much or given much attention. . . . Intellectually he is lost."[27]

The elite selection model is shown in the lower panel of Figure 4. The application of the algorithm just described yields the values, $r_{KM} = p_{MK} = r_{KW} = p_{WK} = 0.80$; $p_{Ma} = p_{Wb} = 0.60$; $r_{KQ} = 0.70$; $r_{KX} = 0.211$; $r_{KV} = 0.233$; and $r_{KS} = -0.182$. The implied correlations between socioeconomic background variables and teaching quality are now 25 per cent larger than the original correlations between background and achievement variables. This greater gross "discrimination" by socioeconomic back-

[27] Sexton, *op. cit.*, pp. 51–52.

ground follows from the assumption that more intelligent students receive better teaching. Larger correlations between socioeconomic background and teaching quality could be generated only by assuming that intelligence has a negative direct effect on academic achievement. In this sense, the elite-selection model provides a maximum reasonable estimate of the influence of socioeconomic background on teaching quality. The elite selection model is formally consistent with the assumption that intelligence testing is a mechanism for discrimination against the poor, but the results suggest that the mechanism is not very effective. While nearly half of the variance in teaching quality is "explained" in the elite-selection model, most of the explanation is due to student intelligence, and the direct effects of the student's socioeconomic background on teaching quality are smaller in the elite model than in the equal-access model. In neither interpretation does socioeconomic background account for much of the variation in intelligence. Indeed, the latter variable introduces a large component of variation in academic achievement which is independent of social origins.

The value of these conclusions depends, of course, on the validity of prior assumptions. Teaching quality was not measured directly; it is simply a plausible label for the hypothetical variable K under stated conditions. In particular, it is worthwhile to reconsider the assumption of both models that K accounts for that portion of the common content of M and W which is not explained by intelligence. It is equally possible that teaching quality has little of the influence imputed to it here. In that case, socioeconomic background must have a large effect on teaching quality if it is to mediate the effects of background on achievement. As long as intelligence and teaching quality are assumed to account for all of the influence of socioeconomic background on academic achievement, the data are consistent with either the assumption that teaching quality has a powerful influence on academic achievement or that socioeconomic background has a powerful influence on teaching quality.

ESTIMATION OF NET SCHOOL EFFECTS

An interpretation of the determination of educational aspiration (T) by the background variables (X, V, S, Q) and a set of intermediate variables (Z, O, E) is shown in Figure 5. The numerical results are based on average within-school correlations for county boys shown above the diagonal of Table 3. The model reproduces the correlations of X, V, S, and Q with T to within 0.045 of their observed values without assuming that any of them influence T directly. As noted earlier, the disproportionate influence of Z on T should not be taken too seriously; further evidence on that point may be discovered in the path diagram.

Relationships between socioeconomic background and educational outcomes within schools carry implications for the interpretation of variation in educational outcomes between schools which have been misinterpreted in the literature about the effects of school quality on achievement.[28] The relationships between students' characteristics and educational outcomes are much the same in any school, and student-body composition differs on factors influencing educational outcomes within schools. Thus, it is reasonable to ask how much of the gross variation in performance levels among schools is explained by the differing composition of student bodies.

A model for the interpretation of school differences in educational outcomes is shown in Figure 6. It is based on the additive decomposition,

$$m_j = c_j + r_j\,, \qquad (10)$$

where the lower-case letters represent variables in raw form. The variable m_j is the mean for school j on an educational outcome measure. The value of r_j, is chosen to make equation (10) hold for each school. The covariance adjustment, c_j, is the effect on

[28] For example, Thomas F. Pettigrew, "Race and Equal Educational Opportunity," *Harvard Educational Review*, XXXVIII (Winter, 1968), 66–76.

the school mean of composition on one or prior variables from the within-school model. If the subscripts i, j, and k are taken to stand for students, schools, and predetermined variables, respectively, and b_{mk} is the within-school regression coefficient in raw form of m on x_k, then

$$c_j = \sum_k b_{mk}(\bar{x}_{\cdot jk} - \bar{x}_{\cdot\cdot k}) \,. \qquad (11)$$

That is, c_j is the amount by which the school mean on the outcome variable would change if the school means on each of the predetermined variables were shifted to the grand mean for all schools and students. If each variable in equation (10) is expressed in standard form, we obtain

$$\frac{m - \bar{m}}{\sigma_m} = \frac{c - \bar{c}}{\sigma_c}\frac{\sigma_c}{\sigma_m} + \frac{r - \bar{r}}{\sigma_r}\frac{\sigma_r}{\sigma_m}\,, \qquad (12)$$

or

$$M = p_{MC}C + p_{MR}R\,, \qquad (13)$$

where M, C, and R are the variables in standard form and p_{MC} and p_{MR} are the

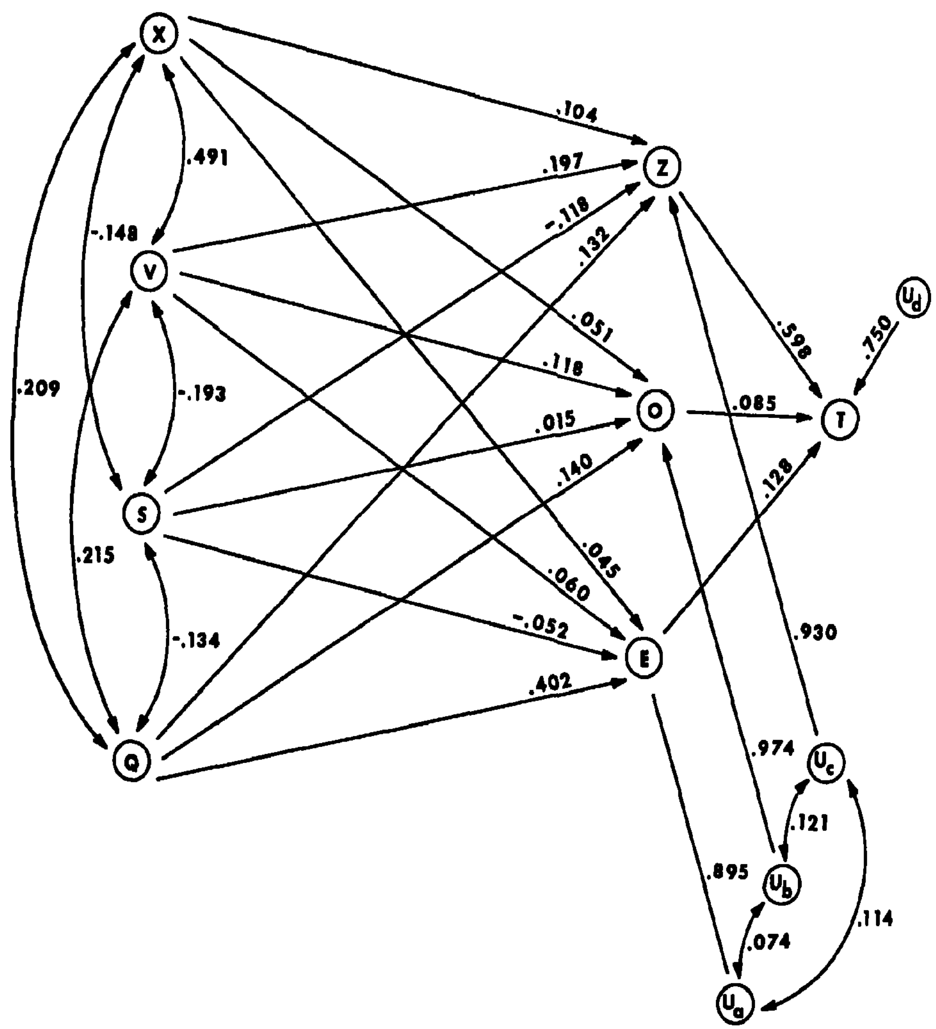


FIG. 5.—Within-school multiple correlation analysis of educational aspiration based on average correlations for boys in Davidson County. Item identifications: X = father's occupation; V = father's education; S = number of siblings; Q = intelligence; Z = father's educational expectation; O = organizational membership; E = English mark; T = student's educational aspiration; U_a = unmeasured determinants of E; U_b = unmeasured determinants of O; U_c = unmeasured determinants of Z; and U_d = unmeasured determinants of T.

path coefficients for the determination of M by C and R.[29] In the case of composition on a single variable, the path coefficient p_{MC} and the between-school or "ecological" correlation r_{MC} are easily calculated. The remaining path coefficient, p_{MR}, and the correlation coefficients r_{MR} and r_{CR} can be determined by simultaneous solution of the normal equations

$$r_{CM} = p_{MC} + r_{CR}p_{MR}\,, \quad (14)$$

and

$$r_{RM} = r_{CR}p_{MC} + p_{MR}\,, \quad (15)$$

and the equation expressing the complete determination of M by C and R,

$$1 = p_{MC}^2 + p_{MR}^2 + 2p_{MC}r_{CR}p_{MR}\,. \quad (16)$$

Equation (16) is a proportionate allocation of the variance lying between schools to terms representing composition, residual factors, and the joint effects of composition and residual factors, respectively.

The complete determination of a dependent variable by a combination of measured and unmeasured variables is a feature common to all path models. Usually, the assumption that the measured and unmeasured determinants of the dependent variable are uncorrelated is the condition which permits identification of the path coefficients. Here, the use of constants (b_{mk}) from the within-school model in the adjustment for composition is a substitute for the assumption that the measured and unmeasured determinants of M are uncorrelated, and a value of r_{CR} is obtained in the course of the solution.

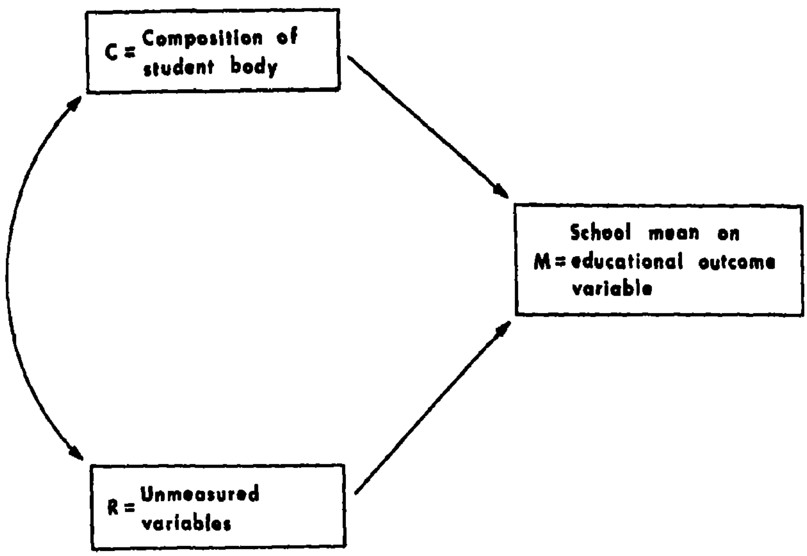


FIG. 6.—Between-school model for the determination of an educational outcome by composition on student characteristics and residual factors.

While the topic of group and individual level effects is not ordinarily approached from this point of view, it is the supposed influence of C on M via the correlation r_{CR} which is frequently described as a "structural" or "contextual" effect of school composition. Here, the correlation r_{CR} is simply taken as a problematic aspect of the data. The identification of R with specific causal factors is equivocal because R includes all causes of school differences in academic achievement which are not included in C. Thus, no assumption is made about the causal priority of C and R. There is no further decomposition of equation (16) which can provide a clearer picture of the relative importance of school composition and other

[29] See Duncan, "Path Analysis: Sociological Examples," *op. cit.*, p. 8, for a similar example.

factors in the determination of school performance levels.

Table 5 displays five decompositions suggested by the model in Figure 5, of the form just described. The decompositions are not based on the average correlations for county boys reported in Table 3; rather, they are based on weighted multiple correlation analyses of compositional and residual components obtained from separate covariance analyses at each grade level. It should be emphasized that the figures in Table 5 are decompositions of between-school variance, not of the total individual variance in the educational outcomes. The latter decompositions may be approximated from Table 5 by multiplying the entries by the appropriate proportion of variance lying between schools taken from Figure 1. For example, the proportion of the total variance in T explained by student body composition on X, V, S, and Q is (0.335) (0.114) = 0.038.

About one-third of the between-school variance in educational aspiration is explained by each of the components in the decomposition on the background variables and intelligence. Composition is more important in the decomposition on the intermediate variables, where it accounts directly for more than half the variance; the remainder of the variance is split equally between the residual and joint components. That is, the causal assumptions of the right side of Figure 5 directly explain more than half of the variation in educational aspirations among schools; other aspects of school quality may account for no more than 6 per cent of the total variance in educational aspirations.

It is no surprise that the decomposition of Z on the background factors is nearly identical to that of T. The decompositions of O and of E are more interesting. In the case of organizational membership, the result approximates the outcome of an experimental design employing randomization. While the effect of composition is small, the measured and unmeasured determinants of levels of organizational membership are virtually unrelated. There is negative overlap between the compositional and residual determinants of English mark; marking standards are more stringent in high-status than in low-status schools.[30]

In summary, covariance adjustments account for significant portions of the variation in educational outcomes among schools, even where the models on which the adjustments are based are substantively deficient. Interpretation of residual differences among schools is complicated by the association of measured and unmeasured determinants of school performance levels.

TABLE 5

DECOMPOSITION OF BETWEEN-SCHOOL VARIANCE IN EDUCATIONAL OUTCOMES FOR BOYS IN DAVIDSON COUNTY RING

PROBLEM	COMPOSITION	RESIDUAL	JOINT	TOTAL
	p^2_{MC}	p^2_{MR}	$2p_{MC}p_{MR}r_{CR}$	
T on X, V, S, Q...	0.335	0.377	0.288	1.000
T on Z, O, E.....	0.533	0.242	0.225	1.000
Z on X, V, S, Q..	0.321	0.383	0.296	1.000
O on X, V, S, Q...	0 084	0.922	−0.006	1.000
E on X, V, S, Q...	0.290	0.857	−0.147	1.000

NOTE.—Item identifications: T = student's educational aspiration; Z = father's educational expectation; O = organizational membership; E = English mark; X = father's occupation; V = father's education; S = number of siblings; and Q = intelligence.

[30] Alan B. Wilson, "Social Stratification and Academic Achievement," in A. Harry Passow, (ed.) *Education in Depressed Areas*, (New York: Columbia University Press, 1963), 217-36.

NEIGHBORHOOD COMPOSITION AND SCHOOL QUALITY

A few additional remarks may shed some light on the debate over the effects of "neighborhood context" on educational aspiration.[31] Investigations of neighborhood affects typically assign a single value of an indicator of neighborhood population composition to each school. Clearly, such indicators are uncorrelated with individual deviations from school means, and they can affect educational aspiration only through its between-school component. Thus, the influence of neighborhood factors at the aggregate level is limited by the proportions of variance lying between schools which are reported in Figure 1. However, gross school differences are not an upper limit on the influence of processes which occur in the neighborhood. These may be represented explicitly in within-school models in the same fashion as family influences, provided the requisite measurements have been obtained.[32]

With these cautions in mind, we consider two interpretations shown in Figure 7, of the influence of neighborhood status on the between-school component of educational aspiration. The right-hand portions of the path diagrams depict the decompositions on the first two lines of Table 5. That is, *CB* stands for composition on student background characteristics and *RB* for residual determinants of *T* in the first decomposition; *CI* stands for composition on the intermediate variables and *RI* for residual determinants of *T* in the second decomposition. The two neighborhood factors on the left-hand side of the diagram are *HSG*, the proportion of high school graduates in the adult population, and *FAM*, the proportion of households with husband-wife families including children of school age. Both variables are interpreted as indicators of neighborhood composition which may influence the composition of student bodies or other aspects of school quality.

The neighborhood factors account for the relationship between student-body composition and school quality; that is, the correlations between the unmeasured variables, U_a and U_b, are negligible in both path diagrams. While the effects of the neighborhood factors are sufficient to account for the relationship between student-body composition and the unmeasured aspects of school quality, the substantial coefficients for the residual determinants of school quality (0.890 and 0.924) suggest that the common identification of "good" and "rich" schools may be inaccurate. The most interesting aspect of the diagrams is the interpretation of the correlations of the neighborhood factors with educational aspiration, $r_{T,HSG} = 0.775$ and $r_{T,FAM} = 0.387$. These are all similar to

$$r_{T,HSG} = p_{T\,CB} r_{CB\,HSG} + p_{T\,RB} r_{RB\,HSG}\,, \tag{17}$$

and in each case one-half or more of the gross influence of the neighborhood factor is attributable to its effect on student-body composition. The conclusion that neighborhood factors do contribute modestly to the explanation of educational aspiration beyond their influence on student-body composition should be tempered by several cautions. (*a*) The net effects of the neighborhood variables on school quality may involve composition on variables excluded from the within-school model. (*b*) The within-school models on which the decompositions are based are known to be unsatisfactory. (*c*) There is little variation in educational aspiration among schools.

PERSISTENCE OF SCHOOL QUALITY

We can now return briefly to the issue of intercohort persistence in school character-

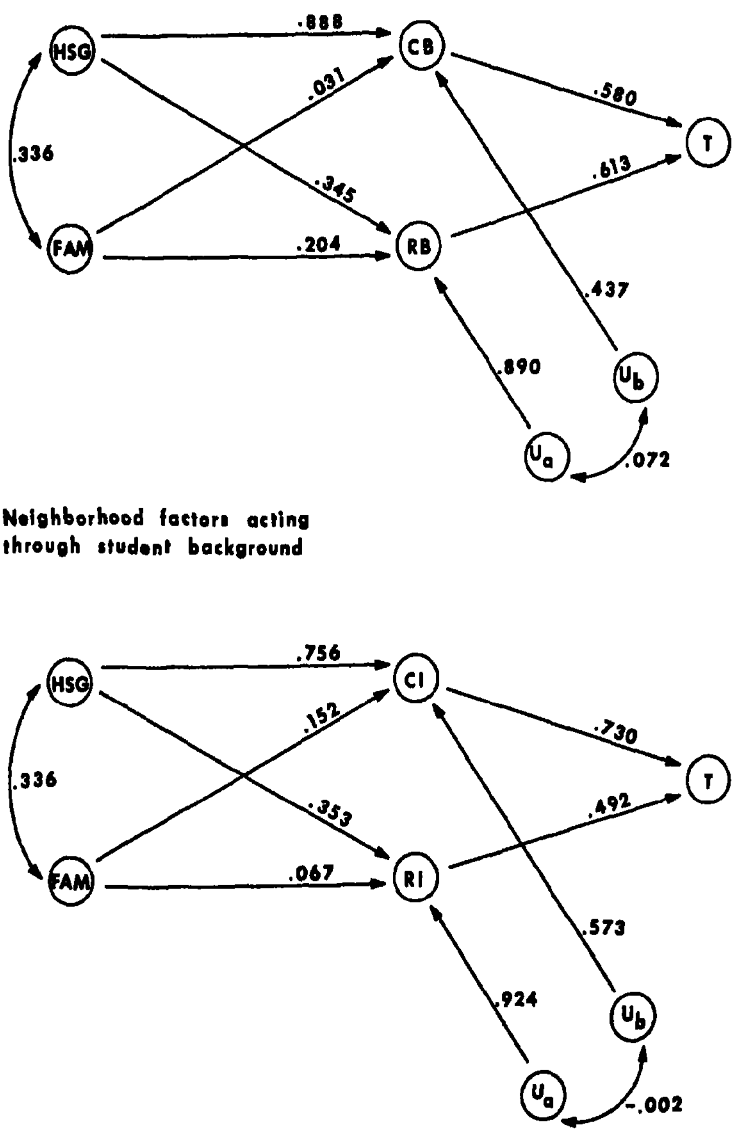
[31] Sewell and Armer, "Neighborhood Context and College Plans," *op. cit.*; Ralth H. Turner, "On Neighborhood Context and College Plans (I)" *op. cit.*; John A. Michael, "On Neighborhood Context and College Plans (II)" *op. cit.*; Richard P. Boyle, "On Neighborhood Context and College Plans (III)," *op. cit.*; and Sewell and Armer, "Reply to Turner, Michael, and Boyle," *op. cit.*

[32] Otis Dudley Duncan, Archibald O. Haller, and Alejandro Portes, "Peer Influences on Aspirations: A Reinterpretation," *American Journal of Sociology*, LXXIV (September, 1968), 119–37.

istics. Table 6 displays the averages of the intercorrelations for adjacent grade-cohorts of the composition and residual terms from the decompositions in Table 5. In every case there is greater persistence in the composition of student bodies than in the residual determinants of the educational outcomes. While organizational change in schools may be slow, it is more rapid than change in student body composition. Again, this differential rests on the minimal adequacy of the within-school models on which the decompositions are based.

SCHOOL QUALITY AND EDUCATIONAL ATTAINMENT

The determination of ultimate educational attainment, denoted by Y, is interpreted



A. Neighborhood factors acting through student background

B. Neighborhood factors acting through intermediate variables

FIG. 7.—Neighborhood factors in educational aspiration. See text for explanation of variables

within the analysis of covariance framework by the path diagram in Figure 8. The lowercase subscripts, b and w, indicate between-school and within-school components of variation, respectively. Unsubscripted variables are the sum of their within-school and between-school components. The variation of V, T, and Q is separated into within- and between-school components, and the within- and between-school models for the determination of aspiration account for the relationships among V, T, and Q. The crucial feature of the model is the absence of relationships between any of the between-school components and any of the within-school components. The within-school portion of the model shows the determination of the within-school components of educational aspiration by socioeconomic status, ability, and uncorrelated residual factors. The paths from V_b and Q_b to T_b in the between-school segment of the model show the direct effects of composition on ability and socioeconomic status on the between-school component of educational aspiration. These effects are strictly implied by the pattern of determination of educational aspiration in the within-school segment of the model and the relative magnitude of within and between-school variation in each of the variables. The correlated error term, R_b, represents school quality and accounts for the remainder of the between-school variation in aspiration.

In the total segment of the model the process is extended to the determination of ultimate educational attainment. Paths are drawn from V to Y and from Q to Y to indicate that the influence of socioeconomic status and ability on ultimate educational attainment is not exhausted by their effects on aspiration. The major point to be made here is that the relationship between school quality (R_b) and ultimate educational attainment is by no means direct. It depends on the relationship of R_b with T_b, V_b, and Q_b; on the relationships of T_b, V_b, and Q_b with their respective total components; and on the relationships of the total components A, V, and Q with ultimate educational attainment.

The figures on the diagram are only intended to be suggestive. They were obtained by a crude collation of results from the Nashville study and the Sewell and Shah 1957 follow-up study of Wisconsin high school seniors.[33] A detailed interpretation of the figures is probably not worthwhile. It may be sufficient to point out that no coefficient in the diagram substantially exceeds that on the path running from R_w to T_w.

CONCLUSIONS

Schooling introduces a large component of variation in adult social and economic

TABLE 6

AVERAGE INTERANNUAL INTERCOHORT PERSISTENCE OF COMPOSITIONAL AND RESIDUAL DETERMINANTS OF EDUCATIONAL OUTCOMES[a]

Problem	Composition	Residual
T on X, V, S, Q	0 902	0 631
T on Z, O, E..........	0 834	0 653
Z on X, V, S, Q.......	0 929	0 579
O on X, V, S, Q..	0 908	0 592
E on X, V, S, Q.......	0 845	0 767

Note.—Item identifications: T = student's educational aspiration; Z = father's educational expectation; O = organizational membership; E = English mark; X = father's occupation; V = father's education; S = number of siblings; and Q = intelligence.

[a] See text for explanation.

achievement which is random with respect to social origins. At the same time, studies of the educational process report that success in school is highly contingent on social origins. The apparent conflict of these two interpretations is in part an artifact of their verbal presentation. That educational success is an important mechanism for the intergenerational transmission of status is not inconsistent with its other role—to facilitate mobility.

A closer examination of the concepts and methods used in school studies reveals other reasons for their tendency to emphasize the conservative features of the educational process. As in other areas of sociological in-

[33] Sewell and Shah, *op. cit.*

quiry, few rules govern the relationship between analysis and interpretation. Consequently, effects of socioeconomic origins on educational performance are frequently overinterpreted. The equally important problems of the relationships among indicators of educational performance and their effects on social and economic achievement in adulthood often are neglected. Finally, efforts to identify and interpret effects of school quality in cross-sectional data are disproportionate to the importance of school quality in the stratification process.

These observations have been illustrated by exploring the determinants of three outcomes of secondary education—academic achievement, course marks, and aspirations—in six grade-cohorts of white public school

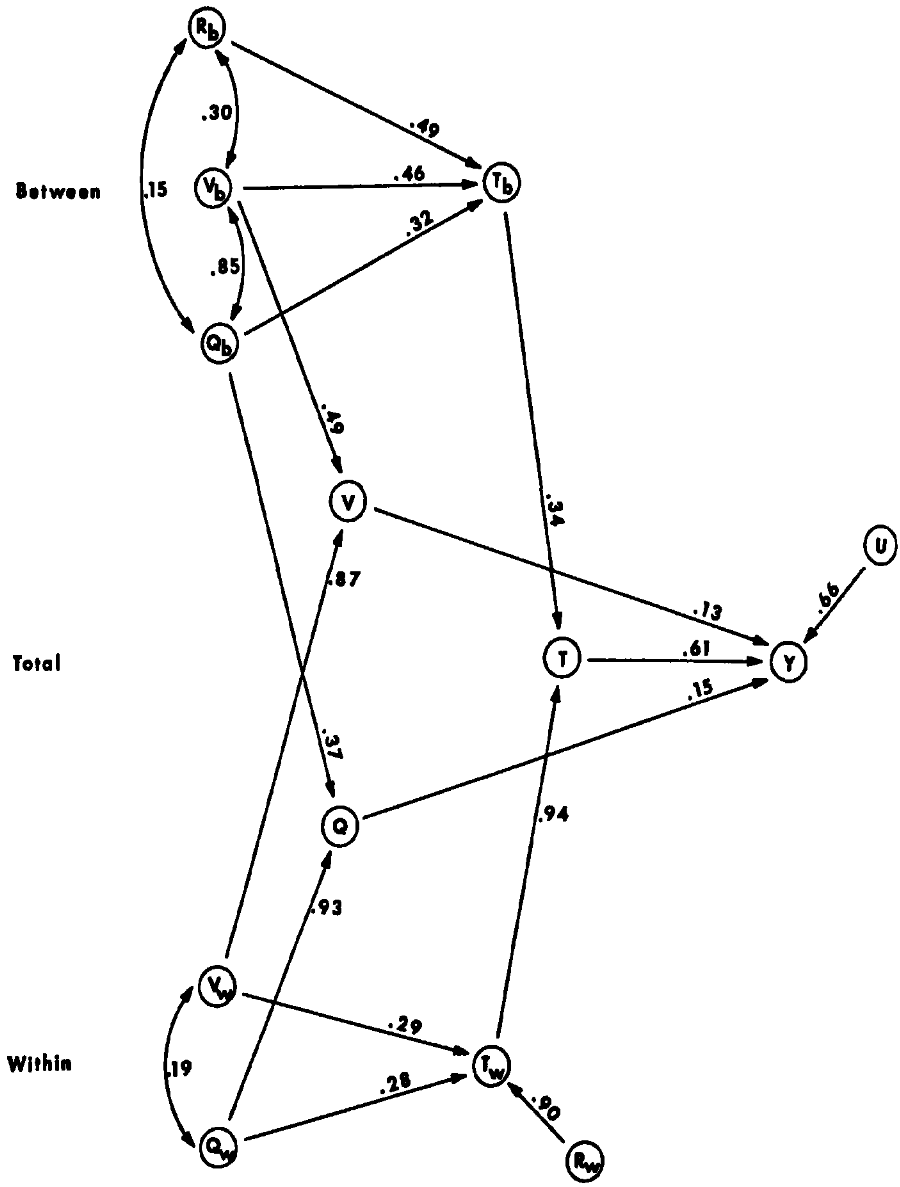


FIG. 8.—Illustrative model of school factors in educational attainment. See text for definition of variables

students in the Nashville metropolitan area. It is necessary to develop explanations of the determination of educational outcomes within schools before differences between schools can be interpreted. Two guidelines were used in the present effort to develop such explanations: (1) an effort to identify intervening variables through which socioeconomic background factors influence educational outcomes, and (2) a requirement that explanations account for the similarity in performance in pairs of educational outcomes with common causes. There was greater success in regard to the first of these criteria than the second.

Socioeconomic background affects academic achievement primarily through its association with intelligence. Socioeconomic background influences course marks only through its effects on intelligence and academic achievement. There is little evidence of discrimination by socioeconomic background in the teaching or marking of students. Socioeconomic background and intelligence influence aspiration through their effects on course marks, membership in school organizations, and perceived parental expectations.

Socioeconomic background and intelligence do not account for the similarity of individual achievement in reading and mathematics. Socioeconomic background, intelligence, and academic achievement do not account for the similarity of course marks in English and arithmetic. Socioeconomic background, intelligence, academic performance, membership in school organizations, and perceived parental expectations do not account for the similarity of educational and occupational aspirations. Thus, none of the within-school explanations of educational outcomes is entirely satisfactory.

The differences in educational outcomes among schools are small in comparison with the magnitude of individual variability in those outcomes. Only about one-fifth of the total variance in academic achievement, one-ninth of the variance in aspirations, and one-sixteenth of the variance in course marks are attributed to the gross effect of the schools attended. Moreover, there is little evidence of interaction of school attended with the variables that influence educational outcomes within a school. Thus, it is meaningful to adjust school performance levels for the effects of differences in student-body composition, and such adjustments account for moderate proportions of the variation in school performance levels, despite the deficiencies of the within-school models on which they are based. School performance levels do have a strong tendency to persist over time, but this may be a result of the persistence of characteristics of students that influence performance.

Relationships of aggregate measures of neighborhood status with level of school performance do not have a clear interpretation in terms of socialization processes outside the school. They may be interpreted as indicators of ecological processes influencing student-body composition and of demand factors in the market for educational quality. Most of the influence of neighborhood status on educational aspiration may be interpreted as an effect on student-body composition. Neighborhood status has only a modest influence on school quality.

Quality of high school plays a minor role in determining ultimate educational attainment. Indeed, it appears likely that our estimation of the substantive importance of high school quality will vary inversely with our understanding of the details of the schooling process.

Brown University

Socioeconomic Differentials among Religious Groups in the United States

Sidney Goldstein

ABSTRACT

The availability of hitherto withheld tabulations from the 1957 Current Population Survey, in which a question on religion was asked, provides a unique opportunity to examine a nationwide sample of the U.S. population for the relation between religion and socioeconomic status, as measured by education, occupation, and income.

Over-all, without controls, the data show significant differences among the religious groups with respect to socioeconomic status. Judged by median income, median education, and the proportion of white-collar workers, Jews occupied considerably higher status position than did either Protestants or Catholics.

Controlling for place of residence by restricting the analysis to the urban population and controlling for education as well in examining the occupation data, and for occupation and education separately in controlling for the income data, suggest that, when these controls are introduced, there is a considerable narrowing of the differentials in socioeconomic status among the three religious groups. In fact, for several subcategories of each of the groups, the income differentials virtually disappear, as do some of the striking differentials in occupational composition. These controls suggest that many of the differences among the three religious groups to which the crude data point are in fact a function of the differential educational achievement and/or occupational composition of the members of these groups, as well as a result of their differential concentration in urban and rural places. With these factors controlled, although admittedly in a somewhat crude fashion, the conclusion seems warranted that both education and occupation play a much more crucial role than does religion itself in influencing the income levels of members of the three religious groups.

On November 16, 1966, the director of the U.S. Bureau of the Census announced that the 1970 census will not include a question on religion. In his words, "The Bureau has been considering a number of requests from individuals and organizations which proposed that a question on religion be added to the nationwide census which is to be taken beginning in April, 1970. The decision not to add this question is based on the fact that a substantial number of persons again expressed an extremely strong belief that asking such a question in the decennial population census, in which replies are mandatory, would infringe upon the traditional separation of church and state."[1]

Thus, in 1970, as in previous decennial censuses, information on religion will not be included among the data collected on the social, economic, and demographic characteristics of the American population. For at least another ten years, therefore, religion will retain its distinctiveness as being "the most significant social characteristic that is not now included in the decennial census."[2] Despite the fact that, "like the factors of educational attainment, occupation, and income, it [religion] is the axis around which much of a person's life is oriented,"[3] social scientists will be unable to obtain informa-

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[3] Donald J. Bogue, *The Population of the United States* (New York: Free Press, 1959), p. 688.

tion on religious identification from the 1970 decennial census that would enhance their analyses of the demographic and social characteristics and behavior of the American population. This will be in sharp contrast to the situation in many other countries. In the 1964 United Nations *Demographic Yearbook*, out of a total of 214 independent nations and dependencies listed, as many as eighty-five present statistics on religious affiliation as part of their regular official census tabulations.[4] The United States does not.

Serious consideration was also given to this issue in the 1960 census.[5] In fact, in March, 1957, the Bureau of the Census included the question, "What is your religion?" in its monthly Current Population Survey. Unlike the decennial census, this survey, which encompassed approximately 35,000 households in 1957, is voluntary. The purpose of including a question on religion was twofold: (1) to ascertain the public reaction to such a question, and (2) to evaluate the quality of the answers obtained to the specific wording of the question. But even before the first results of the survey were made available to the public in February, 1958, the director of the census announced, in December, 1957, that the 1960 census of population would not include any inquiry on religion. The reason given was the same one cited in 1966, namely, that "a considerable number of persons would be reluctant to answer such a question in the census where a reply is mandatory."[6] This decision was reached despite the fact that the 1957 voluntary Current Population Survey indicated that only 1 per cent of all persons 14 years old and over had made no report on religion, thereby suggesting that the American people were quite willing to reply to such a question, at least on a voluntary basis.

[4] United Nations, *Demographic Yearbook, 1963* (New York: United Nations, 1964), Table 11; *Demographic Yearbook, 1964*, Table 32.

[5] Taeuber, *op. cit.*, pp. 4–5.

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In the absence of any question on religion in 1960 and in the forthcoming 1970 census, the data from the 1957 Current Population Survey still provide one of the best bases for determining the religious composition of the American population and the social and economic characteristics of individuals in the various religious groups. Until recently then, the only source of statistics from the 1957 survey was the Current Population Report of February 2, 1958, "Religion Reported by the Civilian Population of the United States: March 1957."[7] Other nationwide statistics on religious composition are, of course, available from various surveys undertaken by public opinion polls and other organizations. Extensive use of such data was made by Donald Bogue in *The Population of the United States*.[8] In addition, insights into the characteristics of particular religious groups and the differences between the members of these groups and those of the total population are available from a number of community population surveys, many of which are sponsored by specific religious bodies. These do not, however, give a national picture. Finally, data have in the past also been available from the Census of Religious Bodies, which was taken periodically as a separate census between 1906 and 1936 by means of a questionnaire mailed to the pastors and clerks of the parishes or congregations. It was limited to an enumeration of the membership of the various religious groups and did not, therefore, provide any information on their social and economic characteristics.

The data published in the February, 1958, Current Population Report were restricted to an enumeration of persons 14 years old and over by religion, cross-tabulated by color and sex, by region of residence, by urban-rural residence, and by age and sex. Information was also presented on (1) related children under 14 years old,

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[8] *Op. cit.*

cross-tabulated by religion reported for the family head and his wife, thereby providing insights into the religious composition of the total population; and (2) married couples by religion reported, thereby permitting determination of the number of persons married to individuals belonging to the same or different religious groups at the time of the census, a crude measure of intermarriage since it overlooks marriages in which one spouse converted to the religion of the other.

When the February, 1958, Report was released, it was generally assumed that other reports would follow. Because of various pressures on the Bureau of the Census, this did not happen. As a result, a wealth of data on the social and economic characteristics of Protestants, Catholics, and Jews, and of various denominations within each of these three major religious groups have not been available from this national survey. In 1967, the Freedom of Information Act was passed by Congress.[9] In accordance with the provisions of the Act, the Bureau of the Census made these unpublished tabulations available upon request. Unfortunately, the original survey records and tabulation materials have been destroyed, but the unpublished tabulations are now available for analysis. Although they do not provide all of the possible cross-tabulations which might be desired, they do provide a new source of data on the social and economic characteristics of religious groups in the United States. Referring to 1957, they are obviously already outdated. Yet, in the absence of more recent data from the 1960 or the forthcoming 1970 census, they represent a major source of information on the demographic characteristics of religious groups in the United States and provide important base data against which future changes in composition can be measured. Since the published statistics are readily available and have already been analyzed,[10] they will be used here only incidentally. This analysis will focus on differentials by religion with respect to education, labor force participation, occupation, and income.

[9] Public Law 89-489.

THE DATA

The data are based on information collected in the monthly population sample survey conducted by the Bureau of the Census in March, 1957.[11] At that time, the sample was spread over 330 sample areas, comprising 638 counties and independent cities. A total of about 40,000 dwelling units and other living quarters are designated for the sample at any time, and completed interviews are obtained each month from about 35,000 households. Of the remainder, about 1,500 are households from which information should be obtained but is not, and the rest are vacant dwellings or other dwellings, such as households with usual residence elsewhere, that are not available for enumeration by the survey. The final estimates, based on the survey, involve the adjustment of weighted sample results to independent estimates of the civilian non-institutional population of the United States. These estimates were made separately for the various age, color, and sex classes.

Since the estimates are based on sample data, they are subject to sampling variability.[12] The sampling variability may be relatively large in the case of the smaller figures and in the case of small differences between figures. In addition to sampling variation, the estimates are subject to bias due to error in response and to non-reporting. Thus, where the household members differ in their

[10] See, e.g., Bogue, *op. cit.*, pp. 688–709; Paul C. Glick, "Intermarriage and Fertility Patterns among Persons in Major Religious Groups," *Eugenics Quarterly*, VII (March, 1960), 31–38; Erich Rosenthal, "Jewish Fertility in the United States," *Eugenics Quarterly*, VIII (December, 1961), 198–217.

[11] U.S. Bureau of the Census, "Tabulations of Data on the Social and Economic Characteristics of Major Religious Groups, March, 1957" (unpublished). For a fuller, published discussion of how the Current Population Survey of 1957 was conducted and the definitions used, see U.S. Bureau of the Census, "Religion Reported by the Civilian Population of the United States: March 1957," *op. cit.*, pp. 1-4.

[12] U.S. Bureau of the Census, "Religion Reported by the Civilian Population of the United States: March 1957," *op. cit.*, p. 4.

religion but only one member gives the information, there may have been a tendency to report the same religion for all of the household members. Also, a few persons may have misreported their religion.

The figures relate primarily to the civilian population. Approximately 809,000 members of the armed forces living off-post or with their families on-post are included, but all other members of the armed forces are excluded. For convenience, the population covered is referred to by the Bureau of the Census as the civilian population.

The data on religion are based on voluntary answers to the question, "What is his religion—Baptist, Lutheran, etc.?" The answer given for each person indicated what he regards as his religion and not whether the person is an active member of a church or synagogue. The question was asked only of persons 14 years old and over. These persons were classified as "Protestant," "Roman Catholic," "Jewish," "Other Religion," "No Religion," and "Religion Not Reported." Persons reported as Protestant are further classified into one of the four largest Protestant denominational groups—Baptist, Lutheran, Methodist, or Presbyterian—or as "Other Protestants," but data on these denominational groups are not included in the recently released tabulations. The group "Other Religion," consists largely of persons reporting such organizations as the Eastern Orthodox Church, the Polish Catholics, and the Old Catholics. It also includes the relatively few persons who are Buddhists, Moslems, and other non-Christians. The group "No Religion" includes persons who reported that they are atheists, agnostics, or said that they had no religion.

Both the published data and the newly released statistics based on the 1957 survey dichotomize the Protestant group into white and non-white. Non-whites constituted 10 per cent of the total sample and 13.3 per cent of the Protestant group. Only 2.5 of the Roman Catholics and 0.2 per cent of the Jews were non-whites. Their numbers in the residue categories, Other Religion, No Religion, and Religion Not Reported ranged between 10.6 and 13 per cent, closely reflecting their proportions in the total population. But the major point is that nearly all non-whites (88 per cent) were Protestant, so that any statistic on the non-white Protestant group essentially refers to non-whites as a whole. Comparison of non-white Protestants, on the one hand, with the white Protestants, the Roman Catholics, and the Jews, on the other, therefore points out racial differentials rather than religious ones. Since this analysis is directed toward differentials among religious groups, it was decided to restrict the comparisons to white Protestants, Roman Catholics, and Jews. To permit the reader to compare the characteristics of these three major groups with those of the non-white Protestants, the data for the latter are included in the tables. The statistics for the total population include both the non-white Protestants and the other three small residue categories.

FINDINGS

Before turning to the major focus of this analysis, the evaluation of socioeconomic differentials by religion, a brief overview of the distribution of the American population by religious group and of the ecological and demographic features of the major religious groups is in order (Table 1). Of the total population 14 years old and over in 1957, two-thirds were identified as Protestant and one-fourth as Roman Catholic. Just over 3 per cent of the total gave Jewish as their religion; and 2.7 per cent (an overwhelming majority of whom were men) reported no religion. Under 1 per cent of the total did not report religion in answer to the question. This group was about equally divided between men and women, but it was more heavily concentrated in urbanized areas.

The different religious groups vary considerably in the distribution of their respective populations between urban[13] and rural

[13] The urban population comprised all persons living in (a) places of 2,500 inhabitants or more incorporated as cities, boroughs, and villages; (b) incorporated towns of 2,500 inhabitants or more ex-

places. In 1957, 64 per cent of the total U.S. population 14 years old and over lived in urban areas. But among the three major religious groups, the concentration in urban areas varied from 55 per cent of the white Protestants to 79 per cent of the Catholics and to virtually all (96.1 per cent) of the Jews. The differentials were, in fact, even sharper, since 87.4 per cent of all Jews lived in urbanized areas of 250,000 or more persons, in contrast to 53.9 per cent of the Catholics and only 24.5 per cent of the white Protestants.

TABLE 1

SELECTED SOCIAL CHARACTERISTICS OF PERSONS 14 YEARS OLD AND OVER BY RELIGION, UNITED STATES*

Religion	Number (in 1,000's)	Percentage Distribution by Religion	Percentage in Urban Areas	Percentage in Urbanized Areas of 250,000 or More	Percentage in Northeast	Percentage in South	Percentage Non-white	Males per 100 Females
Total	119,333	100.0	63.9	36.6	26.2	30.6	10.0	92.9
Protestant	78,952	66.2	56.6	27.2	16.8	38.3	13.3	89.0
White	68,475	57.4	55.2	24.5	17.1	35.1	....	89.4
Non-white	10,477	8.8	66.1	44.6	14.8	59.3	....	86.2
Roman Catholic	30,669	25.7	78.8	53.9	46.0	13.9	2.5	92.7
Jewish	3,868	3.2	96.1	87.4	69.1	7.7	0.2	92.7
Other Religion	1,545	1.3	77.4	52.9	41.9	17.4	11.7	100.9
No Religion	3,195	2.7	54.2	29.5	11.2	34.7	13.0	281.3
Religion Not Reported	1,104	0.9	68.2	49.5	23.3	33.7	10.6	98.6

* Based on data in U.S. Bureau of the Census, "Religion Reported by the Civilian Population of the United States: March 195 *Current Population Reports*, Series P-20, No. 79 (February 2, 1958).

These urban-rural differentials parallel the variations among the four major regions of the United States—Northeast, North Central, South, and West. The white Protestants are most concentrated in the South, which accounted for just over one-third of all white Protestants in the United States. Two-thirds of all those living in the South were white Protestants, in contrast to only 37 per cent of those in the Northeast. In fact, the Northeast accounted for only 17 per cent of all white Protestants in the United States compared with 46 per cent of all Roman Catholics and 69 per cent of the Jews. Like the South, the West and the North Central regions were heavily white Protestant (65 and 63 per cent, respectively). Thus Protestants constituted a minority in the population only in the Northeast, being exceeded by the larger group of Catholics with 45 per cent of the total. The heavy concentration of Jews in the Northeast, reflecting its highly urban characte also accounts for their being 8.5 per cent o: the population living there in 1957, compared to less than 3 per cent of the population in each of the other three regions. Ir fact, Jews constituted less than 1 per cent of the population living in the South. These differentials in urban-rural residence and in regional concentration help to explain the variations to be noted later in socioeconomic composition.

Variation in age composition is also a key factor in influencing social and economic differentials among population groups (Table 2). In 1957, the median age of those 14 years old and over in the United States was 40.4

cept in New England, New York, and Wisconsin, where the term "town" is used to designate minor civil divisions of counties; (*c*) densely settled urban fringe areas, around cities of 20,000 or more, and (*d*) unincorporated places of 50,000 or more outside of any urban fringe. The remaining population is classified as rural. U.S. Bureau of the Census, *ibid.*, p. 3.

ears. The median age of white Protestants 11.3) was close to that of the total popula-on.[14] In contrast, Roman Catholics had a iedian age of 38.7; their younger popula-on reflects the effects of their higher-than-verage fertility levels over the preceding ecades. The higher median age of the Jew-ih group, 44.5 years, no doubt results from heir low fertility, reinforced by the residue ffects of high levels of immigration in earli-r decades, resulting in a disproportionate iumber of older persons in 1957.[15] Thirteen per cent of the Jews were 65 and over compared with 10 per cent of the Catholics; but the Protestants had the highest concentration of older persons: 13.6 per cent of those 14 years old and over.

Education.—A major mechanism for social and occupational mobility in the United States is education. Various immigrant groups have placed differential emphasis on use of this mechanism for moving ahead.[16] Jews, in particular, have traditionally placed a very high value on education, including, in modern times, secular learning. In part, this stems from the intellectual content of Judaism; in part, it results from their perception of education as a means of social mobility. This high level of motivation for a good education is clearly reflected in the statistics (Table 3) that show the number of years of school completed by persons in the various religious groups. The median school years completed by Jews was 12.3 years compared with 11.3 for white Protestants and 10.4 for Roman Catholics. Thus, almost two full years of schooling separate the medians of Jews and Roman Catholics. But even these differentials mask sharp variations with respect to the proportion who have achieved certain levels of education. For example, 59 per cent of all Jews had at least a high school education, and 17 per cent had four years of college. In contrast, 44.9 per cent of the white Protestants and 39.6 per cent of the Roman Catholics had educational levels equal to at least a high school diploma, and only 7.7 and 6 per cent of the persons in these two major religious groups had graduated from college.

These differentials characterize both men and women. For example, among Jewish males, 38.2 per cent had some college education in contrast to 14.7 per cent of the

TABLE 2

PERCENTAGE DISTRIBUTION BY AGE OF PERSONS 14 YEARS OLD AND OVER BY RELIGION, TOTAL UNITED STATES*

Religion	Total 14 Years and Over	14–19 Years	20–24 Years	25–44 Years	45–64 Years	65 Years and Over	Median Age (Years)
Total	100.0	11.7	8.2	39.0	28.8	12.3	40.4
Protestant	100.0	11.8	8.0	38.1	29.0	13.0	40.8
White	100.0	11.5	7.7	37.7	29.5	13.6	41.3
Non-white	100.0	14.2	10.0	41.0	26.0	8.9	37.2
Roman Catholic	100.0	12.1	8.7	42.3	27.0	10.0	38.7
Jewish	100.0	9.0	6.0	35.9	36.0	13.0	44.5
Other Religion and Not Reported	100.0	10.6	7.9	34.2	32.0	15.4	43.5
No Religion	100.0	9.0	9.2	37.1	30.9	13.8	42.0

* Based on data in U.S. Bureau of the Census, "Religion Reported by the Civilian Population of the United States: March 1957," *Current Population Reports*, Series P-20, No. 79 (February 2, 1958), Table 4.

[14] Wilson H. Grabill, Clyde B. Kiser, and Pascal K. Whelpton, *The Fertility of American Women* (New York: John Wiley & Sons, 1958), pp. 51–79.

[15] Glick, *op. cit.*, pp. 36–37.

[16] Milton M. Gordon, *Assimilation in American Life* (New York: Oxford University Press, 1964), pp. 185–87.

Catholic men and 18.6 per cent of the white Protestant men. Among Jewish women, 22.5 per cent had some college education compared with 9.1 per cent of the Catholic women and 15.4 per cent of the white Protestant women. In all religious groups, it was more common for men to complete their college education than for women. In fact, considerably higher proportions of men, especially in the Jewish group, completed college education than received only some college training, whereas among women, proportionately more had only one to three years of college education than received a college degree.

Unfortunately, the data on education by religion are not cross-tabulated by age; it is, therefore, not possible to ascertain the extent to which these religious differentials are narrowing among the younger age groups. In the United States as a whole, the over-all increase in the level of education among the younger segments of the population is evidenced by the data from the March, 1967, Current Population Survey, which show a continuous rise in the median school years completed, from 8.5 among males aged 65 and over to 12.6 among men aged 25–29, and from 8.7 to 12.5 for females.[17] In addi-

[17] U.S. Bureau of the Census, "Educational Attainment: March 1967," *Current Population Survey*, Series P-20, No. 169 (February 9, 1968).

TABLE 3

PERCENTAGE DISTRIBUTION OF YEARS OF SCHOOL COMPLETED BY PERSONS 25 YEARS OLD AND OVER BY RELIGION AND SEX, TOTAL UNITED STATES*

Years of School Completed and Sex	Total	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Males:					
Elementary: 0–7	23.2	18.7	55.4	22.9	14.7
8	18.5	19.3	10.9	18.9	13.1
High school: 1–3	17.3	18.1	15.4	18.2	9.7
4	22.1	24.0	10.0	24.0	21.5
College: 1–3	7.3	8.2	2.8	6.6	12.6
4 or more	9.4	10.4	2.3	8.1	25.6
Not Reported	2.2	1.3	3.2	1.3	2.8
Total percentage	100.0	100.0	100.0	100.0	100.0
Median school years completed	10.3	10.9	Under 8	10.3	12.5
Females:					
Elementary: 0–7	20.3	15.7	48.7	21.1	16.6
8	17.4	17.6	13.5	18.9	13.1
High school: 1–3	18.1	18.9	17.6	18.4	10.2
4	29.5	31.5	12.7	31.4	35.8
College: 1–3	7.4	8.9	3.3	5.0	12.8
4 or more	5.7	6.5	3.0	4.1	9.7
Not Reported	1.6	0.9	1.2	1.1	1.8
Total percentage	100.0	100.0	100.0	100.0	100 0
Median school years completed	10.9	11.6	8.1	10.5	12.3
Total:					
Elementary: 0–7	21.7	17.1	51.8	22.0	15.6
8	17.9	18.4	12.3	18.9	13.1
High school: 1–3	17.7	18.5	16.6	18.3	10.0
4	26.0	27.9	11.4	27.8	29.0
College: 1–3	7.3	8.6	3.1	5.8	12.7
4 or more	7.5	8.4	2.7	6.0	17.3
Not Reported	1.9	1.1	2.1	1.2	2.3
Total percentage	100.0	100.0	100.0	100.0	100.0
Median school years completed	10.6	11.3	Under 8	10.4	12.3

* Data in this and the remaining tables in this paper are based on information in U.S. Bureau of the Census, "Tabulations of Data on the Social and Economic Characteristics of Major Religious Groups, March 1957" (unpublished).

tion, community survey data have indicated that the median education level among the younger members of a religious group is considerably above that of the older segments. In the Providence, Rhode Island, metropolitan area, for example, the median education for Jewish males aged 25–44 was 15.9 years compared with 8.3 for those aged 65 and over.[18] In the general population the corresponding medians for these two age groups were 12.1 and 7.9, respectively. Although confirming a considerable improvement in educational level among younger segments of the population, these data do not, in comparison with the Jewish medians, suggest a narrowing of differentials. They may, however, be too restricted to provide a fair basis for testing this hypothesis.

Labor force participation.—For the United States as a whole in 1957, 81.1 per cent of all males 14 and over and 35.1 per cent of all females were in the labor force (Table 4). The proportion for males varied only minimally among the three major religious groups, between the narrow range of 81.1 and 81.6 per cent. For the females, there was slightly greater variation. The lowest level characterized Jewish women, of whom 30.7 per cent were in the labor force. Intermediate were the white Protestant women, with 33.7 per cent in the labor force, followed by the Roman Catholics with 35.8 per cent.

The over-all similarity in the labor force participation rate of males masks some significant differences by religion within specific age groups. Reflecting the higher educational achievement of Jews which results in many remaining in school instead of entering the labor force, the levels of labor force participation by Jews aged 18–24 is considerably below that of Roman Catholics and Protestants—only 53.9 per cent in contrast to approximately 80 per cent of the Protestants and Roman Catholics. By ages 25–34, however, and continuing through the 35–44-year group, all groups had very high labor force participation rates. Differences become apparent again in the 45–64-year group and are accentuated in the 65

TABLE 4

LABOR FORCE PARTICIPATION RATES OF PERSONS 14 YEARS OLD AND OVER BY RELIGION, AGE, AND SEX, TOTAL UNITED STATES

Age and Sex	Total*	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Both sexes	57.0	55.8	60.3	57.6	55.1
Male	81.1	81.1	80.3	81.6	81.5
14–17 years	30.5	32.7	32.7	26.7	†
18–24 years	79 1	78.5	82.1	80.0	53.9
25–34 years	97.0	96.9	96.4	97.2	97.0
35–44 years	97.8	97.9	96.9	98.1	99.1
45–64 years	92.7	93.4	89.3	92.5	96.1
65 years and over	37.4	38.7	37.0	34.1	46.9
Female	35.1	33.7	43.3	35.8	30.7
14–17 years	17.7	18.5	11.5	19.1	†
18–24 years	45.5	42.3	40.2	54.4	57.2
25–34 years	34.8	33.6	48.5	32.8	25.5
35–44 years	42.6	41.2	58.1	40.5	33.5
45–64 years	41.1	40.3	51.3	39.9	38.2
65 years and over	11.5	11.6	13.2	11.0	8.5

* Includes, in addition to the groups shown separately, persons with Other Religion or Religion Not Reported.

† Base is less than 150,000.

[18] Sidney Goldstein and Calvin Goldscheider, *Jewish Americans: Three Generations in a Jewish Community* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968), pp. 67–70.

and over age group. In particular, these latter differences largely involve the higher participation rates of older Jewish males in contrast to older Protestants and Roman Catholics. Whereas only 34.1 per cent of the Catholic men and 38.5 per cent of the Protestant men 65 years old and over remained in the labor force, this was true of 46.9 per cent of the older Jewish males. This differential reflects the higher proportion of professional and self-employed persons among Jews. To the extent that retirement from the labor force is more voluntary for professionals and the self-employed generally, the disproportionate number of Jews in these categories contributes to their higher-than-average labor force participation rates in the older age groups.[19]

For females, too, the age-specific labor force participation patterns of Jews differ from those characterizing the total adult groups. In the 18–24-year age group, Jewish women have the highest labor force participation rate, with 57 per cent in the labor force compared to 54 per cent of the Roman Catholics and 42 per cent of the Protestants. In all succeeding age groups, however, the labor force participation rates of Jewish women are below those of the other two religious groups. Also noteworthy, the labor force participation rate of Catholic women 18–24 is higher than that of Protestants and not far below that of Jewish women. The differences between Roman Catholics and white Protestants are not great, however, for age groups above 25. The reasons for the fairly close similarity in the relatively high labor force participation rates of Jewish and Roman Catholic women aged 18–24 are not clear. The high rates for the Jewish group may reflect their relatively high educational achievement accompanied by a somewhat later age at marriage with the consequent greater opportunity to use the skills obtained by education. Greater and more successful use of family limitation may also contribute to this pattern.[20] However, these factors are not operating to the same degree in the Roman Catholic female population, and other explanations for the similarity must be sought. Since this difference persists even when the comparison is restricted to the urban population,[21] it does not seem to stem from the higher concentration of Catholics or Jews in urban places, although greater refinement by size of urban place might reduce some of the differentials.

Further insight into the extent of religious differentials in labor force participation can be gained from examination of participation rates of (1) married women with varying numbers of children present in the household (Table 5). The over-all levels of participation vary only minimally by religious group for all married women whose husbands were present in the household (between 27.3 and 29.6 per cent). But for each of the age groups under 45 years whether or not the women participated in the labor force, varied considerably by religion and consistently for all age group under 65 years. Especially for those between 25 and 45, the participation rates of Jewish women were lowest, followed by those of Roman Catholic women, then white Protestant women. Over-all, these data suggest therefore, that the underlying factors accounting for the low participation rates of Jewish women and somewhat low rates for Roman Catholic women permeate all age groups under 45. For those groups 45 and over, for whom there are sufficient numbers of cases to calculate rates, the differences among the three major groups are minimal.

Religious differentials also persist when the participation rates are examined for the presence or absence of children of different ages. In all three religious groups, the presence of children under 6 years of age reduces the participation levels considerably. For example, in all religious groups the participation rates of women with children under 6

[19] *Ibid.*, pp. 71–91.

[20] Pascal K. Whelpton, Arthur A. Campbell, and John E. Patterson, *Fertility and Family Planning in the United States* (Princeton, N.J.: Princeton University Press, 1966), pp. 69–124 and 221–99.

[21] For the urban data, see Table 11 in "Characteristics of Major Religious Groups, March, 1957" (see n. 11 above).

years is 50 per cent or less than the rates of those with children 6–17 years but with none under 6 years of age.

Yet, the participation levels of Jewish women for each category of number of children present at home is below that of Roman Catholic and Protestant women, even in the group with no children under 18 years of age, but especially in the groups with young children under 18 years of age, but especially in the groups with young children. For example, among those with children under 6 years old, only 11.8 per cent of Jewish women, compared with 18.2 per cent of white Protestant women, were labor force participants. For this category, the level of participation of Roman Catholic women is more similar to that of Jewish women than to white Protestant women. In contrast, in those categories with children 6–17 years of age only and with no children under 18 years of age, the levels for Roman Catholic women were much more similar to those of white Protestant women than to the Jewish women.

The lower participation levels of Jewish women at all ages except 18–24 (Table 4), and for all categories of family status (Table 5), as measured by presence of children, suggests that their higher socioeconomic status plays a key role in influencing participation levels; but the impact of SES is augmented by the presence of small children. The cross-sectional data suggest that for Jews, as for members of other religious groups, marriage and especially the presence of children under age 6 leads to a considerable exodus from the labor force. Participation levels generally rise again with increasing age of women (through the 45–64 group). However, they do not reach the high levels characterizing the 18–24 age group, although they closely approach it for white Protestants. Among Catholics the low level of participation by women with children under age 6 may stem from the larger number of young children in these families and the need to remain home to care for them. But as these children grow up and go to school, the sharp rise in participation rate of Catholic women suggests that the pressure to go to work to help support the family may be greater than in other groups of higher status. Moreover, even when some increase in labor force participation occurs, as mong Jews, it may be for somewhat different reasons as, for example, the desire to utilize occupationally the special skills obtained in higher education. In short, variations in SES, coupled with differences in the presence of young children and the size of family, may be key factors in influencing the

TABLE 5

LABOR FORCE PARTICIPATION RATES FOR MARRIED WOMEN LIVING IN SAME HOUSEHOLD AS HUSBAND, BY RELIGION, AGE, AND PRESENCE OF CHILDREN, TOTAL UNITED STATES

Age and Presence of Children	Total	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Total married women, husband present	29.6	29.6	40.4	27.3	27.8
Age:					
Under 25 years	29.1	29.2	26.0	30.6	*
25–34 years	27.2	28.1	38.2	22.6	18.7
35–44 years	35.7	36.1	51.1	31.5	24.5
45–64 years	32.3	31.6	45.0	30.9	30.6
65 years and over	6.4	6.2	*	6.7	*
Presence of children:					
No own children under 18 years	35.6	34.1	47.7	36.6	30.0
With children 6–17 years, none under 6 years	36.7	36.2	52.6	35.3	28.6
With children under 6 years	17.0	18.2	23.6	13.2	11.8

* Base is less than 150,000.

pattern of religious differentials in levels of labor force participation of women at various points in the life cycle.

Occupational composition.—Paralleling the earlier noted differentials in educational achievement, the major religious groups are also characterized by sharp differentials in occupational composition, particularly those between the Jewish group, on the one hand, and the Protestant and Roman Catholic groups, on the other (Table 6). Three-fourths of all Jewish employed males work in white-collar positions compared with just over one-third of the Roman Catholic and white Protestant males. This large differential extends to three of the four subcategories of white-collar employment. The proportion of Jewish males in the professions was approximately twice as large as those of Roman Catholics and white Protestants.

TABLE 6

PERCENTAGE DISTRIBUTION OF EMPLOYED PERSONS 18 YEARS OLD AND OVER BY MAJOR OCCUPATION GROUP, BY RELIGION AND SEX, TOTAL UNITED STATES

Major Occupation and Sex	Total	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Males:					
Professional	9.9	10.9	2.6	8.9	20.3
Wage and salary workers	8.4	9.5	2.3	7.5	12.8
Self-employed	1.5	1.4	0.3	1.3	7.5
Farmers and farm managers	7.3	9.5	5.8	3.8	0.1
Managers and proprietors	13.3	14.1	2.2	12.5	35.1
Wage and salary workers	6.3	7.3	0.5	5.7	10.8
Self-employed	7.0	6.7	1.7	6.8	24.3
Clerical workers	6.9	6.8	4.1	8.4	8.0
Sales workers	5.4	6.0	0.5	4.8	14.1
Skilled laborers	20.0	21.2	9.7	22.5	8 9
Semi-skilled laborers	20.9	19.7	27.0	22.4	10.1
Service workers	6.1	4.3	14.1	7.7	2.3
Farm laborers	2.5	2.2	7.6	1.7	0.1
Unskilled laborers	7.7	5.4	26.4	7.4	0.8
Total percentage	100.0	100.0	100.0	100.0	100 0
Total white collar*	35.5	37.8	9.4	34.6	77.5
Total blue collar†	57.2	52.8	84.8	61.7	22 2
Females:					
Professionals	12.2	13.7	6.2	11.4	15.5
Wage and salary workers	11.6	12.8	6.1	11.0	15 1
Self-employed	0.7	0.9	0.1	0.4	0 4
Farmers and farm managers	0.7	0.8	1.4	0.3	0.2
Managers and proprietors	5.5	6.5	1.4	4 3	8 9
Wage and salary workers	2.3	3.0	0.4	1.3	3 2
Self-employed	3.2	3.5	1.1	3.0	5 7
Clerical workers	30.3	32.1	7.2	35.6	43.9
Sales workers	6.9	8.0	0.9	6.4	14.4
Skilled laborers	1.0	1.1	0.3	1.3	0.7
Semi-skilled laborers	17.1	15.8	14.1	21 8	11 2
Private household workers	9.6	5.6	43.8	4.9	0.5
Service workers	13.1	12.7	19.4	11.6	4.6
Farm laborers	3.0	3.3	4.1	1.9	....
Unskilled laborers	0.6	0.5	1.3	0.5	....
Total percentage	100.0	100.0	100.0	100.0	100 0
Total white collar	54.9	60.3	15 7	57.7	82.7
Total blue collar	44.4	39.0	83.0	42.0	17.0

* Includes professionals, managers except farm, clerical, and sales workers.

† Includes skilled, semi-skilled, household, service, farm, and unskilled laborers.

The proportion who were managers, officials, and proprietors (35 per cent) was almost three times as great as the corresponding proportion of Roman Catholics and two and one-half times as great as the proportion of white Protestants. Among both the professional and the managerial groups, the proportion of males who were self-employed varied significantly by religious group. Among white Protestants and Catholics, about one out of eight professionals was self-employed; among Jews this was true of one out of three. Within the managerial group, self-employment was more characteristic of all three groups, but differentials persisted, although at a lower level. About five out of every ten Protestant and Catholic males were self-employed compared with seven of every ten Jews. In clerical work Roman Catholics had a slightly higher proportion of males than did the Jewish group, and both were higher than the white Protestant group; but Jews had from two to three times as many males employed as sales workers than was true of the white Protestant and Catholic groups. In part, the latter may reflect the high proportion of Jews who owned their own businesses and who employed family members as workers.

Conversely, the proportion of males employed as blue-collar workers (craftsmen, operatives, service workers, and laborers) was highest for Roman Catholics (61.7 per cent), followed by white Protestant males (52.8 per cent). Only one out of every five Jewish males held a blue-collar job. The one remaining category, not included in the white-collar group, is that of farmers and farm managers. This occupation was most prevalent among white Protestants of whom 9.5 per cent were farmers or farm managers, compared to only 3.8 per cent of Roman Catholics and just a sprinkling of Jews. Over-all, therefore, these differences show rather small variations between Roman Catholics and white Protestants in occupational distribution of males but considerable differentials between the Jews and the two Christian groups.

Compared with males, women in the labor force are much more concentrated in white-collar positions. This is true of all three religious groups. Although concentration in white-collar jobs is more prevalent for Jews than for either Roman Catholic or white Protestant women, the differentials among the women are less marked than was true among the men. Just over four out of every five Jewish women are in white-collar jobs compared with about three out of every five Catholic and white Protestant women. Examination of the specific occupational categories also points to a narrowing of differentials among women. For example, the number employed as professionals varies only between 11.4 and 15.5 per cent among the three groups. About twice as many Jewish women as Roman Catholic women are employed as managers, but the difference between Jewish and white Protestant women is considerably smaller. Among professionals, self-employment is far less common among women than among men. It ranges between 3 and 7 per cent of professional women in the three religious groups and is about equally high for white Protestant and Jewish women. Within the managerial group, self-employment characterizes a majority of the women in each group: among Catholics, seven out of ten are self-employed; among Jews, six out of ten, and among white Protestants, five out of ten. In contrast to men, therefore, Jewish women are not as disproportionally represented among the self-employed, especially in view of the fact that, if they were working without pay in businesses operated by their husbands, they would be counted as self-employed. This may be related to the tendency for Jewish women to withdraw from the labor force to a greater degree following marriage and family formation and not to return as often once children grow up; self-employment is more likely to be associated with continued participation in the labor force. But the relatively small percentage of women employed as managers and proprietors and the even smaller proportion which the self-employed constitute of the total female labor force precludes attaching too

much significance to the religious differences noted.

One-third or more of the women in each of the three major groups hold jobs as clerical workers, but the proportion so employed is higher for Jewish women (43.9 per cent), compared with non-Jewish women (32.1 and 35.6 per cent). One of the sharpest differences characterized the proportion of women in the three religious categories who are employed as sales workers. Among Jewish women, 14.4 per cent hold such jobs, compared with 8 and 6.4 per cent of the white Protestant and Roman Catholic women. As with males, the proportion of Jewish women in blue-collar jobs is very small, only 17 per cent of the total, whereas for the white Protestant and Catholic women, approximately two out of every five hold blue-collar jobs.

Special tabulations of survey data from the National Opinion Research Center show quite similar patterns of occupational distribution among the three religious groups.[22] These statistics also show, for heads of households, the occupational distribution of the major denominations within the Protestant group. They demonstrate clearly that the Protestant group is by no means homogeneous; in many respects the distribution of Episcopalians and Presbyterians resembled that of the Jewish group. Unfortunately, the census data do not subdivide the Protestant group beyond the distinction between white and non-white, and it is not possible therefore to pursue this kind of comparison. An advantage of the census tabulations, however, lies in that the occupational data have been refined by showing the composition for the urban population, standardized by years of school completed. By restricting the data to a more homogeneous social and economic environment and by holding constant the wide differences in educational achievement, it becomes possible to ascertain more clearly the extent to which occupational differences are directly related to religious affiliation and to what extent they may simply be a reflection of differential opportunities available to persons of various religious affiliation because of the place in which they live and the level of education they have achieved.

Restricting the data to the urban population and standardizing this population by years of completed schooling results in a considerable narrowing of differentials and to actual reversal in the case of some occupational categories (Table 7). With residence and education controlled, 69.7 per cent of the Jewish males are shown to be white-collar workers compared with 42.7 per cent of the white Protestant males and 39.1 per cent of the Roman Catholics. Thus, the concentration of Jews in white-collar positions remains far above that of white Protestant or Catholic males, but the difference is no longer in the ratio of two to one as was the case with the unstandardized data. Moreover, for selected occupation categories there is also a dramatic change. For example, with residence and education controlled, only 9.9 per cent of the Jewish males are professionals in contrast to 11.2 per cent of the Roman Catholics, and 12.1 per cent of the Protestants. On the other hand, the earlier sharp differentials with respect to the managerial and the sales-worker categories remain about the same. Also, the standardized data do not lead to any change in the earlier noted distribution of professional and managerial persons between self-employed and wage and salary workers. Similar conclusions hold for occupational differentials for females after the data are restricted to urban residence and standardized by education. Over-all, therefore, controlling for both education and residence suggests that both these factors explain some of the variation in religious differentials in occupation composition. Yet, despite narrowing differentials persist, suggesting that other factors contribute to the variation in occupational composition among religious groups, and particularly between the Jewish group, on the one hand, and the white Protestant and Roman Catholic groups, on the other.

[22] Bogue, *op. cit.*, pp. 702–5.

In a further attempt to assess the role of education in occupation for the major religious groups, the census made a special tabulation of the occupational distribution of employed college graduates in urban areas (Table 8). The data are not presented separately by sex. It will be recalled that 8.4 per cent of the white Protestants, 6 per cent of the Roman Catholics, and 17.3 per cent of the Jews were college graduates. Of these three groups of college graduates, the Roman Catholics have the highest proportion, 66.1 per cent, employed as professionals, compared with 62 per cent of the white Protestants and 58.2 per cent of the Jews. In contrast a significantly higher proportion of Jewish college graduates earn their living as managers, proprietors, and

TABLE 7

PERCENTAGE DISTRIBUTION OF EMPLOYED PERSONS 18 YEARS OLD AND OVER BY MAJOR OCCUPATION GROUP, BY RELIGION AND SEX, URBAN UNITED STATES (STANDARDIZED BY YEARS OF SCHOOL COMPLETED)

Major Occupation and Sex	Total	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Males:					
Professionals	11.5	12.1	10.2	11.2	9.9
Wage and salary workers	9.7	10.5	8.9	9.5	6.3
Self-employed	1.8	1.6	1.3	1.7	3.6
Farmers and farm managers	0.4	0.6	0.2	0.1	....
Managers and proprietors	14.6	15.3	2.8	12.8	36.8
Wage and salary workers	7.3	8.4	0.6	6.3	9.3
Self-employed	7.3	6.9	2.2	6.5	27.5
Clerical workers	8.6	8.3	9.0	9.8	8.0
Sales workers	6 3	7.0	0.8	5.3	15.0
Skilled laborers	21.3	23.9	11.0	22 2	11.7
Semi-skilled laborers	21.7	21.3	28.1	22.2	14.0
Service workers	7.7	5.9	16.2	8.5	3.4
Farm laborers	0.3	0.3	0.2	0.3	0.1
Unskilled laborers	7.7	5.2	21.6	7.5	1.1
Total percentage	100.0	100.0	100.0	100.0	100.0
Total white collar*	41.0	42.7	22.8	39.1	69.7
Total blue collar†	58.7	56.6	77.1	60.7	30.3
Females:					
Professionals	12.5	12.6	9.5	13.9	8.9
Wage and salary workers	11.7	11.6	9.3	13.4	8.5
Self-employed	0.8	1.0	0.2	0.5	0.4
Farmers and farm managers	....	0.1	....	....	....
Managers and proprietors	5.3	6.3	1.9	3.9	8.9
Wage and salary workers	2.4	3.1	0.5	1.3	3 0
Self-employed	2.9	3.2	1.4	2.6	5 9
Clerical workers	33.5	33.6	15 7	37.1	41.3
Sales workers	7.1	8.4	1.6	6.1	19.0
Skilled laborers	1.1	1.3	0 3	1.2	1.0
Semi-skilled laborers	17.7	16.8	16.5	21.6	15.1
Private household workers	8.8	6.3	29.9	4.4	0.4
Service workers	13.3	14.0	23.2	11.2	5.5
Farm laborers	0.1	0.1	0.1	....	....
Unskilled laborers	0.5	0.5	1.1	0.5	....
Total percentage	100.0	100.0	100.0	100.0	100.0
Total white collar*	58.4	60.9	28.7	61.0	78.1
Total blue collar†	41.5	39.0	71.1	38.9	22.0

* Includes professionals, managers except farm, clerical, and sales workers.

† Includes skilled, semi-skilled, household, service, farm, and unskilled laborers.

officials—22 per cent, compared with only 12.4 per cent of the Roman Catholics and 16.3 per cent of the white Protestants. The absolute differentials for most of the remaining categories are relatively small, with only slightly higher proportions of Jews being employed in clerical and sales work. These data for college graduates alone, therefore, suggest more homogeneous patterns of occupational composition within the college graduates of the three major religious groups. But the fact that differentials persist, as they did for the total male population with education standardized, indicates that religion itself, or at least other factors associated with religion, do have an impact on occupational composition.

Income.—For each person in the sample, information was solicited on the amount of money income received in 1956. This included income from wages and salaries, from self-employment, and from such other sources as pension payments, interests, dividends, net rental income, unemployment benefits, and public assistance (Table 9). Income differentials among the religious groups are to be expected on the basis of variations in occupational composition and educational achievement. Since both high educational achievement and concentration in high white-collar positions are highly correlated with income, the fact that the $4,900 median income of Jewish males is well above that of the white Protestant and the Roman Catholic males ($3,728 and $3,954, respectively) is largely explainable in these terms. This sharp differential is also reflected in the more detailed statistics on distribution by income class. Incomes of $10,000 and over in 1956 were reported by 17 per cent of the Jewish males, compared with only 3.9 per cent of the white Protestants and 2.7 per cent of the Roman Catholics. On the other hand, just over one-fourth of the Jews, but one-third of the Catholics, and almost 40 per cent of the white Protestants, had incomes under $3,000.

These differences extended to females as well. The median income of $1,663 of Jewish women was about one-fifth higher than that of Roman Catholic women ($1,470) and almost 50 per cent more than that of white Protestant women ($1,198). Again, this probably reflects differentials in occupational composition. The differences in medians reflect variations in the over-all income distribution. Considerably fewer women had incomes above $5,000—only 2.6 per cent of the total population. Yet, this was true of 7.4 per cent of the Jewish group, compared with only 3.2 per cent of the white Protestants and 2.5 per cent of the Roman Catholics. A majority of the women in every religious group had incomes under $2,000, but this range encompassed 55.6 per cent of the Jewish women, 59.8 per cent of the Roman Catholics, and 65.8 per cent of the white Protestants.

In an attempt to make income data more comparable, the Bureau of the Census presents income data for the urban employed population standardized for major occupational group (Table 10). Doing so eliminates considerably the sharp differentials noted for the unstandardized data for the total United States. For males, the standardized data show a median income for Jews of $4,773, just slightly above the $4,553 for white Protestants and the $4,509 for Roman Catholics. For women, standardization also eliminates the major part of the differential; the median income of Jewish women becomes $2,352, compared with $2,282 for Roman Catholics and $2,263 for white Protestants.

The narrowing of differentials suggested by the similar median income figures extends to the over-all distribution by income level. For the standardized data, 18 per cent of the Jewish males, compared with 21 per cent of both the Roman Catholics and the white Protestants had incomes below $3,000. At the other extreme, the proportion of Jewish males with incomes of $10,000 and over is reduced to 8.7 per cent from the unstandardized 17.2 per cent, that of white Protestant males is raised to 5 per cent, and that of Roman Catholics to 3.9 per cent. The same narrowing in the distributions appears for women. In the standardized

TABLE 8

PERCENTAGE DISTRIBUTION OF EMPLOYED COLLEGE GRADUATES IN URBAN AREAS BY MAJOR OCCUPATION GROUP, BY RELIGION

Major Occupation Group	Total	White Protestant*	Roman Catholic	Jewish
Professional	63.2	62.0	66.1	58.2
Managers and proprietors	15.7	17.1	12.4	22.1
Clerical workers	8.2	8.3	7.4	8.9
Sales workers	5.8	5.8	6.1	7.8
Skilled laborers	3.2	3.6	3.8	0.9
Semi-skilled laborers	1.5	1.3	1.5	1.3
Other occupations	2.4	1.8	2.9	0.9
Total percentage	100.0	100.0	100.0	100.0

* Non-white Protestant group is not shown because base is less than 150,000.

TABLE 9

PERCENTAGE DISTRIBUTION OF PERSONS 14 YEARS OLD AND OVER BY INCOME IN 1956, BY RELIGION AND SEX, TOTAL UNITED STATES

Income and Sex	Total*	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Males:					
Under $1,000	17.2	16.5	31.6	14.0	10.0
$1,000–$1,999	11.7	11.2	18.3	10.5	9.0
$2,000–$2,999	12.1	11.6	20.0	11.0	7.4
$3,000–$3,999	14.8	14.7	16.1	15.2	11.0
$4,000–$4,999	15.9	16.2	9.4	18.3	14.0
$5,000–$5,999	11.9	12.2	3.1	14.6	13.4
$6,000–$9,999	12.7	13.7	1.5	13.7	18.0
$10,000 and over	3.6	3.9	0.1	2.7	17.2
Total percentage	100.0	100.0	100.0	100.0	100.0
Median income	$3,608	$3,728	$2,005	$3,954	$4,900
Females:					
Under $1,000	46.9	46.1	64.4	41.3	39.0
$1,000–$1,999	19.3	19.7	20.2	18.5	16.6
$2,000–$2,999	15.7	15.6	9.8	18.6	15.2
$3,000–$3,999	11.0	10.6	4.4	14.3	15.1
$4,000–$4,999	4.3	4.7	0.9	4.8	6.5
$5,000–$5,999	1.5	1.7	0.2	1.6	3.6
$6,000–$9,999	0.9	1.2	0.1	0.7	2.3
$10,000 and over	0.2	0.3	....	0.2	1.5
Total percentage	100.0	100.0	100.0	100.0	100.0
Median income	$1,146	$1,198	$ 776	$1,470	$1,663

* Includes, in addition to the groups shown separately, persons with Other Religion and those with No Religion or Religion Not Reported.

data, 41 per cent of the Jewish women, compared with 43 per cent of the Roman Catholics and 44 per cent of the white Protestants, had incomes under $2,000. At the other extreme, 5.2 per cent of the Jewish women, 3.8 per cent of the Roman Catholics, and 5.0 per cent of the white Protestants had incomes of $5,000 and over.

Clearly, then, with only slight differences apparent both in the median income and in the distribution among income groups, the considerably higher income level characterizing Jewish males and females, compared with white Protestant and Roman Catholic men and women, is a function of both their concentration in urban areas and in high white-collar positions. Taking these two variables into account greatly reduces and virtually eliminates whatever income differences otherwise exist. This suggests that, as educational differentials among Protestants, Roman Catholics, and Jews narrow, and that, as increasing proportions of both white Protestants and Catholics enter white-collar positions, the existing income differentials between Jews and the two Christian groups will also diminish.

This conclusion is justified by the data showing median income according to years of schooling completed (Table 11). For all three groups, the median income level rises consistently with increasing education. For those with less than an eighth-grade education, the median income varies between $2,609 for Jews to just over $2,800 for white Protestants and Roman Catholics. Except for the group with one to three years of high school, the difference in median income for Jews, white Protestants, and Catholics is less than 10 per cent for all educational categories below the college level. For the college groups, however, the range increases, with Jews having the highest median incomes and Catholics the lowest. The differential is particularly sharp at the four-year or more educational level, at which the median income of Jews is $8,041 compared with $6,375 for white Protestants and $5,727 for Roman Catholics. But this sharp differential may be somewhat misleading since that particular educational category includes those who have gone on to postgraduate work, which survey data has shown to be true of a considerably higher proportion of Jews than of Catholics or Protestants.[23] This differential in education and the corresponding differential in median income are both related to the likelihood that a higher proportion of Jews who have postgraduate education and who are in professional categories hold high-income positions, such as doctors or lawyers. The higher educational achievement of the Jewish group may also permit them to hold more responsible managerial positions in commerce, manufacturing, or as self-employed proprietors in wholesale and retail establishments.[24] For analytic purposes, it would be desirable to have a more detailed breakdown of the college-educated group, particularly in the future, when proportionately more persons in the population will have a postgraduate education.

The question of whether religion, occupation, and/or education is a more important factor in determining the income level of individuals cannot be clearly determined without further controls. Control for major occupation groups and for place of residence reduces considerably the income differentials in the three major religious groups, but it does not eliminate them completely, particularly with respect to the distribution among income categories. Similarly, comparisons of income level (judged by the median) among various educational categories suggest minimal differences among most categories. Even where large differ-

[23] E.g., in the Providence study of the Jewish community, 25.3 per cent of the adult Jewish population had four or more years of college compared with only 6.5 per cent of the total population (Jews and non-Jews). But of the 25.3 per cent, just over half (13.4 per cent) had some postgraduate education, a percentage which in itself was twice as high as the proportion of the total population which had four or more years of college education, and even slightly above the total proportion in the population who had had any college education (13.1 per cent) (Goldstein and Goldscheider, *op. cit.*, p. 65).

[24] Bogue, *op. cit.*, p. 704.

TABLE 10

PERCENTAGE DISTRIBUTION OF EMPLOYED PERSONS 14 YEARS OLD AND OVER BY INCOME IN 1956, BY RELIGION AND SEX, URBAN UNITED STATES (STANDARDIZED FOR MAJOR OCCUPATION GROUP)*

Income and Sex	Total†	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Males:					
Under $1,000	5.6	5.8	12.4	5.2	4.1
$1,000–$1,999	6.1	5.5	12.7	6.2	6.4
$2,000–$2,999	10.8	9.7	24.1	10.0	7.6
$3,000–$3,999	17.4	17.1	21.2	17.1	13.9
$4,000–$4,999	21.4	21.5	19.2	22.6	23.3
$5,000–$5,999	16.0	16.4	6.4	17.3	17.0
$6,000–$9,999	17.6	19.0	3.5	17.7	18.9
$10,000 and over	5.0	5.0	0.5	3.9	8.7
Total percentage	100.0	100.0	100.0	100.0	100.0
Median income	$4,472	$4,553	$3,038	$4,509	$4,773
Females:					
Under $1,000	23.2	23.5	25.0	23.8	22.5
$1,000–$1,999	20.6	20.1	30.1	19.4	18.8
$2,000–$2,999	24.3	24.3	20.7	24.1	24.7
$3,000–$3,999	19.6	18.7	18.2	21.8	19.1
$4,000–$4,999	7.8	8.5	4.3	7.2	9.7
$5,000–$5,999	2.7	3.0	0.9	2.4	2.8
$6,000–$9,999	1.4	1.7	0.7	1.0	1.7
$10,000 and over	0.3	0.3	....	0.4	0.7
Total percentage	100.0	100.0	100.0	100.0	100.0
Median income	$2,255	$2,263	$1,831	$2,282	$2,352

* Standard used was distribution by major occupation group within each sex group of total urban employed in the three major religious groups combined.

† Summation of groups shown separately only; information not available for urban residents of other religions.

TABLE 11

MEDIAN INCOME IN 1956 OF URBAN MEN 14 YEARS OLD AND BY RELIGION AND YEARS OF SCHOOL COMPLETED

Years of School Completed	Total	White Protestant	Non-white Protestant	Roman Catholic	Jewish
Elementary:					
0–7	$2,654	$2,812	$2,249	$2,819	$2,609
8	3,631	3,712	2,864	3,729	3,844
High school:					
1–3	3,858	3,850	2,849	4,170	4,672
4	4,563	4,684	3,092	4,567	4,913
College:					
1–3	4,526	4,712	*	4,361	5,026
4 or more	6,176	6,375	*	5,727	8,041

* Base is less than 150,000.

ences exist, they may be largely attributable to the heterogeneous educational and occupational composition of the particular education category, that is, those with four years or more of college. Obviously, a number of intervening variables operate, and full evaluation of their effect must await more comprehensive statistics. Yet, standing out above these differences is the fact that the range of differences by education within each of the three major religious groups is far greater than the differences in median income level among the three religious groups. For example, whereas the median income level for the total males varied between a low $3,728 for white Protestants and a high of $4,900 for Jews, the range in median incomes between the highest and lowest educational levels of each of the religious groups is considerably greater: from $2,812 to $6,375 for white Protestants, from $2,819 to $5,727 for Roman Catholics, and from $2,609 to $8,041 for Jews. On this basis, the conclusion suggested by Donald Bogue—that education is a much more potent factor than religious preference in determining the income level of households—seems fully justified.[25]

SUMMARY

The availability of hitherto withheld tabulations from the 1957 Current Population Survey, in which a question on religion was asked, provides a unique opportunity to examine a nationwide sample of the U.S. population for the relation between religion and socioeconomic status, as measured by education, occupation, and income. Earlier published data had already demonstrated the significant differentials in the distribution of the population by religion among the various regions of the United States and between urban and rural places of residence. This differential residential pattern could in itself account for significant variations in socioeconomic status, since economic opportunities vary considerably by place of residence. Urban residence in itself may stimulate proportionately more persons to obtain a higher education; it may also serve as a major attraction to persons from other places who have achieved high education. Recognizing both the interrelation between place of residence and socioeconomic status, the Bureau of the Census attempted, in a limited fashion, to control for these factors in several of the tabulations. Unfortunately, in the absence of the detailed data themselves, further manipulation of the materials was not feasible in this analysis.

Over-all, without controls, the data show significant differences among the religious groups with respect to socioeconomic status. Judged by median income, median education, and the proportion of white-collar workers, Jews occupied considerably higher status positions than did either Protestants or Catholics. The relative position of Protestants varies, depending on the measure used but, generally, white Protestants occupied a second place among the three religious groups. The heterogeneity of the Protestant group makes any conclusion about it difficult. Previous research has demonstrated that within the Protestant group there is a significant range of variation and that in many respects the socioeconomic characteristics of Episcopalians and Presbyterians closely resemble those of Jews.

Controlling for place of residence by restricting the analysis to the urban population and controlling for education as well in examining the occupation data, and for occupation and education separately in controlling for the income data, suggest that, when these controls are introduced, there is a considerable narrowing of the differentials in socioeconomic status among the three religious groups. In fact, for several subcategories of each of the groups, the income differentials virtually disappear, as do some of the striking differentials in occupational composition. These controls suggest that many of the differences among the three religious groups to which the crude data point are in fact a function of the differential educational achievement and/or oc-

[25] *Ibid.*, p. 708.

cupational composition of the members of these groups, as well as a result of their differential concentration in urban and rural places. With these factors controlled, although admittedly in a somewhat crude fashion, the conclusion seems warranted that both education and occupation play a much more crucial role than does religion itself in influencing the income levels of members of the three religious groups.

The differences in education were originally a function of both the value placed on education itself and on the recognition given to the important role that education plays as a mechanism for social and occupational mobility. The high educational achievement of Jews is understandable in these terms. But high educational achievement in itself is not fully adequate to account for the high concentration of Jews in white-collar positions. Especially among older generations of Jews, for example, a disproportionate number are in high white-collar positions even among those with low educational achievement.[26] These persons, most of whom were immigrants, continued the tradition, developed by their forebears in Europe under restrictions of occupational choice, of gaining a livelihood through engaging in trade. On the American scene, their drive to achieve middle-class status coupled with the reduced need for farmers and blue-collar workers, accentuated their entry into the occupational strata represented by the managers and proprietors of retail, wholesale, and manufacturing establishments.

It seems reasonable to assume that, as the over-all educational level of the general population rises, with a consequent decline in education differentials among members of the three religious groups and, as discriminatory restrictions on occupational choice weaken further, there will be a corresponding decline in occupational differentials. In turn, this should lead to greater homogeneity among the three religious groups with respect to income levels. The fact that the data for 1957 demonstrate relationships among religious groupings, education, occupation, income, and residential distribution suggests the importance of obtaining information on religion as a part of census and other social surveys.

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[26] Goldstein and Goldscheider, *op. cit.*, pp. 95–100.

Income and Religious Affiliation: A Regression Analysis[1]

Galen L. Gockel

ABSTRACT

Apparent differences in reported family income among thirteen religious groups can largely be explained by differences in the composition of these groups. This paper reports the results of regression problems in which family income is the dependent variable while education, occupation, religion, race, region, and size of place are the independent or predictor variables. By assigning to all thirteen religious groups the grand means on educational and occupational attainment, plus the national distributions of race, region, and size of place, it was possible to eliminate the effect of these characteristics on the mean family income of the religious groups. The data refer to male, fully employed heads of 7,518 households in a 1962 national sample survey.

This paper represents a convergence of two streams of sociological interest. First, the research reported here can be viewed as a continuation of the inquiry into social stratification among religious groups in the United States. Second, we shall be establishing continuity with a more recent and general interest in exploring the interrelationships among such socioeconomic variables as income, education, and occupational attainment.

The weight of evidence clearly indicates that religious groups in this country differ in class composition; further, there is virtual unanimity regarding the rank of religious bodies on any measure of socioeconomic status which might be employed. During the past generation, data from national public opinion surveys and community studies agree that the constituents of some religious bodies have higher social status than other religious groups.

National sample survey data are available from as early as 1939–40. During that period, the American Institute of Public Opinion and the Office of Public Opinion Research conducted four studies yielding about 14,000 cases. Data on religious affiliation, economic status, education, and region of the country were tabulated and presented by Cantril during World War II.[2] He observes that his data document the "commonly known facts" that Catholics are poorer and less well educated than Protestants. The data are convincing, but only

[1] I wish to cite the guidance of Robert W. Hodge, without whose direction this analysis could not have been initiated or completed. This research was conducted while I was a fellow in the Training Program in Survey Research, administered jointly by the National Opinion Research Center and the Division of Social Sciences of the University of Chicago, under a grant from the National Institute of Mental Health. The active participation of Mr. Frank Bamberger, then of the NORC data processing staff, assured prompt and accurate completion of the regression problems reported in this paper. The support of Peter H. Rossi was essential to this analysis and is gratefully acknowledged.

[2] Hadley Cantril, "Educational and Economic Composition of Religious Groups: An Analysis of Poll Data," *American Journal of Sociology*, XLVIII (March, 1943), 574–79. Unfortunately, Cantril's variables pose special problems which vitiate the otherwise productive use to which his data could be put. The religious groups he identifies are "Protestant," "Catholic" and "Nonmember." As a result, the only meaningful comparison which can be made is a gross Protestant-Catholic one; no Protestant denominations were identified, and Jews were included in the "Nonmember" category. Further, no data on income were obtained. Although the author does not describe how the "economic status" variable was constructed, Pope, in an article cited below, reports that the respondents were rated and assigned to class categories by the interviewer, who applied criteria such as dress, furnishings, neighborhood, occupation, etc. Finally, it would have been of additional help if Cantril had cross-classified the religious groups simultaneously by geographic region, education, and economic status, instead of by only two of these three variables at a time.

if one focuses attention on those respondents living outside the South.

Important for our purposes here is Cantril's observation that Protestants score higher than Catholics on "economic status" as well as on education. One may well ask whether the income advantage of the Protestants does not simply stem from the fact that they have, on the whole, more education. Clearly, income can be viewed as a payoff for educational attainment, and any subgroup displaying higher education can be expected, for this reason alone, to display higher income. Fortunately, Cantril presents actual frequencies stemming from a cross-classification of religious groups by education and economic status (income). We can thus compare the income levels of the two religious groups within groups homogeneous on education attainment. The original zero-order association reveals a 6.4 per cent advantage for the Protestants—66.8 per cent of the Protestants were classified as upper or middle income while 60.4 per cent of the Catholics were so classified. This difference is reduced by about one-half within each of the educational strata, indicating that a Protestant-Catholic difference in education explains a portion of the difference in income between these two groups.

We have cited the Cantril paper in such detail not only for its substantive implications but also as an excellent example of early empirical investigation into the relationship between class and religious affiliation. Although the data were national in scope and based on many cases, the analysis was brief and descriptive and does not explicitly view income differences among religious groups as potentially stemming from differences among them on other variables which are themselves associated with income.

National data collected five years later (1945–46) by the Office of Public Opinion Research have been analyzed by Pope.[3] He subdivided the sample into eight religious groups—Catholics, Jews, and six Protestant denominations. In three separate zero-order tables he distributed the eight groups according to occupation, education, and "class composition" (the latter term derived from an interviewer rating). Inspection of these three tables reveals that on each measure, two groups are clearly identified. The Congregationalists, Episcopalians, Presbyterians, and Jews occupy the four top positions on each measure of socioeconomic status, and the Methodists, Lutherans, Catholics, and Baptists comprise the lower half of the ranking.

Again, Pope's analysis can be viewed as entirely descriptive. While he had access to data on three measures of social status, he was content to document the fact that when religious groups are ranked according to one measure of status (say, occupation) they are ranked on other measures as well. The notion that these various measures might be interrelated in some way had not yet become an issue.

Lazerwitz carried forward the Cantril-Pope tradition in his analysis of more recent (1957–58) national survey data.[4] Having ranked religious bodies on education, Lazerwitz comments that "this interesting rank of the religious groups recurs on occupation of family head." Finally, he notes that the ranking of the groups on education and occupation "is further borne out" by their ranking on income. An inspection of his tables, however, reveals that (*a*) Catholics and Protestants have virtually identical educational attainments, but that (*b*) more Catholics (52 per cent) than Protestants (45 per cent) earn $5,000 or more in total

[3] Liston Pope, "Religion and the Class Structure," *Annals of the American Academy of Political and Social Science*, No. 256 (March, 1948), pp. 84–91.

[4] Bernard Lazerwitz, "Religion and the Social Structure in the United States," In Louis Schneider (ed.), *Religion, Culture and Society* (New York: John Wiley & Sons, 1964), pp. 426–39. While Lazerwitz considers various aspects of religious behavior, such as church attendance, we limit our attention to his Tables 2, 3, and 4, which contain the data relating to social class. He identified seven religious bodies, one of which, the Baptist, is divided into white and Negro subgroups.

family income. This gross comparison at least raises the possibility that Catholics use their education to greater financial advantage than do Protestants.

Bogue also used national survey data collected in the 1950's.[5] His description of the educational, occupational, and income levels of the various religious groups closely approximates the findings of Lazerwitz. The author goes on, however, by asking questions of the following kind:

> To what extent are the occupational differences noted above due to religious affiliation, and to what extent are they due simply to the fact, noted in the preceding section, that some religious groups have had more education than others? For example, are Baptist household heads concentrated in the blue-collar occupational group because they are Baptists, or because a preference for the Baptist religion is more common among socio-economic groups whose members have not had an opportunity to complete as many years of schooling as the rest of the population?

A detailed description of Bogue's data is not warranted here, especially in view of his warning that specific comparisons suffer from the small case bases which are produced when religious groups are simultaneously cross-classified by two socioeconomic variables, each of which has a large number of categories. Yet the general conclusion is clear: The income differences among religious groups are sharply reduced when the factors of education or occupation are introduced. The data reported in this paper can be viewed as an extension of the interest expressed by Bogue, while our regression approach will obviate the problem of small case bases in many-celled tables.

The most recent use of national sample survey data to illuminate the status differences among religious groups has been the work of Glenn and Hyland.[6] Their data, drawn from national surveys conducted from 1943 to 1965, permit an analysis of trends, and their primary interest is in white Protestant-Catholic differences. They observe that Catholics have improved their position substantially over this two-decade period and attribute this to the concentration of Catholics, in the urban North, "where earnings, occupational distributions, educational opportunities, and rates of upward mobility are more favorable than in the typical home communities of Protestants."

The large pooled sample size of the studies available to Glenn and Hyland permits an analysis of only younger white residents of non-southern, large metropolitan areas. In these areas, where Protestants and Catholics are found in equal numbers, the former showed similar but slightly higher levels of socioeconomic status. The Protestant advantage was especially pronounced at the uppermost reaches of the income, education, and occupational variables.

The authors conclude that it is not "religious factors" but areal distribution that explain differences in worldly success between the two major religious groups.

Regarding the issue of interrelationships among the various measures of social status, Glenn and Hyland do view levels of income as stemming from educational and occupational factors. They concede that Catholic underrepresentation in the highest income group may be explained by their "lower representation at the highest educational levels and, in turn, in professional, semi-professional, business, and executive occupations."

So far, we have reviewed some of the major issues reflected in five works which were secondary analyses of national surveys and which dealt specifically with comparisons among religious groups. Interest has recently developed in the more general issue of the relationships among various measures of socioeconomic status.

Utilizing 1950 Census data, Glick and Miller presented a number of tables com-

[5] Donald J. Bogue, *The Population of the United States* (New York: Free Press, 1959), chap. xxiii. Bogue's data were collected by NORC in 1953 and 1955 in two studies designed and analyzed by Jacob J. Feldman, senior study director.

[6] Norval Glenn and Ruth Hyland, "Religious Preference and Worldly Success: Some Evidence from National Surveys," *American Sociological Review*, XXXII (February, 1967), 73–85.

paring the income of groups varying in the extent of education they had received.[7] They document, for example, the extent to which the average annual income increases with increased education and the degree to which total lifetime income is positively related to education. Further, they show that the increment in annual income received per year of education increases with increased education. That is, the positive association between income and education becomes stronger toward the higher end of the educational scale.

In an article presented more for methodological than substantive purposes, Duncan investigated the way in which occupation mediates the relationship between education and level of income.[8] He concludes that "these data suggest that an educational advantage is translated into an income advantage primarily, though not exclusively, by pursuing an occupation in which the prevailing income level is comparatively high."

The studies cited thus far represent two streams of inquiry which, except in the early attempt by Bogue, have not yet merged. On the one hand, comparisons among various religious bodies have been made, using national sample survey data and typically employing one socioeconomic measure at a time. On the other, there has been a recent interest in exploring the interrelationships among socioeconomic variables, but, until the research reported in this paper, this inquiry has not been applied to the various religious subgroups within American society.

NATURE OF SAMPLE AND DATA

The data reported here were collected in 1962 from a large sample of the United States population. The study yielded interviews in almost 12,000 households.[9] The findings which follow pertain to a national cross-section of 7,518 fully employed men who were heads of households in 1962. From each respondent's questionnaire, data were extracted on the following variables: family income, occupation, education, race, census region, a measure of city size, and religious denomination. These variables, together with a description of the form in which they were collected and used in the analysis, are described below.

Family income.—Each individual reported his "total family income" by selecting one of eleven class intervals. The midpoint of each interval was used to score the respondent on income. The lower openended category (under $1,000) was scored $500 and the upper category ($15,000 or over) was scored $17,000.[10]

Occupation.—In preparing data on occupation for a regression analysis, it must be assumed that various occupational categories can be placed on the scale of a quantitative variable. We have chosen to use the quantification of occupational socioeconomic status as developed by Duncan and have applied it to the census major occupation groups.[11]

[7] Paul C. Glick and H. M. Miller, "Educational Level and Potential Income," *American Sociological Review*, XXI (June, 1956), 307–12.

[8] Otis Dudley Duncan, "Occupational Components of Educational Differences in Income," *Journal of the American Statistical Association*, LVI (December, 1961), 783–92.

[9] The original study from which these data are taken was a survey of the adult education activities of Americans. It was conducted by the National Opinion Research Center under a grant from the Carnegie Corporation and is reported in John Johnstone and Ramon Rivera, *Volunteers for Learning* (Chicago: Aldine Publishing Co., 1965).

[10] It should be noted that total family income was available to us—income possibly generated by more than one wage earner. Since we are not trying to make estimates of personal income, and since styles of life are more accurately reflected by total available income, this concept is appropriate. There is a potential difficulty, however, if the religious groups vary in the proportion of wives or other adults who are employed. To put it another way, it is conceivable that differences in income among religious groups may stem from differences in the incidence of multiple earning among them. We are assuming that any differences in this regard among denominations account minimally for differences among them in total family income.

[11] Otis Dudley Duncan, "A Socio-economic Index for All Occupations," in A. J. Riess *et al.*, *Occupations and Social Status* (New York: Free Press, 1961). Briefly, the socioeconomic scores are those predicted for an occupation from a regression equa-

Education.—Formal education had been coded in school years completed. Some of the codes were in specific years (e.g., eight years), in which case the exact number of years was assigned to the respondent. Other codes were in intervals (e.g., nine–eleven years), for which the midpoint was used. A score of 17 was assigned if the respondent reported training beyond the bachelor's degree. The mean for all respondents analyzed here was 11.2 years of education.

Race and region.—A fourfold classification simultaneously identified respondents according to their race (white versus non-white) and census region (South versus non-South, or "North"). Negroes comprised over 94 per cent of the non-whites; the remainder had been classified as Orientals. Approximately 65 per cent of the respondents were thus identified as northern whites, 5 per cent at northern non-whites, 25 per cent as southern whites, and 5 per cent as southern non-whites.

Size of place.—The stratification system used in selecting NORC's Primary Sampling Units provided a rough measure of the variable we shall call "size of place." Respondents were identified as living in either (*a*) one of the ten largest Standard Metropolitan Statistical Areas ("large SMSA's"); (*b*) one of the other SMSA's ("small SMSA's"); (*c*) a county not in an SMSA which contained a city of 10,000 or more ("urban counties"); or (*d*) in a county which contained no city as large as 10,000 ("rural counties").

FINDINGS

The basic data on three measures of socioeconomic status are presented in Table 1, where thirteen religious groups are ranked on mean family income. The rank order comes as no surprise, given past investigations.

We conceive of these variables as connected in a temporal, causal sequence. Income can be viewed as the product of a given occupational level, and occupation itself is a "payoff" for a given investment in education. The over-all consistency in the rankings of the groups from one measure to the next raises questions as to the extent to which intergroup differences in income are simply manifestations of differences among them in occupational and educational attainment. This is the central question to which we shall return.

An inspection of Table 1 identifies a few departures from consistent rankings. To consider a specific comparison, although the Jews score slightly lower than the Episcopalians on mean education, they are over three points higher than the latter group on the occupational scale and display a mean income which is about $650 higher. One comes away from a simple comparison like this with the feeling that Jews "use" their education to a greater advantage than do Episcopalians. Similarly, take the groups ranking third through sixth on mean income—the Congregational, "All Other Religions," Presbyterian, and Mormon groups. Their educational attainments are comparable, as are the levels of occupational attainment. Yet the range of mean incomes of these four groups is almost $1,900, with a gap of roughly $1,050 separating the Congregationalists from the Presbyterians. It is clear, on an intuitive basis, that Congregationalists receive a greater return on their educational investment and occupational attainment than do the other three groups.

Another perspective on these data is given

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tion in which the independent variables are the age-standardized proportions in the occupation earning $3,500 or more in 1949, and having attained at least a high school education. The regression weights were obtained by a multiple regression analysis involving the prestige ratings of forty-five occupations included in the 1947 National Opinion Research Center study of occupational prestige. A multiple correlation of .91 resulted when the percentage of "excellent" plus "good" ratings were regressed on the measures of income and education noted above. For the full description of the socioeconomic score and its derivation, see Duncan, "A Socio-economic Index." We have used the scores presented in Duncan's Table VII-4 for the broad occupational groupings, since detailed occupation was not coded in the original survey from which these data are drawn. In our analysis, the mean for all respondents was 36.1.

in Table 2, which shows income differences; it is derived directly from Table 1. This permits an immediate assessment of the differences in mean income between any two religious groups, as well as between any group and the grand mean. For example, Table 2 shows that the mean income reported by all Presbyterians was about $893 more than that for Lutherans and about $1,072 above the mean for all respondents.

These zero-order differences set the stage for an analysis which is more precise than a cursory comparison of means. Our central concern is with an accurate estimation of the extent to which income differences among religious groups can be accounted for by differences in the compositon of each with regard to education and occupation. Having viewed the preliminary data in Table 1, we turn to regression techniques to answer our major questions. The first portion of the analysis is based on equation (1).

$$\hat{Y} = a + bE + cO + \sum_{i=1}^{12} d_i R_i , \quad (1)$$

where Y = the respondent's income in dollars, E = the respondent's education, O = the respondent's occupational score, and the R_i's are dummy variables which take the value 1 if the respondent is of the ith religious group, and 0 otherwise.[12] The con-

TABLE 1

MEAN VALUES OF FAMILY INCOME, OCCUPATIONAL SOCIOECONOMIC STATUS, AND EDUCATION WITHIN THIRTEEN RELIGIOUS GROUPS (MALE, FULLY EMPLOYED HEADS OF HOUSEHOLDS IN 1962 NATIONAL SAMPLE)

Religious Group*	N	Family Income	Occupational SES	Education
Entire sample:	7,518	$6,941	36.1	11.2
Jewish	242	9,839	53.4	13.3
Episcopalian	199	9,173	50.0	13.5
Congregationalist	127	9,067	44.7	12.9
Presbyterian	435	8,013	43.2	12.5
All other religions	101	7,847	43.3	12.6
Mormon	101	7,188	41.0	12.0
Methodist	1,031	7,185	37.3	11.6
Catholic	1,936	7,132	35.9	11.1
Lutheran	680	7,120	35.6	11.1
Protestant, unspecified	333	7,038	34.2	11.2
No religion	253	6,579	33.8	10.5
Protestant, other	367	5,831	30.2	10.1
Baptist	1,713	5,612	30.5	10.1

* Definitions of some of the groups identified in this paper are as follows:

"Congregationalist" includes Evangelical and Reformed, plus those respondents who gave the postmerger name of these denominations, i.e., United Church of Christ.

"All Other Religions" includes Christian Science, Unitarian-Universalist, plus all other responses which could not be included in one of the other twelve categories. Preliminary tabulations revealed that about one-sixth of this group had been coded "Oriental" in race; presumably they reported an affiliation with one of the Eastern religions.

"Protestant, Other" includes specific Protestant denominations or sects which could not be included in one of the other Protestant categories.

"Protestant, Unspecified" includes respondents who answered "Protestant" to the question, but either failed to elaborate or were not asked their specific denomination by the interviewer.

[12] For a discussion of dummy variables, see D. B. Suits, "Use of Dummy Variables in Regression Equations," *Journal of the American Statistical Association*, LII (December, 1957), 548–51. In our case, there are thirteen religious groups, but only twelve dummy variables are required to exhaust the information. If a person is 0 on $R_1, R_2 \ldots R_4 \ldots R_{12}$, he would have to be 1 on R_{13}, where R_{13} is a dummy variable describing membership in the

TABLE 2

DIFFERENCE IN ACTUAL MEAN FAMILY INCOME BETWEEN PAIRS OF RELIGIOUS GROUPS, AND DIFFERENCE BETWEEN EACH GROUP MEAN AND GRAND MEAN (MEAN OF GROUP IN STUB LESS MEAN OF GROUP IN CAPTION)

Religious Groups	Jewish	Episcopalian	Congregationalist	Presbyterian	All Other	Mormon	Methodist	Catholic	Lutheran	Unspecified Protestant	No religion	Other Protestant	Baptist	All
Jewish	......	666	772	1,826	1,992	2,651	2,654	2,707	2,719	2,801	3,260	4,008	4,227	2,898
Episcopalian	......	......	106	1,160	1,326	1,985	1,988	2,041	2,053	2,135	2,594	3,342	3,561	2,232
Congregationalist	......	......	......	1,054	1,220	1,879	1,882	1,935	1,947	2,029	2,488	3,236	3,455	2,126
Presbyterian	......	......	......	......	166	825	828	881	893	975	1,434	2,182	2,401	1,072
All other religions	......	......	......	......	......	659	622	715	727	809	1,268	2,016	2,235	906
Mormon	......	......	......	......	......	......	3	56	68	150	609	1,357	1,576	247
Methodist	......	......	......	......	......	......	......	53	65	147	606	1,354	1,573	244
Catholic	......	......	......	......	......	......	......	......	12	94	553	1,301	1,520	191
Lutheran	......	......	......	......	......	......	......	......	......	82	541	1,289	1,508	179
Unspecified Protestant	......	......	......	......	......	......	......	......	......	......	459	1,207	1,426	97
No religion	......	......	......	......	......	......	......	......	......	......	......	748	967	−362
Other Protestant	......	......	......	......	......	......	......	......	......	......	......	......	219	−1,110
Baptist	......	......	......	......	......	......	......	......	......	......	......	......	......	−1,329

stant is represented by the first term, while the other lowercase letters represent the net regression coefficients (in raw score form, i.e., dollars) associated with each of the respective variables. Solution of the problem yields the following coefficients: a = \$2,331.01; b = \$268.84; c = \$49.48. In addition, each d_i has a value associated with membership in one of the thirteen religious groups. The individual values of d_i have little interest standing alone; it is the differences between specific pairs of religious groups, and between each group and the grand mean, which are of interest and which are the central focus of the analysis which ensues.

An interpretation of these coefficients may be helpful at this point. In predicting income from equation (1), we can assign \$2,331 to each respondent regardless of occupation, education, or religious affiliation. Net of occupation and religious affiliation, each respondent receives an increment of almost \$269 for each year of education completed. Net of the other variables, each point on the occupational socioeconomic status scale yields over \$49. Finally, there is an additional increment or decrement (d_i) which is associated with membership in a specific religious group.[13]

omitted group, "All Other Religions." Explicitly introducing R_{13} would make the normal equations for solving the regression equation linearly dependent. In the dummy variable format in which we have written the regression equation, the coefficients of the R_i's come out as deviations from the omitted group (which is implicitly represented by the estimate of the constant term a).

An alternative but logically equivalent mode of treating the data—multiple classification analysis—yields a net coefficient for each group. In multiple classification analysis, the coefficient of each group is expressed as a deviation from the grand mean rather than from the omitted category.

[13] It may be noted here that the addition of religion to the regression equation adds little to the prediction of income that has not already been accounted for by occupation and education. An equation limited to these two variables as predictors yields a correlation coefficient $R_{Y(OE)}$ = .496 almost as great as the correlation associated with eq. (1), which contains religion ($R_{Y(OER)}$ = .516).

We wish to assess the differences in mean income which remain after the effects of of occupational and educational levels have been accounted for. The procedure for doing so simply involves comparing the d_i's for the thirteen groups which were generated by the solution of equation (1).[14] These data are contained in Table 3. The entries are the differences "due to religion," after variations in occupation and education are accounted for, between all possible pairs of religious groups. To consider the Presbyterians as an example, their adjusted mean income is about \$1,116 below that of Jews, \$134 above Lutherans, and \$357 above the actual grand mean for all respondents. The reader will recall from Table 2, however, that the difference actually observed between Jews and Presbyterians was \$1,826. The reduction from an actual difference of \$1,826 to an adjusted difference of \$1,116 reflects the extent to which Jewish-Presbyterian income differences were accounted for by differences in the educational and occupational attainments in the two groups. To put it another way, a difference of \$1,116 would still have been observed if both groups had similar occupational and educational levels.

If education and occupation accounted for all the differences between groups in income, each entry in Table 3 would be zero. Although the entries do depart from zero (that is, although there are still effects "due to religion") they are substantial only when the two groups being compared are located at or near the ends of the stratification ladder.

The final column of Table 3 indicates the extent to which the income of each religious group departs from the national average after it has been adjusted for the effects of the group's educational and occupational

[14] Comparing the d_i's for two groups is arithmetically equivalent to comparing their means adjusted for education and occupation. The Episcopalian-Baptist difference of \$1,695 (Table 3) is generated not only by comparing their d_i's (740 − [− 955]) but also by comparing their mean incomes adjusted by d_i (\$7,859 − \$6,164).

TABLE 3

DIFFERENCE IN FAMILY INCOME (NET OF EFFECTS OF EDUCATION AND OCCUPATION) BETWEEN PAIRS OF RELIGIOUS GROUPS, AND DIFFERENCE BETWEEN EACH GROUP MEAN (NET OF EFFECTS OF EDUCATION AND OCCUPATION) AND GRAND MEAN (MEAN OF GROUP IN STUB LESS MEAN OF GROUP IN CAPTION)

Religious Group	Jewish	Episcopalian	Congregationalist	Presbyterian	All Other	Mormon	Methodist	Catholic	Lutheran	Unspecified Protestant	No religion	Other Protestant	Baptist	All
Jewish	......	555	246	1,116	1,295	1,703	1,406	1,255	1,250	1,303	1,548	2,011	2,250	1,473
Episcopalian	......	......	−309	561	740	1,148	851	700	695	748	993	1,456	1,695	918
Congregationalist	......	......	......	870	1,049	1,457	1,160	1,009	1,004	1,057	1,302	1,765	2,004	1,227
Presbyterian	......	......	......	......	179	587	290	139	134	187	432	895	1,134	357
All other religions	......	......	......	......	......	408	111	− 40	− 45	8	253	716	955	178
Mormon	......	......	......	......	......	......	−297	−448	−453	−400	−155	308	547	−230
Methodist	......	......	......	......	......	......	......	−151	−156	−103	142	605	844	67
Catholic	......	......	......	......	......	......	......	......	− 5	48	293	756	995	218
Lutheran	......	......	......	......	......	......	......	......	......	53	298	761	1,000	223
Unspecified Protestant	......	......	......	......	......	......	......	......	......	......	245	708	947	170
No religion	......	......	......	......	......	......	......	......	......	......	......	463	702	− 75
Other Protestant	......	......	......	......	......	......	......	......	......	......	......	......	239	−538
Baptist	......	......	......	......	......	......	......	......	......	......	......	......	......	−777

composition. Only five of the thirteen groups display an adjusted mean which varies more than $500 from the grand mean. Parenthetically, it may be noted that our speculation regarding the use to which Congregationalists put their educational and occupational investments is confirmed in Table 3. Earlier we noted that Congregationalists reported a substantially higher income than the "All Other," Presbyterian, and Mormon groups, even though all four had obtained roughly the same amount of education and had scored equally high on the measure of occupational socioeconomic status. The d_i associated with being a Congregationalist was the second highest of those generated by equation (1) and yielded an adjusted income over $1,200 higher than the national mean.

INTERACTION OF RELIGION WITH EDUCATION AND OCCUPATION

Thus far we have spoken of the "use" which members of various religious groups make of their educational investment and occupational attainment. We have not yet attempted to treat the separate effects of these two variables on the income of each group. Values derived by the solution of regression equation (1) indicated that in general, among all respondents, each year of education yields almost $269 in family income annually; this was the value of the net regression coefficient associated with the education variable. The coefficient for occupational attainment was over $49 in the sample as a whole.

Whether, and to what extent, these coefficients vary from one denomination to the other is, at present, an open question to which we now turn. A multiple regression equation was used which permits religious affiliation to interact with these two variables. It has the form:

$$\hat{Y} = a + bE + cO + \sum_{i=1}^{12} d_i R_i + \sum_{i=1}^{12} e_i R_i E + \sum_{i=1}^{12} f_i R_i O \qquad (2)$$

The first four terms have interpretations identical to the first four terms in equation (1), but their numerical estimates will have different values from those in equation (1) because the solution now involves additional terms.

The coefficients of the R_iE's adjust the dollar increment received by a person for each year of schooling completed according to the religious group to which he belongs. This interaction term allows individuals of different religious bodies to receive different "rates of return" on their educational investments. Equation (1), unlike equation (2), assumed that the same relation between education and income held, regardless of religious affiliation. In a similar way, the R_iO's adjust the dollar increment received by a respondent for each point he has attained on the occupational scale according to the denomination to which he belongs.

If we view equation (2) from another perspective, the first three coefficients (a, b, and c) are the same for all respondents; the remaining coefficients (the d_i's, e_i's and f_i's) vary from one religious body to the next. Thus the equation specific to each religious group may be obtained by combining coefficients a and d_i to obtain the constant for that group; adding coefficients b and e_i to obtain the coefficient of education for that group; and summing c and f_i to derive the coefficient of occupation for that group.

An example may help. When equation (2) is solved, the first three coefficients have the following values for all respondents: $a = \$1,589.99$; $b = \$287.02$; $c = \$61.35$. For Catholics, as an example, the coefficients are $d_i = \$724.58$; $e_i = -\$6.92$; $f_i = -\$13.77$. Substituting in equation (2), $\hat{Y} = \$1,589.99 + (\$287.02)E + (\$61.35)O + \$724.58 + (-\$6.92)E + (-\$13.77)O$. Combining coefficients in the manner indicated in the previous paragraph gives, for Catholics, $\hat{Y} = \$2,314.57 + (\$280.10)E + (\$47.58)O$. Thus, the equation gives the regression of income on occupation and education within each denomination. Table 4 contains the coeffi-

cients, derived in the same way as our Catholic example, for the thirteen religious groups.[15]

The table arrays the groups in order of the magnitude of the coefficient of education. In general, those groups with higher educational achievements receive a higher increment of income per year of schooling completed. Thus the Episcopalians, Congregationalists, and Jews receive higher than average returns per year of schooling completed. This generalization is by no means perfect, however, since the "No Religion" and "Unspecified Protestant" groups display relatively high coefficients of about $370; further, both the Presbyterians and Lutherans have coefficients lower than we would expect if it were true that the higher the level of education, the higher the payoff per year of education.

A note is in order relative to the position of Catholics. Table 1 revealed that Catholics were quite close to the national mean on all three measures of status—income, education, and occupational socioeconomic status. These data simply corroborate the findings on this point in the studies by Bogue and Lazerwitz reported above. National data thus make a heresy of what Greeley has described as an "article of faith among sociologists that Catholics are concentrated in the lower economic classes."[16]

Table 4 further reveals that Catholics fare no worse than average in the financial "use" to which they put their educational

TABLE 4

REGRESSION OF INCOME ON OCCUPATION AND EDUCATION WITHIN RELIGIOUS GROUPS

Religious Group	N	Constant	Coefficient of Education	Coefficient of Occupation	Rank of Protestants According to Social Liberalism of Clergy*	Rank of Protestants According to Doctrinal Liberalism of Members†
Episcopalian	199	−$3,088	$778	$35	2	2
Congregationalist	127	984	388	68	1	1
No religion	253	508	373	63	....	....
Protestant, unspecified	333	1,558	367	39	....	....
Jewish	242	1,946	365	57	....	....
All other religions	101	1,590	287	61	....	....
Catholic	1,936	2,315	280	48	....	....
Protestant, other	367	2,186	269	30	....	....
Presbyterian	435	2,219	250	61	4	4
Methodist	1,031	2,185	241	59	3	3
Baptist	1,713	1,856	237	44	6	5
Lutheran	680	3,909	148	44	5	6
Mormon	101	1,590	142	37	....	....

* SOURCE.—Philip E. Hammond and Robert E. Mitchell, "Segmentation of Radicalism: The Case of the Protestant Campus Minister," *American Journal of Sociology*, LXXI (September, 1965), 133–43. The rank of the denomination is based on *parish* ministers' attitudes toward the United Nations and the World Council of Churches. The denomination whose clergy most "strongly approved" of these organizations is given rank 1. To make our classification congruent with that of Hammond and Mitchell, we have merged their data on more specific denominations, for example, Baptists (American and Southern). We excluded their data on the Disciples, since this group was not identified in our coding. Finally, the reader should note that their term "United Church" is identical to our premerger term "Congregationalist."

† SOURCE.—Charles Y. Glock and Rodney Stark, *Religion and Society in Tension* (Chicago: Rand McNally & Co., 1965), Table 5-7. The rank of the denomination is based on members' responses to the idea that "holding the Bible to be God's truth" is a requirement for salvation. The denomination whose members least frequently held that this belief is "absolutely necessary" for salvation is given rank 1. As above, it was necessary to combine the authors' data for denominational subgroups, such as the American and Southern Baptists. On other measures which would have served just as well as measures of liberalism-fundamentalism, the Episcopalian and Methodist positions were reversed. The data were elicited by a self-administered questionnaire completed by church members in four counties in the San Francisco area.

[15] It can be shown that, although we derived these coefficients from a single equation involving interaction terms, the same data would have been generated if regressions on education and occupation had been conducted within the religious groups separately.

[16] Andrew M. Greeley, "The Protestant Ethic: Time for a Moratorium," *Sociological Analysis*, XXV (Spring, 1964), 20–33.

and occupational attainments. Indeed, the denominations containing the bulk of Protestants rank below the Catholics in the magnitude of their coefficients of education.

The final two columns of Table 4 provide another standard of comparison among the Protestant denominations. There is a strong similarity between the two rankings—one of which taps the social attitudes of the clergy, while the other is a measure of the theological orientation of the constituents. Further, these rankings approximate the order of the denominations according to mean income (Table 1) and according to the coefficients of education.

At first, the greater liberalism of the higher income groups may seem anomalous, since there is evidence suggesting that such denominations as Episcopalian and Congregationalist have tended to vote Republican and to oppose governmental programs in social welfare to a greater degree than other religious groups.[17] It may be helpful to consider the two types of liberals identified by Smith about two decades ago using national sample data:

> Liberals, as defined by non-economic or interclass values (e.g., endorsement of civil liberties and internationalism and opposition to prohibition), were found to be better informed and educated than the "conservatives." On the other hand, liberals, as defined by approval of certain politico-economic proposals (e.g., more power for labor, the government ownership of banks, increased unemployment compensation), tended to be poorer informed and educated than the conservatives.[18]

More recently, Lipset, for whom commitment to Western political democracy is a major concern, has investigated the relationship between the lower classes and democracy. In his essay on working class authoritarianism, he analyzes or cites cross-national data to indicate a relatively low commitment to democratic or civil libertarian ideals among those lower on the occupational scale.[19]

Table 4 suggests that religious bodies whose constituents are more liberal theologically have clergy who are more liberal in the "non-economic" areas as measured by Hammond and Mitchell (see Table 4).

RACE, REGION, AND SIZE OF PLACE

Thus far we have considered only two predictor variables—respondent's income and occupation. We have stated that income differences among denominations which remain after these variables have been taken into account are differences "due to religion." However, there are other differences among religious groups which may further account for the apparent intergroup variation in reported income.

For example, it is well known that Negroes are traditionally affiliated with the Baptist and Methodist denominations. To the extent that these two groups contain a disproportionate percentage of Negroes (whose average income is low), their incomes will be depressed. Similarly, denominations overrepresented in the South or in non-urban areas can be expected to display incomes which are relatively low simply because incomes are generally lower in these areas. Further, denominations varying in their areal distribution will be differentially exposed to education of high quality. Although our only direct measure of education is quantitative—the highest grade completed—our introduction of race-region and size of place reflects variations in quality of schooling to some extent. In general, we would expect those groups which contain high proportions of constituents who are white, northern, or from metropolitan areas to have received an education that better equips them for the world of work than de-

[17] Wesley Allinsmith and Beverly Allinsmith, "Religious Affiliation and Politico-economic Attitude," *Public Opinion Quarterly*, XII (Fall, 1948), 377-89.

[18] G. H. Smith, "Liberalism and Level of Information," *Journal of Educational Psychology*, XXXIX February, 1948), 65-81.

[19] Seymour M. Lipset, *Political Man* (Garden City, N.Y.: Anchor Books, 1963), pp. 87-126.

nominations among whom these characteristics are less typical.

Because data on race, region, and size of place are available to us, we wish to take these variables into account in the same way that we have introduced education and occupation into our analysis of religious differences in income. Earlier in this paper we defined these variables and outlined the way in which they are utilized here. The regression equation which applies is directly analogous to equation (1):

$$\hat{Y} = a + bE + cO + \sum_{i=1}^{12} d_i R_i + \sum_{j=1}^{3} e_j A_j + \sum_{k=1}^{3} f_k S_k \qquad (3)$$

The first four terms are interpreted as in equation (1). In the fifth term, A_j is a variable which takes the value 1 if the respondent is included in the jth race-region category, and 0 otherwise; S_k takes the value 1 if the respondent resides in the kth size of place category, and 0 otherwise. Each of the two coefficients therefore represents an increment in dollar income accruing to respondents because of their inclusion in a specific race-region group (e_j) or specific size of place category (f_k) net of any increments stemming from other variables in the equation.[20]

Our task is to take all four variables (race-region here being considered as one variable) into account in explaining the variations in income among denominations. We shall equate the groups on education and occupation, using the grand means on these variables as before; but we shall also assign the national distributions of race-region and size of place to each of the thirteen religious groups. This will make the Jews, for example, less urban, less white, and less northern. At the other extreme, it will assign to the Baptists higher proportions of respondents who possess each of these three characteristics.

An example will add clarity before we turn to Table 5 for the complete picture. For a specific religious group, equation (3) yields

$$\begin{aligned}\hat{Y} &= \$508.75 + (\$244.63)(11.165) \\ &+ (\$46.39)(36.098) + d_i \\ &+ [(\$1{,}569.99)(.652) + (\$900.96)(.051) \\ &+ (\$1{,}208.89)(.245)] \\ &+ [(\$1{,}354.04)(.227) + (\$846.24)(.412) \\ &+ (\$397.01)(.180)] = \$508.75 \\ &+ \$2{,}731.29 + \$1{,}674.59 + d_i \\ &+ \$1{,}365.76 + \$727.48 = \$7{,}007.87 \\ &+ d_i\,.\end{aligned}$$

Thus once we equate the respondents on all the predictor variables considered here, we can allow each religious group to retain its own coefficient (d_i).

Table 5 is analogous to Tables 2 and 3. Here, however, we present differences in income net of all the predictor variables employed in equation (3). The story of this table is best learned by comparing its entries with comparable entries in Tables 2 and 3. Consider the difference between Episcopalians and Methodists, for example. The actual difference in their observed mean incomes was $1,988 (Table 2). Net of education and occupation, the Episcopalians held an advantage of $851 over the Methodists. Finally, when race-region and size of place are taken into account, the difference

[20] Again, to make the problem solvable, one subgroup of each variable was omitted from the problem. These coefficients are therefore expressed in terms of deviations from the omitted group and accurately represent differences among the groups. Because of their intrinsic interest we present the values of these coefficients:

Northern white (e_1)	= $1,570
Northern non-white (e_2)	= $901
Southern white (e_3)	= $1,209
Southern non-white	Omitted category
Large SMSA (f_1)	= $1,354
Small SMSA (f_2)	= $846
Urban county (f_3)	= $397
Rural county	Omitted category

TABLE 5

DIFFERENCE IN FAMILY INCOME (NET OF EFFECTS OF EDUCATION, OCCUPATION, RACE, REGION, AND CITY SIZE) BETWEEN PAIRS OF RELIGIOUS GROUPS, AND DIFFERENCE BETWEEN EACH GROUP MEAN (NET OF EFFECTS OF EDUCATION, OCCUPATION, RACE, REGION, AND CITY SIZE) AND GRAND MEAN (MEAN OF GROUP IN STUB LESS MEAN OF GROUP IN CAPTION)

Religious Group	Jewish	Episcopalian	Congregationalist	Presbyterian	All Other	Mormon	Methodist	Catholic	Lutheran	Unspecified Protestant	No Religion	Other Protestant	Baptist	All
Jewish	......	158	−131	653	904	1,452	680	1,075	804	849	1,015	1,329	1,410	970
Episcopalian	......	......	−289	495	746	1,294	522	917	646	691	857	1,171	1,252	812
Congregationalist	......	......	......	784	1,035	1,583	811	1,206	935	980	1,146	1,460	1,541	1,101
Presbyterian	......	......	......	......	251	799	27	422	151	196	362	676	757	317
All other religions	......	......	......	......	......	548	−224	171	−100	−55	111	425	506	66
Mormon	......	......	......	......	......	......	−772	−377	−648	−603	−437	−123	−42	−482
Methodist	......	......	......	......	......	......	......	395	124	169	335	649	730	290
Catholic	......	......	......	......	......	......	......	......	−271	−226	− 60	254	335	−105
Lutheran	......	......	......	......	......	......	......	......	......	45	211	525	606	166
Unspecified Protestant	......	......	......	......	......	......	......	......	......	......	166	480	561	121
No religion	......	......	......	......	......	......	......	......	......	......	......	314	395	− 45
Other Protestant	......	......	......	......	......	......	......	......	......	......	......	......	81	−359
Baptist	......	......	......	......	......	......	......	......	......	......	......	......	......	−440

between their d_i's (and thus of their adjusted mean incomes) shrinks still further to $522.

We interpret this to indicate that a substantial proportion of their apparent income advantage over Methodists stems from Episcopalians' higher levels of education and occupational attainment, plus higher proportions in three favored subgroups—whites, northerners, and residents of metropolitan areas. The final differences, $522, is the amount "due to religion" after the other predictor variables have been controlled.

The final column of Table 5 indicates the extent to which the income of each religious group departs from the national average after it has been adjusted for the effect of all predictor variables. Only three of the adjusted mean incomes vary more than $500 from the grand mean. It is of interest to note that the Congregationalists (United Church of Christ) now show the highest adjusted income. Although they are clearly a northern group, they are represented in towns and small metropolitan areas in greater proportion than other major northern groups. Thus, when they were assigned the national distribution on the size-of-place variable, their adjusted income was not as adversely affected as, say, the Jews and Episcopalians, who are distinctly more urban in residence.

Those groups which are most urban (Jewish, Catholic, and Mormon), most northern (Mormon, Catholic, and Congregationalist), and most predominantly white (all groups except Baptist, Methodist, "Other Protestant," and "No Religion") display an additional drop in their adjusted income when size of place, region, and race are taken into account.

The summary conclusion to be learned from Tables 2, 3, and 5 is that the variables considered in this paper go a long way toward explaining apparent differences in family income among the thirteen religious groups cited in this study.

DECOMPOSITION OF TOTAL VARIATION

It is possible to develop a gross over-all estimate of the individual net contributions made by each of our predictor variables (including religion) to the total proportion of the variation in income explained in our regression analysis by equation (3). This equation yields a multiple correlation coefficient $[R_{Y(EORAS)}]$ of .535 (R^2 = .286). This value of R^2 can be decomposed into components, taking advantage of the identity

$$R^2_{YV_i} = \sum_i r_{YV_i} b^*_{YV_i} .$$

Here, Y is the dependent variable, the V_i's are the predictor variables in the equation, and the $b^*_{YV_i}$'s are the standard partial regression coefficients associated with each of the variables.[21]

In our context, then, R^2 can be decomposed into five terms, each of which is simply a product of the b^* and the r associated with each of the five predictor variables. Given our problem, the equation is

$$R^2_{Y(EORAS)} = r_{YE} b^*_{YE} + r_{YO} b^*_{YO} + \sum_{i=1}^{12} r_{YR_i} b^*_{YR_i} + \sum_{j=1}^{2} r_{YA_j} b^*_{YA_j} + \sum_{k=1}^{3} r_{YS_k} b^*_{YS_k} .$$

The secondary subscripts for each of the b^*'s have been omitted. Each of the last three terms is itself a sum of products involving the individual subcategories in the variable. Substitution and multiplication yield .286 = .092 + .118 + .026 + .022 + .028. Thus almost 29 per cent of the total variation in income can be ascribed to the variation of our predictor variables. About 9 per cent can be attributed to variation in education, 12 per cent to variation in occupation, and about 2.5 per cent each to religion, race-region, and size of place.

[21] For a discussion of this identity, see H. M. Walker and Joseph Lev, *Statistical Inference* (New York: Holt, Rinehart & Winston, 1953), equation (13.6). An explanation of b^* is given in the same work at equation (13.5) and (13.13).

SUMMARY

This analysis has been based on male, fully employed individuals residing in 7,518 households included in a 1962 national sample survey. Our major concern has been an investigation of the extent to which apparent differences in reported family income among thirteen religious groups can be explained by differences in the composition of these groups. Specifically, we have used a regression approach in which income is the dependent variable while education, occupation, religion, race, region, and size of place are the independent, or predictor, variables. By assigning to all thirteen religious groups the grand means on educational and occupational attainment, plus the national distributions of race, region, and size of place, we have eliminated the effects of differences in these characteristics on the mean incomes of these groups.

We have seen that the apparent differences in actual income from group to group have been substantially reduced, and for specific comparisons have been eliminated. Table 5 revealed that for only three groups (Congregationalists, Episcopalians, and Jews) does the effect of membership in a religious group produce an adjusted annual family income which departs more than $500 from the grand mean, once compositional effects have been removed.

Certainly, a purification of some of these measures would have further reduced the apparent income differences among denominations. Notably, some satisfactory index of the quality of education would have taken into account the fact that some religious groups, because they differ in their racial, regional, or urban-rural compositions, receive educations which differ in quality and therefore vary in the skills their members can use to exchange for wages.

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Sidney Goldstein Comments on Galen Gockel's "Income and Religious Affiliation: A Regression Analysis"

It is more than coincidental that, approaching the problem from two different sets of data and employing different analytic tools, both Gockel and I reach almost identical conclusions: As I put it, ". . . many of the differences among the three religious groups to which the crude data point are in fact a function of the differential educational achievement and/or occupational composition of the members of these groups, as well as a result of their differential concentration in urban and rural places." With these factors and race controlled, I concluded that both education and occupation play a more crucial role than does religion itself in influencing the income levels of members of the three major religious groups. Using a regression approach, Gockel shows that the apparent differences in actual income among the thirteen groups he examines are substantially reduced and sometimes eliminated when occupation, religion, race, and size of place are used as predictor variables.

Each of our sets of data has advantages and disadvantages. I was limited to working largely with the three major religious groups and to a restricted number of cross-tabulations. As indicated in my paper, the original records and tabulation materials were destroyed by the Bureau of the Census, limiting the analysis to the recently released cross-tabulations. But the nationwide sam-

EDITOR'S NOTE: Because Mr. Gockel and Mr. Goldstein reached essentially the same conclusions in their research, at approximately the same time, we have asked each of them to comment on the other's articles. Mr. Gockel's comments follow Mr. Goldstein's.

ple was larger, being based on 35,000 households compared with Gockel's 12,000. The individuals in the census sample represent all persons fourteen years old and over, whereas Gockel restricts his analysis to heads of household. The census data on occupation refer to all employed persons, whereas Gockel chose to limit his data to those fully employed. One might question the wisdom of this latter choice if the rate of part-time employment varies among religious groups. My data also permit some comparisons for males and females; he limits himself to men. But his data have the advantage of permitting comparisons among thirteen religious groups, including nine subdivisions of the one combined Protestant group with which I had to work. Further, by having access to the original data, he was able to design his tabulations to best suit his research needs. Unfortunately, except for labor force participation rates, the census data I used did not cross-tabulate occupation or education by age. For unstated reasons, Gockel did not include age as a variable. To control for generational differences in the relation between education and occupation and their effect on income, age controls would be highly desirable in both studies. Yet, despite these differences in the samples and procedures used, and the respective limitations of each set of data, we independently examined virtually the same variables and reached similar conclusions.

The fact remains, however, that both Gockel and I must point to exceptions to the general patterns we identified and must look to other explanations for them. His regression analysis shows that only 29 per cent of the total variation in income can be ascribed to the variation of his predictor variables. As much as one-third to one-half of the original large differences between the average family incomes of Jews, Episcopalians, and Congregationalists and the grand mean income remain after the effects of education, occupation, race, region, and size of place have been controlled. For some of the other groups with smaller deviations (Mormons, Methodists, Lutherans, and Unspecified Protestants) the absolute differences are almost as great or even greater after control for all five variables than they were before any controls were introduced. It is true that with controls the range of deviations from the grand mean is considerably reduced. (With no factors controlled the range covered $4,227, between a high of + $2,898 for Jews to −$1,329 for Baptists; after control for all five variables, only $1,541 separated the $1,101 high of Congregationalists and the −$440 low of Baptists.) But the range remains respectable, suggesting that still other factors must interplay to produce differences among religious groups. Moreover, the fact that comparison of the rank "deviation positions" occupied before controls and those occupied after controls gives a Spearman rank correlation of 73.6 suggests that the remaining explanatory factors have a significant effect on the relative income positions of the members of the thirteen religious groups.

Gockel suggests the need for controlling differentials in the quality of education received among the members of the thirteen religious groups. This could be important, but I suspect that in itself it would not greatly reduce the unexplained variance. I have suggested that religious group differences in the values placed on achievement, as well as in the positions occupied by the respective groups in the total social structure, and the different access this gives them to positions of high income (e.g., the minority status of Jews) must be considered in accounting for inconsistencies when education and occupation are controlled. Also, I would urge pursuit of the relevance of differentials in age composition among religious groups to the variation in both education and occupational composition and their effects on income levels. Other research I have done on the Jews shows considerable status inconsistency between education and occupation among certain older-age cohorts. The census data show a greater tendency among older Jews to remain active in the labor force compared with both

Catholics and Protestants. Together with greater status inconsistency, this may explain some of the income differentials.

I believe that by demonstrating the importance of interrelations among socioeconomic variables within and between religious groups both our studies present strong arguments for reassessing earlier conclusions concerning the nature of status differentials among religious groups. At the same time, the findings of both our reports argue strongly for the continued collection of information on religion and the extension of our lines of inquiry to account for the variation which remains unexplained.

Mr. Gockel's Comments

Prof. Goldstein's article has two chief merits. First, he has performed a service simply by obtaining and disseminating the census tabulations on religion. The data contained there will undoubtedly function as a reference which investigators concerned with stratification and/or religious groups will cite for some time to come. What the data lack in currency they partially make up in quality. Heretofore one had to rely on tabulations from individual or merged national sample surveys and the problems of sampling error or bias which attend them.

Whether the presentation of these tables required the extensive exegetical commentary offered by Goldstein is another question. It is, however, as much a question of editorial judgment as of scholarly worth.

The second merit of the article has nothing to do with the data at all but with the author's review of the census bureau's decision to withhold religious tabulations from the 1960 census and to omit a question on religion from the 1970 questionnaire. A full study of the "various pressures on the Bureau of the Census" would be a substantial and intellectually profitable enterprise for a student of American society. One of its findings would surely view this particular episode as a consequence of the tenacity of primordial religious and ethnic ties in a society which, it is claimed, applies universalistic rather than particularistic criteria in making judgments.

Aside from these aspects of Goldstein's article, his major conclusions, when added to those which flow from the data in my article, should nail down the fact that differences in other areas among religious groups explain a substantial portion of their differences in socioeconomic status.

In a way, this is hardly a novel conclusion. After thinking it over before reviewing the data, most observers would understand that status differences among religious groups flow from differences among them on other grounds. However, systematic speculation of this type and on this subject is not a commonplace occurence, and it is the function of articles such as Goldstein's and mine to substitute accurate documentation for informed speculation.

A final caution is in order and stems from the rhetoric in which our conclusions are necessarily couched. When we say that the income differences between two religious groups are minimized by a specific statistical procedure, we may imply to the average reader that these differences do not really exist. They do.

The income inequities between, for example, Baptists and Episcopalians are real. Our analysis simply says that the low income of the former group flows primarily from the fact that within it are heavily represented the most disadvantaged sectors of our society, and not from the fact that its members are Baptists.

Behavior Rhythms in Mental Hospitals[1]

Murray Melbin

ABSTRACT

Patients in a small private mental hospital behave crazily much more often on weekdays than they do on evenings or weekends. They also show disturbance on evenings and weekends, but during these periods such upsets are very likely to be normal in form. This alternating timetable does not occur in a large state hospital. The difference is accounted for by the weekday presence, in the private hospital, of professional treatment personnel and clinical trainees who are receptive to the revelation of symptoms and react with interest and tolerance. The same process that leads to disappearance of symptoms upon administering a placebo may lead to their emergence if the therapeutic regime promotes it. The finding is additional evidence that the immediate environment influences behavior once thought to be due solely to individuals' internal processes.

Patients in a small private mental hospital behave crazily far more often on weekdays than they do on evenings and weekends. They also show disturbance on evenings and weekends, but upsets during these periods are very likely to be normal in form. However, in a large state institution nearby, this alternating timetable of disturbance does not occur. There, both common and unusual forms of disturbance are equally probable at any time during the waking hours of the day, or day of the week.

It is not altogether surprising to find a timetable for upsets and excitement; there is periodic discharge of pent-up feelings outside mental hospitals as well. Examples are the familiar revels on payday evenings and on Saturday nights, the raids on girls' dormitories on college campuses, and the carefree assemblies of students at beach resorts in the early spring. There is more excitement and adventure at these times, more drunkenness, more brawls. These weekly or seasonal events have their place in the bio-social cycle, offsetting the drudgery of work or the bleakness of winter. But why does unusual behavior occur mainly during the workweek at the small private hospital and more randomly in the larger setting? This difference implies that the expression of mental disturbance has to do with the environment in which it happens.

The finding is based on evidence gathered for another purpose. I did not look for it and did not expect it. Since its significance rests on the adequacy of its measurement, I will describe the research program and the grounds for behavior classification. I will then show that the timetable of disturbances can be understood within the framework of a growing body of knowledge about breakdowns and changes in behavior.

THE RESEARCH PROGRAM

I discovered the timetable in the course of examining data that had been collected in a study of mental-hospital ward attendants, or psychiatric aides. In order to learn about the behavior of aides with mental patients, four trained observers including myself gathered information about them in a survey at two hospitals. One was a small, private, psychiatric institution of 200 beds that functioned not only as a treatment

[1] I thank Lawrence B. Katz, Jr., for assistance in organizing and tallying the data, and Jerome D. Frank of Johns Hopkins University and Gerald M. Platt of Harvard University for their constructive critiques of this paper. The findings reported here are from a research program that has been supported by a Russell Sage Foundation Residency Fellowship, National Science Foundation grant G10919, and National Institute of Mental Health grant M-5702(A). Preparation of this paper was facilitated by grant RO2-NU-00251 from the National Institutes of Health.

center but also as a teaching unit for a nearby medical school. The other was a large state hospital of 3,000 beds; it had a negligible psychiatric-teaching affiliation. Both institutions housed male and female patients in a wide range of ages with a variety of diagnosed illnesses. We chose the times and places for observing aides by a probability sample design that included a random selection procedure: we listed the sites where patients and aides could be found (including wards, gym, canteen, and grounds) in the two hospitals and apportioned the waking hours of the day, from 7 A.M. to 11 P.M., into two-hour time units. We used a third list to cite the seven days of the week. Then, combinations of site, time, and day of week were drawn randomly without replacement. Because of the larger size of the state hospital, some extra observation units were selected for it. Over a period of two years we took turns visiting designated sites in both hospitals when scheduled, and recorded the conduct of aides in the presence of patients, being careful to minimize our intrusion into the situation. Along with recording the aides' conduct, we made notes of what patients were doing. The observation periods totaled forty-eight hours for the private hospital and fifty-five hours for the state hospital.[2]

To classify the behavior reported in the field records, I used a descriptive scheme based on overt events and tried to skirt the morass of assuming causes and consequences, or inferring internal processes or breakdowns of such processes. The use of categories that assume causes can undermine the impartiality required in studying determinants of conduct. So, for example, "A patient requests an item, or a service, or information"; "An aide and a patient pass each other or meet"; and "Patients are eating in the dining room" are typical of the categories I established. I was not selective in the classification. I did not presume to know in advance what is important and what is not, and I assigned every recorded event to a category even if it resulted in a lone entry. In addition to ordinary aspects of hospital life I also established categories for overt features of patients' actions that are in this essay called disturbances.

It is difficult to establish categories of disturbance. A language deals aptly with events that are familiar and relevant to its culture. The uncommon is harder to portray in words. It is hardest to develop a category for grouping unusual events together. To paraphrase Tolstoy, common disturbances are all alike, every uncommon disturbance is uncommon in its own way. Though I felt less secure in moving from specific acts to a general class, my policy was to assemble candidates for mergers, and I finally grouped bizarre acts according to the primacy of three categories: uncommon verbal behavior, uncommon physical gestures, and performing an ordinarily private act in public.[3]

[2] Since aides were the focus of the original research project, all records made about patients' behavior referred to events in the presence of aides (and of other staff members who happened to be around). If aides were not present we did not record patients' actions. This has the following implications for the reported tallies: In the private hospital, aides are assigned throughout. There were always one or more present at the designated observation sites. If this had been a sampling design for patients, the same array of observation times could have been chosen (twenty-four units of two hours each), and the same record of patients' behavior would have been made. On the other hand, the ratio of aides to patients is lower at the state hospital and aides are assigned mainly to sites where signs of illness abound. Out of sixty scheduled hours of observation (thirty units of two hours each), there were five hours during which no aides were present at the designated sample sites. These were wards whose patients were composed and able to take care of themselves. Because of this, one may interpret the data for the state hospital as based on records representing 92 per cent of the patient population and more descriptive of sicker patients than of those in better condition. I deem this within an acceptable range of sampling error. An additional 8 per cent of information about the behavior of patients at the state hospital would not contribute enough to alter the statistical evaluation of the findings.

[3] Mental illness encompasses more behavioral forms than I have classed as unusual disturbances. Schizophrenics characteristically show little interest in their environment and are prone to sit quietly and withdrawn. By the classification method used,

Table 1 lists patients' behavior, classified as either an unusual or a common form of disturbance. Unusual acts are those for which people are put into mental hospitals. The common forms of disturbance do not incur detention on the ground of mental illness. In grouping disturbances into common and unusual, I used my judgment about their frequency in our society. It is common to curse and complain but unusual to rant incoherently. It is common to hit someone or kick a chair but unusual to gyrate and twitch. We have all enacted the common forms or at least have observed such acts in daily life. They are taken as normal expressions of being upset. Nor is unusual behavior entirely distinct from mental health. People rarely do these things or observe others acting so, but on occasion healthy individuals do act strangely. It is the frequency, the repetitive enactment of an unusual repertoire, or the severity of a given performance, that leads to branding the actors crazy and consigning them to mental hospitals. The adequacy of this classification is, of course, a potential matter of controversy, as is the general issue of behavior categories for social science.

TABLE 1

CATEGORIES OF UNUSUAL AND COMMON FORMS OF DISTURBED BEHAVIOR

Unusual Disturbances	Common Disturbances
A patient performs an unusual verbal act:	A patient is in distress:
Rants, mumbles, or yells incoherently	Cries, sobs
Asks a nonsensical question repeatedly	Is afraid and ready to flee
Makes an irrelevant request repeatedly (e.g., asks for permission to put on socks although already wearing a pair)	Berates himself
Makes animal-like noises (e.g., in response to questions)	Expresses worry about own illness or fate of own family
A patient performs an unusual physical act:	A patient complains or denounces the hospital:
Gyrates and whirls	Curses
Lies stiffly on the floor in front of a doorway	Insults a staff member
Eats food spilled on the floor	Criticizes a hospital arrangement (e.g., loudly declares impatience with "the way everything is locked up around here")
Performs motion rituals (e.g., twists neck and shuffles rapidly in place while holding arms stiffly angled out from body)	A patient is negligent or destructive toward property:
Manifests tics	Sits on Ping-Pong table
A patient performs a usually private gesture in public:	Teeters on chair so that only one or two chair legs touch floor
Walks along corridors or into living room dressed only in underwear, or naked	Kicks furniture
Performs a physically affectionate gesture toward person of same sex (e.g., lays head in lap)	Slams door furiously (i.e., enough to crack pane of glass in door)
Makes a heterosexual advance (e.g., attempts to embrace other)	Shreds napkins
	A patient attacks someone by hitting or threatening to hit:
	Slaps face
	Raises fist and threatens (or wields object and threatens)
	Hurls object at someone

I established the categories and classified the records prior to being aware of a timetable for some of these events. Following the classification, and in the course of tallying the frequencies, I discovered a pattern in

such conduct on the part of patients would not have been distinguished from that of patients who were not withdrawn but who were sitting quietly watching television in the living room. The finding, then, refers to the more active forms of disturbance. The disturbances also vary in severity. Instances of a patient exhibiting distress include both a person showing apprehension when a large dog on a leash was led onto a ward, and a person sobbing. The range of unusual disturbances is even wider.

the timing of disturbances in the private hospital. The patterns for both hospitals are given in Tables 2 and 3. As shown in Table 2, the patients in the private hospital are far more likely to show an unusual disturbance on weekdays than they are on evenings and weekends. An unequal distribution of disturbances occurs on evenings and weekends, but then it is clearly in favor of the common forms of expression. By contrast, as shown in Table 3, the state hospital patients display no selectivity in the timing of their disturbances.

Because of this distinctive timetable in the private hospital and the lack of one in the state hospital, I investigated possible bias due to the observers or to the sample.

Since an observer was present on all occasions, whatever effect he may have had on patients' behavior was probably not selective for unusual versus common forms of their disturbances. But did observers differ in their perceptions and recording of what was happening? Observers were rotated in assignments to various sites in the two hospitals. Inasmuch as behavior differed among the sites, the diversity of observer's assignments made it unfeasible to check recording reliability among them. (Remember, too, that the categories of behavior were established after the stage of field observation; there was no prior basis for gauging reliability.) As shown in Table 4, the differences among observers' reports were related both to differences in amounts of their observation and the locations of disturbances. The observers on admissions and maximum security wards reported more instances of both common and unusual disturbances. This relation is evident by comparing the reports of observers no. 2 and no. 3. Relative to their hours of observation in the private hospital, no. 2 contributed disproportionately fewer citations of disturbance and no. 3 (the author) contributed disproportionately more of such citations. In the state hospital, no. 2 visited the ward housing acutely ill male patients, and here the disproportionate citations of no. 2 and no. 3 were reversed.

TABLE 2

TIMETABLE OF UNUSUAL AND OF COMMON DISTURBANCES AMONG PATIENTS IN A PRIVATE (200 BED) MENTAL HOSPITAL*

	Weekdays†	Evenings and Weekends†	Totals
Unusual disturbances...	21	3	24
Common disturbances...	13	14	27
Total.....	34	17	51

* $\chi^2_{1 d.f.} = 8.85$; for two-tailed test, $p < .01$.

† Weekdays = Monday through Friday, 7:00 A.M.–5.00 P.M.; Saturday, 7:00 A.M.–1:00 P.M. (56 hours). Evenings and Weekends = Monday through Friday, 5:00 P.M.–11:00 P.M.; Saturday, 1:00 P.M.–11:00 P.M.; Sunday, 7:00 A.M.–11:00 P.M. (56 hours).

TABLE 3

TIMETABLE OF UNUSUAL AND OF COMMON DISTURBANCES AMONG PATIENTS IN A STATE (3,000 BED) MENTAL HOSPITAL*

	Weekdays†	Evenings and Weekends†	Totals
Unusual disturbances...	13	11	24
Common disturbances..	14	8	22
Total.....	27	19	46

* $\chi^2_{1 d.f.} = 0.42$; N. S.

† Weekdays = Monday through Friday, 7:00 A.M.–5:00 P.M.; Saturday, 7:00 A.M.–1:00 P.M. (56 hours). Evenings and Weekends = Monday through Friday, 5:00 P.M.–11:00 P.M.; Saturday, 1:00 P.M.–11:00 P.M.; Sunday, 7:00 A.M.–11:00 P.M. (56 hours).

As for the sample of times and places, Table 5 shows that unusual and common disturbances were dispersed in both hospitals. Although the maximum security wards contribute more instances relative to the amount of observation time accorded them, one or more of these events were also noted at almost every other site in each setting. There was little difference in the distribution of observation times between the high-incidence sites and all others. Only in the case of the male maximum security ward at the private hospital did the times of observation for a high-incidence site fall

TABLE 4

OBSERVERS, HOURS OF RECORDING, AND TYPES OF DISTURBANCES RECORDED

Observer*	Private Hospital			State Hospital		
	Hours Recording	Unusual Disturbances	Common Disturbances	Hours Recording	Unusual Disturbances	Common Disturbances
No. 1	18	13	9	...	...	...
No. 2	22	6	12	20.5	11	8
No. 3	8	5	6	8.5	1	0
No. 4	...	...	...	26.0	12	14
Total	48	24	27	55.0	24	22

* No. 1, including female maximum security ward and canteen; No. 2, at state hospital, including male admissions ward with security wing; No. 3, at private hospital, including male maximum security ward; No. 4, including female maximum security ward.

TABLE 5

SITES WITHIN EACH HOSPITAL, HOURS OF RECORDING, AND TYPES OF DISTURBANCE

Site within Hospital*	Private Hospital			State Hospital		
	Hours Recording†	Unusual Disturbances	Common Disturbances	Hours Recording†	Unusual Disturbances	Common Disturbances
Admissions ward, male (M)	4.0	...	...	4.0	5	2
Admissions ward, female (F)	4.0	2	...	6.0	2	2
Maximum security ward, M	3.5	5	5	...	...	...
Maximum security ward, F	4.0	4	3	4.0	5	5
Geriatric ward, M	...	...	...	4.0	...	4
Geriatric ward, F	...	...	...	2.0	2	...
Geriatric ward, M and F	4.0	2	2	...	...	...
Other locked ward, M	...	...	...	4.0	2	...
Other locked ward, F	4.5	3	5	...	...	...
Other locked wards, M and F	9.0‡	2	7	...	...	...
Open wards, M	...	...	...	10.5§	4	2
Open wards, F	...	...	...	12.0§	4	6
Open wards, M and F	3.0	...	2	...	...	...
Canteen	3.5	5	2	3.0	...	1
Gym, outdoor field	5.0	...	1	2.0	...	...
Party, dance, picnic	3.5	1	...	3.5	...	...
Total	48.0	24	27	55.0	24	22

* The sample was first stratified to reflect the distribution of types of wards and sites according to designations made by psychiatrists in each hospital. Sites were then matched as closely as possible across hospitals. There was no separate maximum security ward for acutely disturbed male patients at the state hospital; a wing of the admissions ward housed this group. Only the private hospital had wards housing both male and female patients, and they were matched by pairs of wards at the state hospital. The private hospital had few open wards, the state hospital had few locked wards.

† Although the units of observation were scheduled to be two hours each, our rule was to remain beyond that time if in the midst of observing a continuing episode of interaction; another rule was not to record if aides were not present (see note 2). The instances of observation hours not divisible by 2 reflect actual recording times resulting from following these rules.

‡ Two wards.

§ Three wards.

entirely within the fifty-six-hour period identified as Weekdays, but this could not have biased the outcome of the χ^2 test of association between type of disturbance and time of day because, as Table 5 shows, an equal number of unusual and of common disturbances were noted there.[4]

It happens that an equal number of unusual disturbances were reported for each hospital. This is a coincidence and should not be taken to imply equal frequency in the settings. Several sampling differences underlie the total number of events reported in Tables 2 and 3. One is that fifteen times as many patients live in the state hospital as in the private hospital. On a per capita basis, one might then conclude that the rate of unusual disturbances is fifteen times greater in the private hospital. Even this is an understatement, for seven more hours of observation were made at the state hospital, thereby allowing more opportunity to note disturbances. What is the significance of generally higher rates of disturbance at the private hospital? They are undoubtedly affected by the amount of tranquilizing drugs used in each institution; the private hospital uses less. Therefore, absolute frequencies of disturbance is a separate issue.[5] It is the marked difference between hospitals concerning the relation between type of disturbance and time of day that claims our attention. Why do disturbances appear to be alternatives in the small private institution and not in the state setting?

[4] I also checked disturbances by the day of the week on which they occurred. Here there was also much dispersion, with incidence of disturbance related to the amount of observation as well as to the site. Because these inspections of the data indicated weak effects—if any—due to observers or to sampling times, I did not undertake a rigorous design to factor out these influences. One may also ask whether the patterns of disturbance are based on a few patients observed many times. I cannot give a definite answer because the data were gathered with aides as the focus of attention. We did not record the identities of patients. However the observations were made at a dozen or more different sites in each hospital, and, as shown in Table 5, both types of disturbance were well distributed among the sites. Also, the observations in both hospitals were spread over a period of two years. Given the limited mobility of patients among hospital wards and the population turnover due to discharges and new admissions, the chance is very small that one or a few patients turned up several times in different places precisely during our field visits and were upset in particular ways on those occasions. This strongly suggests that the entries in Tables 2 and 3 represent the actions of different individuals. If the rare possibility did occur, of the same patient observed more than once at different times and places, it would be equivalent to sampling with replacement. It would be valid and pertinent to note that he acted in an uncommon way on a weekday and acted normally upset on an evening or a weekend.

[5] The data reported in Tables 2 and 3 permit us to test the association between the two variables but not to draw conclusions about the relative frequency of types of disturbances for the two time periods within a given hospital. The frequencies are not directly comparable because the sample amounts of observation time were based on the presence of aides, not on the two equal time periods (see n. 2). Since there were more aides on duty during the day shift at both hospitals, more hours of observation were made then. The actual amounts of observation were: in the private hospital, twenty-nine hours on weekdays and nineteen hours on evenings and weekends; in the state hospital, thirty-one hours on weekdays and twenty-four hours on evenings and weekends.

To get an inkling of the comparable frequencies of types of disturbance within or across hospitals, we require an additional step of estimation based on redefining the population to be patients rather than aides. Patients are present about equally in daytimes and evenings. (Some of the healthier patients go home on weekends. This would be expected to inflate the relative frequencies of unusual to common disturbances at that time. Because this would weigh against what was found, it implies that the striking data of Table 2 are conservative.) A fair estimate can be made by taking the time period with the least hours of observation as a base line and reducing—using a random selection procedure to discard events—the reported frequencies for all other time periods until they reflect equal amounts of observation. Since nineteen hours of observation during weekends at the private hospital is the smallest amount, I carried out this procedure by first ascertaining the proportions that nineteen was of each amount of observation in another time period. Then I reduced the frequencies for that time period to that proportion. For example, there were twenty-nine hours of observation at the private hospital on weekdays (of which nineteen equals 65.5 per cent). I therefore randomly discarded 34.5 per cent of the thirty-four disturbances reported for that time period in that hospital. Following the same procedure for the state hospital, I discarded 39 per cent of the

AN EXPERIMENTAL DESIGN

The pattern of findings strongly suggest an experimental design. The tabled frequencies are arranged as if some important circumstance had the character of being sometimes introduced and sometimes withdrawn in the private hospital (call its patients the Experimental group) but not in the state (Control group) setting. The outcomes of this hypothetically varied circumstance were (*a*) more unusual disturbances on weekdays, and (*b*) more alternativity in forms of disturbance for the Experimental group but not for the Control group. This difference in outcomes prompts a retrospective search for a plausible factor that was varied. Even when one is found, of course, it will not be a demonstration parallel to a laboratory experiment in which the variable was deliberately introduced or withdrawn. It is a natural experiment.

Certain disadvantages of the contrived situation are avoided in this natural experiment: (1) The finding is not a one-trial outcome. The sampled observations were conducted over a span of more than two years, equivalent to frequently repeating the experimental conditions. (2) The observers were not oriented to this possible outcome and therefore probably did not influence it. We were interested in the actions of aides and only recorded behavior of patients in the belief that it would be wise to have data about the contexts for aides' actions. (3) We did not deliberately manipulate selected features of the situation. Here, with minimal intervention, is an outcome of the elements that combine to determine human behavior.

The two groups are only partly matched. They are similar in sex (both male and female), in age (young and old), and in locales in which they were observed. However they had different socioeconomic backgrounds. Individuals of different backgrounds end up in different hospitals. More upper- and middle-class persons are housed in the private hospital. More working-class persons are housed in the state hospital. In this report, I have negated class differences among ways of expressing hostility by grouping several forms together as common forms of disturbance. It may be that the distribution of types of illness differs between the hospitals, but the types of disturbance focused on here are the same in both hospitals. It is unlikely that some differences in illness can explain more frequent happenings of unusual disturbance on weekdays and of common forms on evenings and weekends in one hospital and explain the absence of a timetable for these same symptoms in the other hospital. The pertinent variable must be something repeatedly characteristic of the Experimental group that is related to different time periods during the week and consistently not experienced in this way by the Control group.

The absolute use of tranquilizers and other drugs is higher at the state hospital. This may help explain the fewer active disturbances per capita there, but it cannot account for the timetables. A change in drug dosage in both hospitals is almost always initiated immediately after a disturbance; the lead time in changing dosages before a disturbance is more varied. Tranquilization is likely to be a consequence rather than a determiner of disturbed behavior. Also, changes in drug dosages are

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disturbances reported for weekdays and 21 per cent of the disturbances reported for evenings and weekends. The resulting frequencies, rounded to the nearest whole numbers, are shown in the table below.

	Private Hospital		State Hospital	
	Weekdays	Eves/Ends	Weekdays	Eves/Ends
Unusual disturbances...	13	3	9	9
Common disturbances...	9	14	7	6

Since these estimates are based on nineteen hours of observation during each time period in each hospital, the frequencies are directly comparable. They confirm the impression that there is a higher rate of disturbance at the private hospital. Though based on a smaller sample (thirty-eight hours total for each hospital), there is still a significant relation between type of disturbance and time of day in the private hospital ($\chi^2_{1d.f.} = 6.81$; $p < .01$), but not in the state hospital.

made in response to both common and unusual forms of tumult.

The physical and social structure of a mental hospital is founded on the assumption that environments may be designed in which disturbed behavior may be controlled. This is exemplified in the arrangements for allocating and treating patients. In both hospitals, decisions to house patients on one ward or another are based on how manageable they are and how responsive to treatment they appear to be. If one is bizarre or violent he will be transferred to a "maximum security ward" or an "acutely disturbed ward." These buildings are staffed with more personnel and they are prepared to deal with such conduct.

This routing and rerouting practice is often justified by ascribing control of behavior to forces that can be located alternatively in the individual or in his environment. In this view, inner impulses are always pressing for expression. The form in which they emerge is competently managed by the mentally healthy person, but in illness there is a weakening of psychological control to the point where impulses break through and are discharged in strange ways. Thus an unusual disturbance is the result of being flooded by impulses with which one cannot cope. Professional staff members in the hospital often make this hypothesis explicit when they conclude that an individual showing disturbed behavior should be on a locked ward. They say, "Patients on this ward lack inner controls and are afraid of the impulses that may overwhelm them. By our presence and supervision we provide the sense of security and control they need when they can't manage their impulses."

The skilled staff members thereby testify that they act as alter egos for patients who cannot care for themselves. From this surmise, one would expect that unusual disturbances—signs of unruly impulses—would occur less often when skilled staff members are present to provide such controls and more often when they are absent. The private and state mental hospitals do differ in the availability of professional treatment personnel. The former has an ample staff of psychiatrists, social workers, and psychologists. They are well in evidence and have frequent contacts with patients on weekdays and on Saturday mornings. Evenings and weekends find the patients largely in the care of nurses and aides, and the hospital meets its requirements for medical supervision by having one psychiatrist on call.

By contrast, in the state hospital there are far fewer professionals available at any time, so that there is little difference in the compositon of the ward staff at different times of the week. Much of the professionals' contact with patients is focused on diagnostic efforts at the time of admission. Many sites in the state hospital are rarely visited by them at any time of the day or week.

The findings contradict the deduction made by the professionals from their interpretation of their function in controlling impulses, namely, on weekdays when professionals are present there are more disturbances in the private mental hospital, and more of the disturbances at that time are of unusual form.

Activities and planned recreation are more frequent during weekays than on evenings or weekends. Since they serve treatment purposes, one might assume that they take the patients' minds off their problems. This leads to a prediction that with less recreation provided on evenings and weekends, the patients will pay more attention to their own problems; they will become increasingly tense and will more likely go stir crazy and blow up. But activities in the hospital may also be interpreted in the opposite way. The accelerated pace of activities may aggravate patients and stimulate more disturbances during weekdays. Similarly, one may speculate that the sheer density of people present may provoke disturbances. More are present on weekdays, and this may become a pressure upon the patients who react accordingly. Since there are fewer activities or staff members at the state hospital, days and evenings are more alike, and these interpretations should yield

predictions that there would be less contrast in levels of disturbance between these times. However, while the events at the state hospital are compatible with the pressure-of-activities and pressure-of-people hypotheses, the distinction among types of disturbances at different times in the private hospital cannot be predicted by them. How can one account, for example, for the disproportionately greater number of common disturbances on evenings and weekends in that setting?

A REVERSE PLACEBO EFFECT

The professionals' estimate of their function does not yield a prediction satisfied by the evidence. But their concentrated presence in the private hospital is in phase with the concentrated occurrence of unusual disturbances. It is possible that they influence patients' behavior in ways they do not imagine.

The influence begins to be established as patients are inducted into the treatment culture of the hospital. Induction proceeds with a general acknowledgment that psychiatrists, psychologists, and social workers are clinicians having expertise in matters of behavior disorder. The patient usually shares this belief, and from the first contact onward he learns that the clinician seeks reciprocity with him. The clinician indicates that he will develop a therapeutic program and set its course. At the same time he asks the patient to help him understand the nature of the illness to be cured by telling him where and how it hurts. As the patient tries to co-operate by revealing his trouble, the expert deals tolerantly with the symptoms, conducting himself in a way calculated to instill trust in the patient and to get the patient to join him in focusing on the problem.

The hallmark of psychotherapy is the repetition of this ritual. The patient shows his broken mind—as he would his broken arm or his tongue—to the specialist, and the specialist reciprocates with a blend of tolerance and focused interest. This practice varies among schools of psychotherapy and even from meeting to meeting. There are also other aspects of treatment. But pervading all is the clinician's enlistment of the patient's active co-operation first in disclosing the symptoms of his trouble and then in exploring what may underlie them. Effective therapeutic technique, therefore, may lead to a patterned appearance of symptoms.

It is an outcome of the same process that leads to the disappearance of symptoms after administering a placebo. In the typical placebo effect, a patient is given a dose of an inert substance in the ceremonial context of medical practice; for certain maladies, this is enough to result in symptom relief and sometimes even in cure. Frank suggests that the placebo acquires potency by being a tangible symbol of the practitioner's role as healer and that this is intensified by his ability to mobilize the patient's expectation of help.[6] Just as the acceptance of a placebo may lead to the disappearance of symptoms, the same conditions—a patient's trust in his clinician and aroused expectancy of help—may lead to unusual disturbances if the therapeutic regime promotes the expression of symptoms (rather than their disappearance). If a skin rash can be made to vanish by symbolic manipulation, a mental outbreak can be evoked by that means. "The little that is known of personality attributes of those who respond favorably to placebos," writes Frank, "suggests that they are predisposed to accept and react to socially defined symbols of healing."[7] Patients from higher socioeconomic backgrounds have more familiarity with and faith in medical practice. They have more confidence about getting well than do those from underprivileged backgrounds. The latter, used to suffering the cuts of circumstance, more often accept their hospitalization abjectly and believe

[6] Jerome D. Frank, *Persuasion and Healing* (Baltimore: Johns Hopkins Press, 1961), pp. 66–71, 74. Frank cites (p. 70) a study by Hankoff (1958) showing that even schizophrenics respond favorably to placebos.

[7] *Ibid.*, p. 74.

that—like other disasters that befall them—their condition is hopeless. So professionals enjoy leverage over patients in the private hospital that contributes to the reverse placebo effect.[8] Since speech is the main medium in psychotherapy, there is another bit of supporting evidence: although twelve of the twenty-one unusual disturbances at the private hospital on weekdays were of the unusual-verbalisms category, there were no unusual verbalisms on evenings or weekends.[9]

The fact that unusual disturbances predominate on weekdays and common ones predominate on evenings and weekends in the private hospital suggest that the two forms may be alternatives of one another. When a patient is upset and a clinician is near, the patient will probably show an unusual form. When a patient is upset at other times, he is likely to use a common form of expression.[10]

The emphasis on disclosing one's problems is accentuated in the private hospital by another difference between it and the state institution. The private hospital is a training ground for a large medical school. Each year sixteen psychiatric residents become apprentices at the hospital, a ratio of one to every twelve patients for this group alone. In addition, there are on the scene a number of graduate students receiving field work experience supervised by professionals in the psychology and social work departments, several fourth-year students from the medical school, and a score of budding psychiatric nurses from a school of nursing affiliated with the hospital. A core element of the curriculum is teaching them how to recognize different types of mental disorder. A co-operative patient may occasionally be interviewed in front of a class. Trainees also audit case conferences in which they learn what the patient did or said, along with the instructor's interpretation of how those acts might fit a suspected diagnosis. The students are readied for the revelation of symptoms.

Whereas an experienced clinician might scan disturbances with a slow-to-judge eye, the residents and students respond more eagerly from intellectual interest and from desire to assume professional treatment manners. They focus on evidence that will enable them to assimilate diagnostic categories. A student might even cherish an event that vividly demonstrates illness, believing that he is on the right track when it appears. It is the sign of the disorder. It confirms a diagnosis. It fulfills his search for a point at which he may seize upon a therapeutic opportunity.

In contrast, the state hospital has few professional-treatment personnel and few trainees. Staffing patterns for both the weekday and the evening-weekend periods were similar. Aides made up about 70 per cent of the hospital staff and were more broadly responsible for the patients' welfare. Not well informed about matters psychiatric, aides were not selectively responsive to unusual events. There were fewer unusual disturbances per capita in the state hospital and no timetable for alternative forms of disturbance.

[8] Freud observed that once a patient entered treatment his behavior became dependent upon the therapeutic situation itself: "As soon as the treatment has taken a hold upon the patient, it appears that the entire productivity of the illness henceforward becomes concentrated in one direction—namely, upon the relationship to the physician . . . we no longer have to do with the previous illness, but with a newly created and transformed neurosis which has replaced the earlier one. . . . All the patient's previous symptoms have abandoned their original significance and have adapted themselves to a new meaning, which is contained in their relationship to the transference; or else only those symptoms remain which were capable of being adapted in this way" (Sigmund Freud, *A General Introduction to Psychoanalysis* [1915–17], trans. Joan Riviere [New York: Permabooks, 1953], Lecture 27, p. 452).

[9] The temporal distributions for the other categories in both hospitals were roughly proportional to the over-all distribution of classes of disturbances.

[10] There is a subsidiary prediction concerning the psychiatrist on duty during evenings and weekends for which I do not have the data to test. On those evenings when he is on duty, his patients should manifest unusual disturbances more than other patients if they are aware of his presence.

ENVIRONMENT-REGULATED INTERNAL PROCESSES

The underlying course of action that makes kin of placebo effects and self-fulfilling prophecies has a wider scope of influence than forms of illness. It can lead to improvement or deterioration of performance in other spheres of activity as well. Many processes internal to individuals come under environmental control. The extent of this regulation is only now beginning to be realized.

A self-fulfilling prophecy reported by Rosenthal and Jacobson on changes in intelligence among elementary school children complements the finding and interpretation offered here. The experimenters administered a test for intelligence in the spring to all the pupils in an elementary school and drew up a randomly selected list of children from each of the school's eighteen classrooms. The experimenters then confided to the teachers that on the basis of the tests these selected children would probably show marked gains in intellectual ability during the coming year. Except for this notion instilled in the teachers' minds, there were no known differences between the randomly selected pupils and those not designated as poised for an academic spurt. The authors report: "Eight months later these 'unusual' children (who had actually been selected at random) showed significantly greater gains in IQ than did the remaining children in the control group. These effects of teachers' expectancies operated primarily among the younger children."[11] Once young pupils were categorized certain ways by their teachers, something happened that resulted in the pupils confirming the expectations held of them.

Another class of internal processes that come under environmental control (from which I will later draw a relevant comparison) are the body's physiological functions, including heart rate, respiration, evaporative water loss, muscular reaction time, body temperature, and others. These follow a timetable of fluctuating activity over a (roughly) twenty-four-hour period. They increase their levels and rates steadily from the time of arising, reaching a peak in midafternoon, and then decline until the following morning. Because they are so inherent in the organism, biologists refer to this collective rhythmic phenomenon as the "body's clock." Yet the clock is responsive to environmental cues. Daylight, for example, has been strongly correlated with the types of activity that promote faster and harder work by physiological functions, and biological adaptation has resulted in the clock coming under stimulus control of sunlight or its equivalent.[12] A change in the work shift of a factory employee, or a swift trip by jet airplane traversing several time zones along an east-west route around the world, will result in disturbed physiological rhythms and it will take several days before the clock becomes synchronized to its newly phased environment.[13]

The ability to concentrate and solve problems is immediately dependent on varied sensory stimulation. Experiments in which subjects—packaged in blindfolds, muffled ears, and insulated limbs—were restricted in the variety of sensations they could receive, yielded the finding that the subjects' interest and ability to reason deteriorated while they were so constrained.[14]

[11] Robert Rosenthal and Lenore Jacobson, "Self-Fulfilling Prophecies in the Classroom: Teacher's Expectations as Unintended Determinants of Pupils' Intellectual Competence" (paper read at the meetings of the American Psychological Association, Washington, D.C., September 4, 1967).

[12] A summary of knowledge about biological rhythms appears in Rene Dubos, *Man Adapting* (New Haven, Conn.: Yale University Press, 1965), chap. ii. see also J. Aschoff, "Circadian Rhythms in Man." *Science*, CXLVIII (June 11, 1965), 1427–32.

[13] G. T. Hauty, and T. Adams, "Phase Shifts of the Human Circadian System and Performance Deficit during the Periods of Transition. I. East-West Flight" (Oklahoma City: Federal Aviation Agency, Civil Aeromedical Research Institute, report AM 65-28, December, 1965). Also by the same authors, "Phase Shifts . . . II. West-East Flight" (report AM 65-29, December, 1965).

[14] W. H. Bexton, W. Heron, and T. H. Scott, "Effects of Decreased Variation in the Sensory Environment," *Canadian Journal of Psychology*, VIII (1954), 70–76. For a symposium summary of this

There are, of course, many kinds of environment-dependent social conduct, from automobile traffic flow to voting patterns in political elections to level of productivity among members of a work group. But derangements, along with intellectual ability, thinking, and physiological functioning, were once presumed to be purely internal structures and processes—individualistic, rather inaccessible, even unchangeable. We now know that one's ripening intelligence can be aggrandized by one's teachers, that effectiveness in thinking is conditioned by sensory variety, that light is a pacemaker for body functioning, that a dose of an inert substance delivered in an appropriate context may result in the disappearance of physical symptoms, and that appearance of psychiatric symptoms may be harnessed to the presence of other persons. I listed these cases to show that many processes supposedly snugly inside the human body submit to environmental control. The discovery of a timetable of alternative forms of behavior disorders, which at first seemed surprising and alien to our understanding of mental illness, is now relegated to a large class of phenomena about which something is known. Even if insanity were a chemical disorder, or a lesion in the central nervous system, it is possible that its overt manifestations are regulated by behavior of other people. It follows that the workings of a psychiatric self-fulfilling prophecy will have much in common with the ways in which other internal processes are regulated by external agencies.

MECHANISMS

Self-fulfilling prophecy is a great glob of an idea. W. I. Thomas wrote, "If men define situations as real, they are real in their consequences."[15] His declaration has a magical aura even though it is plausible. Surely consequences do not follow directly from the way a situation is defined. How is one man's definition turned into another man's act? In broad outline one could say that one's definition of the situation leads to anticipating, and then to wanting, another to behave in certain ways; when the other's conduct reliably meets the former's expectations, it is a sign that he has become socialized; he has internalized a norm about the performance of his role and he conforms to it. However, by the subtlety of their accomplishment, placebo effects and self-fulfilling prophecies may exemplify the crux of social influence, and for that reason I will review the proceeding at closer range.

Before one man can meet another's expectations, the latter must do something overtly. Information must be transmitted through muscular movement of the first person and received through the sensory channels of the second before it can be assimilated. It is easy to notice events that modify everyday social behavior: the blare of a horn helps shape automobile traffic flow, family discussions help decide choices in elections, a punch on the arm helps regulate the output of a worker at a bench. Glimpsing the messages that affect internal activity is harder. Influences upon these events seem correspondingly more covert. That may be natural to the types of behavior involved, or perhaps it is only our unfamiliarity with this class of regulation. I will sketch the way it is accomplished, using the example of establishing a timetable of disturbances.

The process begins with an expectation in a person's mind. He becomes prepared—through refined changes in muscle tone—to respond in a particular way to what he anticipates. This muscular readiness or perceptual set, augmented by a strong expectation, may result in his performing the initial phase of the response fully prepared for what he anticipates. It may be noticed by the other person. We are familiar with the situation of others set to listen to our excuses with skepticism on their faces before we start to speak. It affects our delivery. Analogous messages are conveyed by in-

topic, see P. Solomon *et al.* (eds.), *Sensory Deprivation* (Cambridge, Mass.: Harvard University Press, 1961).

[15] Quoted in Robert K. Merton, *Social Theory and Social Structure* (2d ed.; New York: Free Press, 1957), p. 421.

cipient overt reactions on the part of professional treatment personnel. Students on the lookout for key symptoms they are learning about make advance deliveries of partial responses to those symptoms. The response has now become a stimulus.

Can such messages be sufficiently clear? Rosenthal and Jacobson think it is plausible that the teachers' behavior affected their pupils: "By what she said, by how she said it, by her facial expressions, postures, and perhaps, by her touch, the teacher may have communicated to the children of the experimental group that she expected improved intellectual performance." Referring to his previous research on the influence of expectancy on performance, Rosenthal adds that rats whom experimenters believed were more intelligent received more handling and were handled more gently than allegedly inferior animals.[16]

In my own research,[17] I have found that the use of additional sensory channels to convey the same message serves a function similar to redundancy in verbal communication. An individual's sense organs operate interdependently. Touch, for example, is often subservient to vision in the estimation of magnitudes. If some sense receptors are not used in communicating, they are open to the receipt of interfering signals. This is probably why a parent or teacher exclaims, "Look at me while I'm talking to you!" It is equivalent to establishing a high signal-to-noise ratio in radio communication. It makes the message clearer and reception more reliable. In this way someone can convey an expectancy stimulus for unusual acts more fully, by utilizing more sensory channels in ostensibly the same behavior that others employ. Stealthy influences on another's conduct are found in the redundant elements of obvious acts.

If his expectation is confirmed, the clinician may then transmit an institutionalized signal of the treatment procedure. A standard gesture in therapeutic practice is to ask firmly and sympathetically, "Why did you do that? What's the matter?" This disarmingly validates the patient's act, for it reiterates the initial interest of the expert in the nature of the illness.

Why should a patient continue to be responsive and repeat this performance? If the clinician's solicitude strengthens the probability of an unusual disturbance recurring, this explanation is compatible with Skinner's hypothesis that performing a given act depends on the experience of a gratifying consequence.[18] I do not think the clinician's response has to have a rewarding character. Even a stern admonition may work. The reason is that in a realm as feebly understood as mental illness there is much uncertainty about just what to do to help a patient. The staff engages in groping and erratic behavior toward him; not harmful, but directionless. In this haphazard milieu, it is enough that there be one predictable reaction on the part of the clinician. Although it is itself not gratifying, its consistency can be by virtue of its uncertainty-reducing power. What is regular will regulate. In social interaction this regulation is often mutual. Both clinicians and patients seek to reduce uncertainty. Both vary and search as long as the other's behavior is outside a given range, and both arrive at a mutual adjustment. The patient may therefore make disclosures of unusual upsets because he has learned that such conduct brings his environment under control: it summons a reliable response from the clinician who otherwise reacts in a diffuse and indefinite manner. An irony of this situation is that clinicians do not believe they incite patients' unusual disturbances, while patients may believe they regulate clinician's reactions.

Although they first become contingent upon the actual presence of clinicians, unusual disturbances may acquire a rhythm subject to secondary cues. The periodicity of unusual disturbances becomes attuned to whatever events in the milieu signal the

[16] See Rosenthal and Jacobson, *op. cit.*, pp. 3, 9.

[17] To be reported in another writing.

[18] B. F. Skinner, *Science and Human Behavior* (New York: Macmillan Co. 1953), chap. v.

weekday period. These cues are afforded by the behavior of non-professional personnel and of other patients who respond to clinicians in the vicinity.[19] This may be any conduct that differentiates the time period, whether it is formality, uneasiness, imitation of therapists, or other. Their actions initially complement the professionals' presence. Later their acts serve as partial cues to the situation, setting off unusual disturbances even though clinicians are not present.[20] Because of this, it is not necessary to the explanation that patients have direct knowledge of clinicians being present, or that they be aware that this is linked to their upsets. An interactive pattern can be established in which patients' acts are synchronized to a complex social environment, and the pattern—liable to decay—can be rejuvenated by occasional contacts with clinicians.

One more mechanism deserves mention. Since humans are capable of orientation to the future, the temporal component of interaction with clinicians may be affected. An interesting aspect of the linkage between physiological functions and time of day is that, once a synchronization is established, the performance curves of physiological activity become phased so that they appear anticipatory. The heart, for example, after idling most of the night, begins to hasten its pumping rate in advance of the person's wakening and while it is still dark outside.[21] Similarly, patients' actions become phased. The records show that, at the private hospital, unusual disturbances did not occur on Sunday but occurred more often on Monday than on any other weekday, as if patients had readied a big hello for the professionals' return.

[19] It would be useful to know how many staff members of each type were present when patients showed unusual disturbances. The field records, focused as they were on aides, are not complete enough to provide this information.

[20] If behavior of others may become associated with the behavior of professionals, here is a further clue to tracing the route by which social influence is conveyed. One might investigate timetables in alternative forms of behavior among aides. Are they less relaxed on weekdays than on evenings and weekends? What do they do that differs at these times?

[21] Hauty and Adams, "Phase Shifts . . . I. East-West Flight," and "Phase Shifts . . . II. West-East Flight."

ORGANIZATIONAL CONTRADICTIONS

It is understandable that a plan for a hospital would combine treatment and teaching. Medical care and medical education are prized in our society. They are also harmonious values. Enlarging the scope of professional services calls for increased training programs, and emphasis on the latter would serve the former. In planning an organization, these purposes could be formally established and the means for achieving them be rationally worked out. But just as deductions from a set of carefully developed premises sometimes lead to surprising conclusions, events in the private hospital suggest that some of the methods of treatment and teaching may serve one goal while subverting the other.

The meeting place of the conflict is a dilemma posed for the psychiatric patient. He must learn when to act sick in order to receive help. Inasmuch as catharsis is part of psychotherapy, the treatment of mental disorder is like the treatment of a boil. The festering sore is brought to a head and healed in the process of having its noxious materials discharged. But health is gauged according to the absence of handicapping symptoms. So after having received professional attention and guidance for a while, the patient is expected to show progressively fewer unusual disturbances.

At the same time, attention to unusual symptoms is part of the training program in diagnostics. If patients exhibit such disturbances on a weekday schedule, the fitness of the hospital as a teaching institution is enhanced.[22] It makes a set of case ex-

[22] Croog observes that "patients are commonly evaluated by various levels of staff in terms of such characteristics as manageability, personality traits, types of disease or disability, and their potential interest as teaching material. Thus, in a teaching hospital the patient with the rare disease or mys-

amples available to its trainees when they are present.

However, expectations of symptoms for teaching purposes, without awareness that clinicians can bring these displays into phase with their presence, can undermine the validity of judgments about whether or not a patient is benefiting from treatment. What is entered in the patient's dossier? Is a single event of unusual disturbance taken to mean that the patient is responding to the therapist, or does it confirm the severity of illness?[23] It may be labeled "continued manifestation of lack of control" by a staff member who does not realize the part he plays in affecting an inmate's conduct.[24] Here is self-fulfillment of a prophecy with a vengeance.

When an event has useful consequences for one organizational goal, it is harder to recognize that it has dysfunctional implications for another. In this case, the disappearance and appearance of symptoms are conflicting means to compatible ends.

terious disorder often stands in a high position in the hierarchy of ratings" (Sidney Croog, "Interpersonal Relations in Medical Settings," in H. Freeman *et al.* [eds.] *Handbook of Medical Sociology* [Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1963], p. 264). Szasz, more excitedly, writes that "physicians need sick people! ... Anyone who acts sick—impersonating, as it were, this role—and does so vis-à-vis persons who are therapeutically inclined, runs the risk of being accepted in his impersonated role" (Thomas Szasz, *The Myth of Mental Illness* [New York: Harper & Row, 1961], p. 255).

[23] Rosenthal and Jacobson found that teachers rated the children in the two artificially created groups differentially, and did so in a way that reduced the dissonance between a child's performance and the teacher's expectations. For example, the more a child who had not been designated as a "bloomer" gained in IQ, the more he was regarded as less well adjusted, less interesting, and less affectionate. This was especially true for children of a given grade level who were in the slow-learner class rather than in the average- or rapid-learner class. Even children in the slow-learner class who were earmarked for intellectual growth, and who gained as much in IQ relative to the control group children as did their counterparts in the rapid-learner class, were rated less favorably by their teachers (Rosenthal and Jacobson, *op. cit.*, pp. 6–7).

CONCLUSIONS

There is little attention to the timing of a single event if it is observed in a familiar setting and the act is considered legitimate in the role. A systematic survey enables the disclosure of a regularity that the single event could not impart. This, in turn, may teach us something new about the event itself. Probability-sampling designs are called for in studying human behavior just as they are for surveying the distribution of any population of events.

Mental illness is thought to be a personal malady. It is sometimes interpreted as a breakdown of the individual's participation in social life. But even if such disorders are of internal instigation, even if their manifestations deviate sharply from accepted conduct in the culture, they may still come under social control. A mental patient can learn to reveal his symptoms in response to the social definition of clinicians as healers, and in reaction to their conduct expecting disorder. Since the patient becomes attuned to a complex environmental pattern, the clinicians for whom he primarily performs need not even be present for the tumult to occur on schedule. Because the psychotherapeutic ritual may elicit psychiatric symptoms, just as the medication ritual may dispel disease symptoms, placebo effects and self-fulfilling prophecies are manifestations of the same underlying process.

In the ebb and flow of favoring various explanations of social existence, a high tide of attention to what people carry within them from place to place occurred in the first half of the twentieth century. It re-

[24] Hans von Hentig (*The Criminal and His Victim* [New Haven, Conn.: Yale University Press, 1948]) has reviewed cases of homicide in which much evidence points to contributions made by the victims. He argues that it is an oversimplification to categorically distinguish the one who acts and the one who is acted upon (p. 384; see also pp. 397, 448), and he refers to the establishment of a "nefarious symbiosis" between doer and sufferer (foreword). Perhaps the timetable of unusual disturbances in the private hospital is an expression of another such nefarious symbiosis.

flected a wide swing, from earlier doctrines of the mind wholly composed of and dependent upon sensations to exaggerated emphasis on environment-free thought, rational and irrational and unconscious. Consider the familiar claim that a person is "motivated to comply with internalized norms." Now evidence has accumulated that a number of processes, formerly thought to operate wholly inside individuals, are subject to outside control. There is more external regulation of internal systems than hitherto believed. We should therefore reconsider the emphasis put on ideational processes as determinants of action. A balance between such influences and sensory cues ought to be re-established in formulas of behavior and in the focus of research. Many so-called personal attributes and apparent adherence to norms instead of being derived from internal states are part of a behavioral array at the interface between a person's makeup and his milieu. Without experimental manipulation of contexts, or controlled investigations of the type utilized in this report, the massive cues to conduct that are provided by one's immediate surroundings will remain slighted. Until we acquire systematic knowledge of environments we will not appreciate how powerfully and pervasively they work.

In designing and administering an organization in which man will participate, his environment-regulated nature should be taken into account. Recruitment and evaluation that rely on tests presuming to tap deep and enduring behavior dispositions have a lopsided validity. Because they disregard immediate environmental circumstance, diagnostic instruments used in education to assess intelligence, in hospitals to assess mental health, and in industry to assess managerial aptitude, are inadequate predictors of conduct. Cervantes[25] put it: "All that knew *Sancho* wonder'd to hear him talk so sensibly, and began to think that Offices and Places of Trust inspir'd some Men with Understanding, as they stupify'd and confounded others."

HARVARD MEDICAL SCHOOL

[25] Miguel de Cervantes, *Don Quixote* (1605–15), trans. Peter Motteux, rev. Ozell (New York: Modern Library, 1930), Part II, Book III, chap. xlix.

Sponsorship and Organizational Stability: Boy Scout Troops[1]

Phillip R. Kunz

ABSTRACT

Sponsorship is conceptualized as a special case of interorganizational relations in which the organization being sponsored utilizes other organizations to implement its own program at the grass-roots level. A national probability sample of the Boy Scout troops is examined, and it is determined that the type of sponsor has little effect on the activities of the scout troop. The type of sponsor, however, does affect the troop in that sponsors tend to have a very different turnover rate. While it is commonly believed that the sponsors of scout troops would be pluralistic in nature, the evidence indicates that the selection and retention of sponsors becomes a very selective process.

One device for implementing a large-scale organization is the utilization of a pre-existing organizational structure as a "sponsor." The chief problem of this paper is to analyze the relationship of the sponsored or "beneficiary" organization and the sponsoring organization through a study of the Boy Scouts and their sponsors.

Sponsorship as it is understood here includes two defining elements: (1) the beneficiary organization retains its distinct boundaries from the sponsoring organization, and (2) the beneficiary organization legitimately makes use of the sponsoring organization's facilities.[2]

Much of what is called sponsorship in everyday parlance will not be included in this definition. For example, United Fund would *not* be a sponsor of all the recipients of its financial aid. It would be more correctly seen as a sponsored or beneficiary organization encompassed by an organizational context. The various organizations used to raise money for United Fund would be its sponsors. While this definition of sponsorship may depart somewhat from common usage, it will be serviceable for the purposes of this paper.

THE BOY SCOUTS OF AMERICA

The Boy Scouts of America, under the auspices of a federal charter granted by Congress, has had phenomenal success in the extension of its program to eligible boys throughout America. This growth in scouting, especially in the middle class, has taken place without the national organization having had to build a vast organizational structure of its own. The national scouting organization has rather utilized the device of sponsorship to extend its program to the approximately two million boys currently enrolled.

The Boy Scouts of America provides its "character building and citizenship training program" to various local organizations which are chartered annually by the Boy Scouts to use the program. This is much the same as a company which offers a franchise to someone to sell its nationally advertised product. The local organizations are expected to provide and maintain the leadership and meeting place for the troop. How well the troop functions depends to a great extent on this sponsor, which essentially "owns" the troop. In this way, the Boy Scouts of America utilizes an already exist-

[1] Adapted from Phillip Ray Kunz, "Sponsorship as an Organizational Device: The Case of the Boy Scouts of America" (unpublished Ph.D. dissertation, University of Michigan, 1967). I am indebted to Leon H. Mayhew and Gerald Suttles for reading the manuscript, suggesting ideas which improved the paper, and encouraging publication.

[2] While both organizations "benefit" from the relationship in some sense, the organization being sponsored is designated as the beneficiary organization here, since we are looking at the organization specifically.

ing organizational structure to obtain membership, leadership, and access in general to what is necessary for its continuous existence as an organization.

DATA

Data for this study were based on information obtained from a national probability sample of scout troops collected by the Survey Research Center of the University of Michigan.[3] In addition, I obtained information in 1966 relative to the same troops sampled by the Survey Research Center in 1959. This additional information provided for analysis of the troops over a seven-year period. Other information was obtained from the annual reports of the Boy Scouts of America to Congress.

The sponsoring organizations were differentiated in three ways: (1) goal alignment with the Boy Scouts, (2) inclusion of troop members within the sponsoring organization, and (3) organizational resources of the sponsors. If the major purpose of the organization was directed toward youth, for example, a school, it was listed as "youth oriented." Those organizations which had some subunit directed toward youth were listed as "partially youth oriented," like the Rotary Club, which has a committee organized to deal specifically with youth. Those organizations with goals other than youth oriented, like a fire department, were designated as "non-youth oriented." The sponsoring organizations were divided into these three divisions after examining their organization and purpose as given in *The Encyclopedia of Associations*.[4] Inclusion refers to a person being a member of both the sponsoring *and* the beneficiary organizations, for example, a student of the sponsoring school who is also a troop member.

Organizational resources refer not only to financial resources but more especially to the organizational structure which can be used to implement the activity and make the changes that are from time to time necessary in the sponsorship relation. An *ad hoc* committee of parents would not have organizational resources because they would not have the facilities to replace themselves. Contrast this with the Mormon ward, where with some type of divine authority the bishop "calls" a new scoutmaster to "serve."

Each of these variables was employed to ascertain the effect of sponsorship upon stability of the scout troop. Stability was operationalized as (1) whether a troop was still organized after eight years, and (2) whether the troop had changed its sponsor during the same period of time.

HYPOTHESES AND FINDINGS

Where the sponsoring organization and the beneficiary organization have the same general goals, they have a common interest in maintaining the relation. It can be expected, then, that changes in sponsorship will vary negatively with the goal alignment of the beneficiary and sponsoring organizations.

The hypothesis was tested by analysis of the relationship between change in sponsorship and youth orientation. These data are presented in Table 1. The percentage of troops with a change is negatively related to youth orientation as the hypothesis predicts. The χ^2 is 13.62, with two degrees of freedom, which is significant at the .01 level. Looking at the extremes, it may be noted that 69 per cent of the troops sponsored by youth-oriented organizations had not changed sponsor, whereas only 36 per cent of the troops with non-youth-oriented sponsors had retained the same sponsor.

Where the sponsoring organization has organizational resources, it will be able to call upon these resources in maintaining the beneficiary organization. The assumption was made that those organizations with a national affiliation, and especially those with hierarchical superordinants, have more organizational resources.

Table 2 represents the findings of this

[3] Survey Research Center, *A Study of Boy Scouts and their Scoutmasters* (Ann Arbor, Mich.: Institute for Social Research, 1960).

[4] *The Encyclopedia of Associations*, ed. Frederick Ruffner *et al.* (4th ed.; Detroit: Gale Research, 1964), Vol. I.

relationship. The χ^2 in this case is 30.39 (4 d.f.), which achieves the .001 level of significance. As can be observed from Table 2, 79 per cent of the national denominations (religious organizations with a national affiliation) retained sponsorship of their troops. But schools and PTA's were found to be higher in retaining their troops than national civic and service organizations, with 66 and 62 per cent, respectively. This finding indicates that the sponsors with greater organizational resources will reduce the problem of sponsor turnover for the sponsored organization. The value of this type of sponsor for a national organization such as the Boy Scouts is especially evident.

The argument can be made that once organizations with high organizational resources were sponsoring scout troops, they would be better able and more willing to continue sponsorship. National organizations with many local units acting as sponsors would tend to implement such sponsorship as a policy. Thus, it could be expected that change in sponsorship will vary positively with the proportion of an organization's locals which act as sponsors. To test this hypothesis, an Index of Sponsorship for

TABLE 1

CHANGE IN SPONSOR BY YOUTH ORIENTATION (PERCENTAGE DISTRIBUTION)

Youth Orientation	Change in Sponsor: No Change (%)	Change in Sponsor: Change (%)	Total (%)	Total N
Youth oriented..........	69	31	100	226
Partially youth oriented..	62	38	100	81
Non-youth oriented......	36	64	100	33
Total N.............	218	122	....	340

$\chi^2 = 13.62$; 2 d.f.; $p < .01$

TABLE 2

CHANGE IN SPONSOR BY TYPE OF SPONSOR (PERCENTAGE DISTRIBUTION)

Type Sponsor	Change in Sponsor: Same Sponsor (%)	Change in Sponsor: New Sponsor (%)	Total (%)	Total N
National denomination...	79	21	100	126
National civic and service	62	38	100	82
School and PTA.........	66	34	100	38
Local denomination[a].....	50	50	100	60
Local civic and parents...	35	65	100	34
Total N.............	218	122	....	340

$\chi^2 = 30.39$; 4 d.f.; $p < .001$

[a] Local denomination refers to religious organizations that are autonomous and without national affiliation.

denominational-type sponsors was constructed by considering the total number of local church units in the denomination (within the United States) and the total number of troops sponsored by the denomination in 1965. The Index of Sponsorship was computed by first dividing each denomination's local church units by all local church units. This percentage was then divided into the percentage of all troops sponsored by that denomination. Thus, the Church of Christ has 6.2 per cent of all the local church units in the United States; however, it sponsors only 0.5 per cent of all the troops sponsored by religious denominations. Dividing 0.5 by 6.2 yields its index score of 0.08, which indicates that this denomination does not sponsor as many troops as it should, based on the number of local church units it has. Table 3 presents this Index of Sponsorship.

This index was dichotomized into High and Low, with High referring to 1.00 or above, that is, for a denomination which has sponsorship of troops proportional to its number of local units, and Low referring to those with fewer troops than would be expected on the basis of their number.

This dichotomized Index of Sponsorship was then compared with the outcome of the troop. As Table 4 indicates, the χ^2 is 4.41 (1 d.f.), which is large enough to be significant at the .05 level. This indicates that

TABLE 3

SELECTED DENOMINATIONS BY PERCENTAGE OF TOTAL LOCAL CHURCH UNITS IN THE UNITED STATES, PERCENTAGE OF SCOUT TROOPS SPONSORS IN 1965, AND INDEX OF SPONSORSHIP[a]

Denomination	Total Church Units (%)	All Religion-Sponsored Troops (%)	Index of Sponsorship
African Methodist Episcopal	1.8	0.9	0.50
African Methodist Episcopal Zion	1.3	0.4	0.31
Assemblies of God	0.3	0.2	0.67
Baptist	25.9	12.3	0.48
Christian Church (Disciples of Christ) Congregational	2.5	3.4	1.36
Christian Methodist Episcopal	0.8	0.2	0.25
Church of Christ	6.2	0.5	0.08
Church of God	3.0	0.7	0.23
Church of the Brethren	0.4	0.3	0.75
Church of the Nazarene	1.4	0.3	0.21
Eastern Orthodox	0.6	0.2	0.33
Evangelical United Brethren	1.3	1.5	1.15
Friends	0.3	0.1	0.33
Jewish	1.4	1.3	0.93
Latter-Day Saints	1.3	8.7	6.69
Lutheran	5.7	9.2	1.61
Methodist	9.9	20.6	2.08
Moravian	0.1	0.1	1.00
Presbyterian	4.5	10.3	2.29
Protestant Episcopal	2.3	4.1	1.78
Reformed	0.5	0.6	1.20
Roman Catholic	7.3	16.4	2.25
Salvation Army	0.4	0.8	2.00
Unitarian Universalist	0.3	0.1	0.33
United Church of Christ	2.2	4.2	1.91
Other churches	18.3	2.6	0.14
Total %	100.0	100.0	....

[a] The Index of Sponsorship was computed by dividing each denomination's percentage of all local units by the percentage of all troops sponsored by a religious denomination in 1965.

those denominational type sponsors with a proportionally high number of troops have fewer discontinued troops.

This same influence, then, can be expected to prevent sponsors from discontinuing the relation. It follows that discontinuation of sponsorship will vary positively with the proportion of an organization's locals which act as sponsors.

The Index of Sponsorship is also highly related to change of sponsor, as Table 5 indicates. In this case, 81 per cent of the denominational sponsors within the High Index category retained sponsorship of their troops, whereas only 57 per cent of those within the Low category retained sponsorship. The χ^2 was 12.69 (1 d.f.) and attained the .001 level of significance.

Where a change of sponsorship occurs, the beneficiary organization incurs a certain cost in that resources must be utilized to obtain a new sponsor. Those responsible for paying this cost would tend to obtain a new sponsor with greater probability of being stable in the relation. It is expected that when change of sponsorship occurs, it will tend to be in the direction of increased stability.

This hypothesis was tested by comparing the type of sponsor for the 340 troops from the national probability sample at two points in time: 1958 and 1966. As was indicated above, 122 of the troops still intact in 1966 had changed sponsor during that period of time. Table 6, which presents this data, is divided into three parts. The first

TABLE 4

INDEX OF SPONSORSHIP FOR DENOMINATIONAL TYPE SPONSORS BY OUTCOME OF TROOP (PERCENTAGE DISTRIBUTION)

Index of Sponsorship	Outcome of Troop		Total (%)	Total N
	Organized (%)	Discontinued (%)		
High	85	15	100	101
Low	73	27	100	84
Total N	147	38	....	185

$\chi^2=4.41$; 1 d.f.; $p<.05$

TABLE 5

CHANGE OF SPONSOR BY INDEX OF SPONSORSHIP (PERCENTAGE DISTRIBUTION)

Index of Sponsorship	Change of Sponsors		Total (%)	Total N
	No (%)	Yes (%)		
High	81	19	100	101
Low	57	43	100	84
Total N	130	55	....	185

$\chi^2=12.69$; 1 d.f.; $p<.001$

presents a frequency distribution; the second presents a percentage distribution, by row; and the third gives the ratios of the observed to the expected frequencies. The expected frequencies were computed by the row and column marginal totals. A ratio of 2.7, for example, indicates that the observed frequency for that cell is 2.7 times the expected frequency for that cell. A ratio less than 1.0 means that the observed frequency was less than the expected frequency for that cell.

One should note the strong tendency for the sponsor in Time 2 to be within the same type of organization as it was at Time 1, as the diagonal cell from the top left to the bottom right in each part of the table indicates. In order to understand this tendency better, a diagonalization ratio[5] was computed. Laumann gives the following formula:

$$\text{Ratio}_d = \frac{(X_o - X_e)}{X_e} .$$

Laumann explains: "Where X_o is the observed frequency in diagonal cell O, and X_e is the expected frequency for that cell. This ratio might be interpreted as the pro-

[5] I am especially indebted to Edward Laumann for the method of analysis in this section. All of the techniques used here are explained in more detail in Edward O. Laumann, *Prestige and Association in an Urban Community: An Analysis of an Urban Stratification System* (Indianapolis: Bobbs-Merrill Co., 1966), chap. v.

TABLE 6

TYPE OF TROOP SPONSOR IN 1966 BY TYPE OF SPONSOR IN 1958 FOR THOSE TROOPS WHICH HAD CHANGED SPONSOR

1958 SPONSOR (TIME 1)	TYPE	1966 SPONSOR (TIME 2) (1)	(2)	(3)	(4)	(5)	Total
Frequency Distribution							
National denomination	(1)	20	3	2	1	0	26
National civic and service	(2)	5	20	2	3	1	31
School and PTA	(3)	1	1	9	1	1	13
Local denomination	(4)	4	6	1	17	2	30
Local civic and parents	(5)	5	5	2	1	9	22
Total	......	35	35	16	23	13	122
Percentage Distribution							
National denomination	(1)	76.9	11.6	7.7	3.8	0.0	100.0
National civic and service	(2)	16.2	64.5	6.4	9.7	3.2	100.0
School and PTA	(3)	7.7	7.7	69.2	7.7	7.7	100.0
Local denomination	(4)	13.3	20.0	3.3	56.7	6.7	100.0
Local civic and parents	(5)	22.7	22.7	9.1	4.6	40.9	100.0
Ratio of Observed to Expected Frequencies							
National denomination	(1)	2.7	0.4	0.6	0.2	0.0	....
National civic and service	(2)	0.6	2.2	0.5	0.5	0.3	....
School and PTA	(3)	0.3	0.3	5.3	0.4	0.7	....
Local denomination	(4)	0.1	0.7	0.3	3.0	0.6	....
Local civic and parents	(5)	0.8	0.8	0.7	0.2	3.9	....

portion of the observed frequency falling in the diagonal cells that exceeds (or is less than) the frequency to be expected by change to fall in the diagonal cells."[6]

The diagonalization ratio for Table 6 equals 0.652. Thus, when troop sponsors do change, there is a strong tendency to obtain a new sponsor of the same type. One possibility which may obtain is that organizations within each category are linked by some ties which would give the old sponsor or the Boy Scout Council access to the same type of organization as a new sponsor. Note especially the ratios of observed to expected frequencies in the diagonal cells for types (3), (4), and (5) in Table 6. These three types of organizations are much more likely to retain sponsorship within their own category. Because of the small sample size one should not place too much confidence in details, but the over-all structure is important.

The frequencies on the diagonal represent sponsors with *no* change in stability. Our current interest lies especially with the direction of change for those cases off the diagonal. The cases in the upper right-hand side of the matrix represent sponsors with less stability, while those in the lower left-hand side represent sponsors with increased stability at Time 2. A symmetry ratio was computed by dividing the sum of the frequencies on the left of the diagonal by the sum of the frequencies on the right. A symmetrical table would yield a ratio of 1.00. The symmetry ratio for Table 6 is 1.98, which means that the left side has 1.98 times the frequency of the right side. This is the direction of movement predicted by the hypothesis.[7]

While the frequency in most of the cells is small, by inspecting the individual cells on the left-hand side and comparing them with their counterparts on the right-hand side, one can more fully understand the data. For example, the frequency distribution shows that five troops sponsored by local civic or parents groups in Time 1 changed to national denominations in Time 2. On the other hand, no troops initially sponsored by national denominations changed to local civic or parents groups. Five troops sponsored by local civic or parents groups in Time 1 changed to national civic and service organizations, while only one case is shown for the opposite type of move.

Another way of observing this trend is to note that the marginal totals in the frequency table for (1), (2), and (3) *increased* from Time 1 to Time 2, while (4) and (5) *decreased*, again indicating a shift toward more stable sponsors.

The argument was made above that sponsors with high organizational resources would be more likely to retain this relationship over a period of time. In addition, it was argued that the beneficiary organization would attempt to obtain more stable sponsors when changes in sponsorship did occur. It is expected, then, that historically the Boy Scouts of America will have shifted from sponsors with fewer organizational resources to sponsors with more of such resources.

This hypothesis was examined by plotting the total number of troops (less a few troops classified as miscellaneous each year) sponsored by the five different types of sponsors for a period of fifty-one years. This

[6] *Ibid.*, pp. 81–82.

[7] One should use caution with this finding, however, for another test of asymmetry of the matrix fails to achieve significance. This test is described by Laumann as follows: "The asymmetry test is based on the Chi-square distribution. The Chi-square is calculated according to the following formula:

$$\chi^2 = \sum \frac{(X_1 - Y_1)^2}{(X_1 + Y_1)},$$

where X_1 is the frequency in a cell on the right-hand side of the diagonal cells and Y_1 is the frequency in the comparable or counterpart cells on the left-hand side of the diagonal cells. With a five-by-five matrix, there are ten such pairs of cells; and, consequently, there are 10 degrees of freedom. When such a Chi-square achieves an acceptable level of significance, it indicates that the probability of observing such an imbalance between the right-hand and left-hand cell due to chance alone is quite small" (*ibid.*, pp. 82–83). In this case, the χ^2 was 12.79, with ten degrees of freedom, which yields a $p < .30$.

data was obtained from the Boy Scouts' annual reports to congress for those years in which they were published and from the private reports for the years prior to that. Data was not available for three years: 1919, 1924, and 1928. In these three instances the figures for the preceding and following years were used to interpolate the data for the missing years. A regression slope was then computed for each type of sponsor. The beta coefficients are reported in each case in Figure 1. As can be observed there, the slope of the line for national denominational type sponsors ($\beta = 4.46$) is greater than for the others: $\beta = 2.88$ for national civic and service organizations, 1.60 for social denominations, 1.70 for schools and PTA, and, finally, 0.79 for local civic and parents groups. As Figure 1 indicates, the local civic and parent group type sponsors are most marked in decrease, and the national denominational types have increased the most.

Other hypotheses relative to these vari-

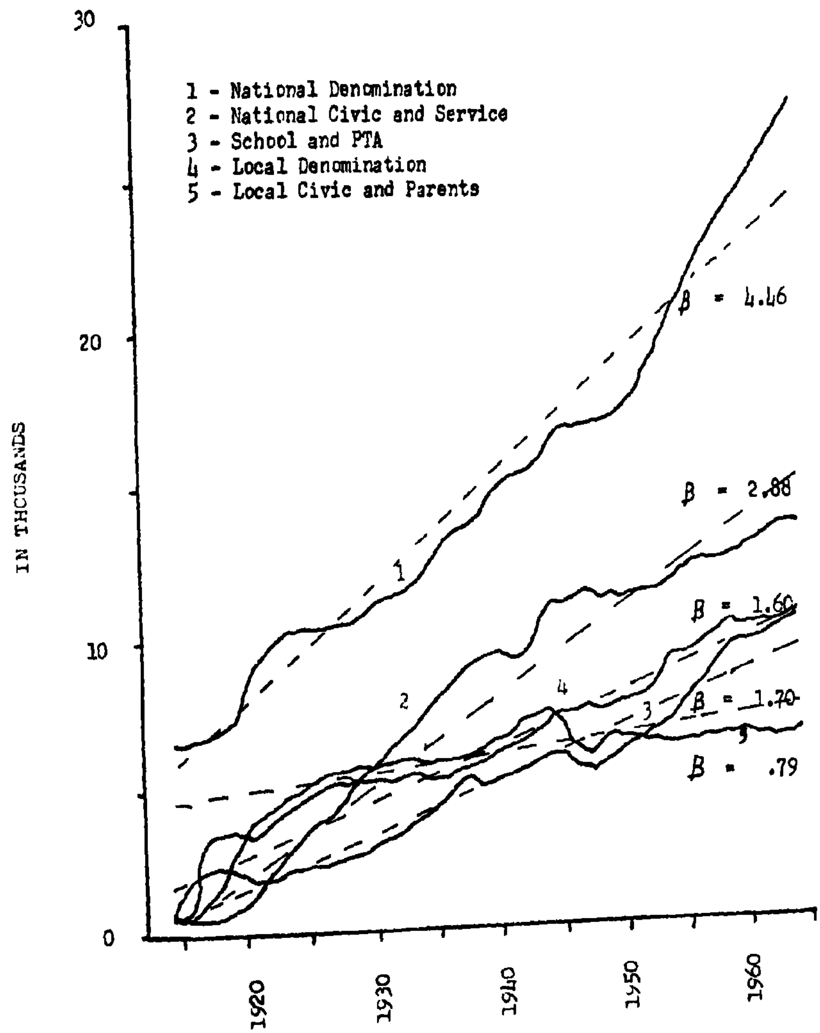


FIG. 1.—Boy Scout troops by type of sponsor 1915–65

ables, for which the data were in the predicted direction, but *not significantly* so, were the following:

Discontinuation of the beneficiary organization will vary negatively with the degree of inclusion of the beneficiary's members in the sponsoring organization.

Turnover in sponsorship will vary negatively with the degree of inclusion of the beneficiary's members in the sponsoring organization.

Discontinuation of the beneficiary organization will vary negatively with the goal alignment of the sponsoring and beneficiary organizations.

Discontinuation of the beneficiary organization will vary negatively with the organizational resources of the sponsoring organization.

None of the hypotheses relative to these variables discloses data in a direction other than that which was predicted.

DISCUSSION

As a society, we are increasingly involved in large-scale organizations. Many commentators on modern social life have observed widespread organizational involvement as a basic and pervasive characteristic of our current society. Modern man is man in organizations.

Because of this, large-scale programs can be carried out for our mutual benefit. Furthermore, complex organizations, as social realities, often relate to other organizations and utilize them to attain some purpose or goal. In this age of large-scale organization, the application of sponsorship, as a means of introducing some stability and support to local small-scale activity, is an important device.

A large organization can limit its own need to build up a vast organizational structure by utilizing other organizations with already existent structures. More important, however, the large organization can gain access via sponsorship to members and other resources which might not otherwise be available.

This device permits the extension of an organization to the grass-roots level without extensive bureaucratic organization of the beneficiary. Sponsorship provides facilities such as membership, training, and monitoring of small-scale activity by an existing organizational system, thus relieving the beneficiary organization of these various functions.

Thus, United Fund with only a very skeletal organization can mobilize once every year with its various community-based sponsors to fund its many recipients. In this case, United Fund is able to maintain itself without maintaining extensive personnel and, in addition, can swell its coffers with whatever pressuring tactics its sponsors can employ upon their own members.

Or, one may examine the National Institute of Mental Health (NIMH) in a like manner. The NIMH provides, among other things, training stipends for students in various universities. The university, as the keeper of the money and the trainer of the student, is in this sense a sponsor of NIMH.

It is easy to see the implication for the possible future extension of this type of organization on the national level. Thus, health programs which are basically the responsibility of the federal government can be carried out by the local existing organizations. Federal projects to inoculate against a disease can utilize local medical associations and carry out the program. The federal government could utilize local police systems to train a force for riot control.

Apparently one could utilize such an organizational device to bring any large-scale program to the local level. The case of the Boy Scouts of America also indicates the possibility of using such a device without hampering a program with similar values and goals as those held by the sponsors. The stability of the local sponsor apparently is of importance for a continuing organization and influences the success of the program. Thus, we conclude that large-scale organizations could avoid many organizational costs by using the device of sponsorship. This device seems to have much poten-

tial for future understanding and development of interorganizational relationships.

It should be pointed out, however, that sponsorship appears to have some inherent weaknesses. That is, this type of organizational device implies restrictions on target populations. In the case of the Boy Scouts of America it is apparent that certain types of boys will not be reached.

Thus, lower-class boys are not equally represented with middle-class boys in the scouting organization. For a number of reasons the Boy Scouts of America has been a middle-class organization, and lower-class boys are more prone to join boys' clubs. In addition, the organization is not keeping up with the increasing numbers of boys of scout age.[8] Perhaps boys living in a more affluent society have other avenues to the same type of activity which the scout organization formerly provided. Also, the increased homogeneity of the sponsors may limit access to memberships. Other reasons undoubtedly exist.

Another of the weaknesses of sponsorship seems to be its inability to accommodate a beneficiary organization to a very pluralistic society. Despite the great potential in sponsoring organizations in the United States, only a limited number can be used by the scouts for their own organizational goals. Partly this is because of the scouts' demand that only organizations with similarities to the scouts be used as sponsors. In addition, there is self-selection by certain types of organizations which want to use the scout program and are therefore more likely to become sponsors; and, finally, certain types of organizations are more likely to survive as sponsors. All of these factors tend to maintain the homogeneity of the sponsors as a whole in the scout organization. In spite of these weaknesses, however, the Boy Scouts of America has utilized the device of sponsorship to its advantage.

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[8] Kunz, *op. cit.*, pp. 98–100.

Sources of Consistency of Political Opinion[1]

Roy T. Bowles and James T. Richardson

ABSTRACT

This article is advanced as a tentative step toward explaining one aspect of opinion organization. The relationship of liberal-conservative consistency of political opinion to ideological conceptualization of political events, interest in politics, and ability to use abstract ideas is examined. An explanatory model is advanced. It hypothesizes that interest in politics and ability to use abstract ideas precede ideological conceptualization, which in turn leads to consistency of opinion. Data obtained by interview from 208 Young Republicans and Young Democrats in a Texas state college provide tentative support for this model, although some modifications are apparently needed. When the three variables are combined in a multiple-prediction formula, error in the prediction of consistency is reduced by 70 per cent.

A number of studies have shown that some people are consistently liberal or consistently conservative in their opinions toward a given combination of issues, while others hold liberal opinions on some issues but conservative opinions on others.[2] These findings raise the question: How do those who are consistent in their political opinions differ from those who are inconsistent?

In this article we will examine the relationship of three independent variables—level of ideological conceptualization, interest in politics, and ability to use abstract ideas[3]—to liberal-conservative consistency of opinion. First, these variables will be examined in the light of an explanatory model; second, they will be treated within a simple statistical-predictive model.

[1] The research reported herein was supported in part by a grant from the Organized Research Funds of Texas Technological College. The authors wish to acknowledge the assistance of Thomas H. Clover, Ronnie Turner, Michael Brooks, and James Henly. They wish to thank Professors Louis N. Gray, Melvin L. DeFleur, and Bruce H. Mayhew for their suggestions. This is a revision of a paper titled "Interests in Politics, Ideological Conceptualization, and Liberal-Conservative Consistency of Opinion" which was presented at the meetings of the Pacific Sociological Association in April, 1967.

[2] Robert Axelrod, "The Structure of Public Opinion on Policy Issues," *Public Opinion Quarterly*, XXXI (Spring, 1967), 49–60; Marvin E. Olsen, "Liberal-Conservative Attitude Crystallization," *Sociological Quarterly*, III (January, 1962), 17–26; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, 1960), 194–98; Willard A. Kerr, "Untangling the Liberal-conservatism Continuum," *Journal of Social Psychology*, XXXV (October, 1952), 111–25; Roy T. Bowles, "The Organization of Individual Political Ideologies," in *Proceedings of the Southwestern Sociological Association* (Dallas: Southwestern Sociological Association, 1965), pp. 158–63; Roy T. Bowles, "The Social Sources of Opinion Consistency" (unpublished Ph.D. dissertation, University of Oregon, 1965).

[3] Another variable which we would expect to relate to consistency of opinion, and which should ultimately be incorporated into a model explaining consistency, is cross-pressures. As applied to the study of opinion consistency, the concept of cross-pressures assumes that the opinions an individual holds toward a given issue are a product of "pressures" in his background or situation. If the pressures an individual experiences on a combination of issues all push him toward liberalism or all push him toward conservatism, we would expect him to be consistent in his opinions. If he is pushed toward liberalism on some issues and toward conservatism on other issues, we would expect him to be inconsistent. Unfortunately, data on cross-pressures are not available in the present study. For a more detailed elaboration of this hypothesis, see Bowles, "The Social Sources of Opinion Consistency," *op. cit.*, and Martin Kriesberg, "Cross-Pressures and Attitudes: A Study of the Influence of Conflicting Propaganda on Opinions Regarding American Soviet Relations," *Public Opinion Quarterly*, XIII (Spring, 1949), 5–16.

MODELS

An ideology may be defined as an abstract frame of reference or a set of general principles. It is a body of cognitive and evaluative propositions about the world. These propositions serve as a "table of equivalences" by means of which events are given meaning and arranged into classes.[4]

Individuals differ in the manner in which they perceive or respond to politically relevant events and objects. Some respond in terms of the immediate impact they perceive these events to have on their concrete personal situation. They associate political events and personal situations without looking for causal processes. For such individuals, the personality characteristics of candidates, broken campaign promises, or the coincidence that a particular party is in power during a war may be more important than general policy orientations or long-range implications of broad programs. Other people interpret events in terms of an abstract frame of reference, that is, a set of general principles or continua. This type of person identifies the position which a political object takes on a continuum within his frame of reference and evaluates it accordingly. He applies his abstract principles in attempting to analyze the ultimate impact of an event. The person who interprets political events in an abstract framework is thinking ideologically, while the person who interprets them concretely is not.

Because the non-ideological person does not see relationships between events, his opinions on one issue are not likely to be connected with his opinions on another. The ideological person is likely to see events as being related to each other; consequently we would expect his opinions on various issues to be interrelated. On the basis of this rationale, we hypothesize that *individuals who conceptualize political events ideologically will be more consistent in their opinions than those who do not.*[5]

Ideological thought involves relating facts and propositions to each other. It also involves applying the implications of general principles to specific situations. Finally, it involves screening and organizing propaganda and information from the environment. The higher one's interest in politics, the more energy he will exert in the performance of these activities.[6] Therefore, we hypothesize that *level of ideological conceptualization will relate positively to interest in politics.*

The authors of *The American Voter* suggest that a political ideology "subsumes content of a wide scope and diversity."

[4] In this discussion of ideologies we are relying heavily on Campbell *et al.*, *op. cit.*, pp. 216-55.

[5] While ideologies could develop around many different types of continua, we will be concerned with the tendency to conceptualize political events in terms of a liberal-conservative continuum. The authors of the *The American Voter* found that few ideologues used other dimensions in viewing political events (*ibid.*, pp. 222-23). Many readers will note the implication of this article for dissonance and balance theories of opinion organization. In general, these theories would predict a tendency for all segments of the population to develop consistent opinions. We are suggesting that, so far as political opinions are concerned, the tendency to develop consistency will be much stronger among those who conceptualize political events in ideological terms than it will among those who do not. It may be possible to explain this phenomenon in terms of dissonance theory by saying that opinions on different political issues are relevant to each other only for those who think ideologically about political events. For a brief review of dissonance and balance theories, see Paul F. Secord and Carl W. Backman, *Social Psychology* (New York: McGraw-Hill Book Co., 1964), pp. 108-19.

[6] For data relevant to this hypothesis see Campbell *et al.*, *op. cit.*, pp. 85-86, 249; Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: University of Chicago Press, 1954), pp. 19-20, 285; and Earl R. Carlson, "Concepts of Man and Attitudes on Social Issues" (paper presented to the convention of the Western Psychological Association, Long Beach, Calif., May, 1966). For theoretical background, see John Dewey, *How We Think* (New York: D. C. Heath & Co., 1933), pp. 10-16; William James, *Pragmatism* (New York: Longmans, Green & Co., 1959), esp. pp. 58-59; Karl Mannheim, *Essays in Sociology and Social Psychology* (New York: Oxford University Press, 1953), pp. 79-119; and Francis X. Sutton, *The American Business Creed* (Cambridge, Mass.: Harvard University Press, 1956), pp. 305-7.

Hence, it "must be capped by concepts of a high order of abstraction."[7] One's ability to place political events in an ideological system will depend upon his ability to use abstract ideas.[8] Therefore, we hypothesize that *ideology will vary directly with ability to use abstract ideas.*

Thus, we are suggesting an explanatory model in which ability to use abstract ideas and interest in politics precede ideological conceptualization of political events; ideological conceptualization of political events is hypothesized to lead to consistency of opinion. These relationships are diagrammed in Figure 1.

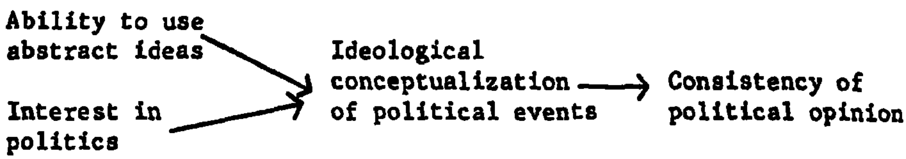


Fig. 1.—Explanatory model

The predictive model does not deal explicitly with the causal relationships between the variables. Instead, it simply asks how much of the variation in consistency of opinion can be predicted or accounted for by variations in the three independent variables.

METHOD

The data to be used in testing these hypotheses were drawn from interviews conducted with a stratified random sample of Young Democrats and Young Republicans at a state college in Texas during the spring of 1965. Interest in politics was measured by three sets of questions. One set asked respondents to identify the positions or offices of ten different state, national, and international figures.[9] Another asked them to provide information about five different state and national issues which were being discussed in the press at the time.[10] The third asked them to indicate the extent of their active involvement in politics.[11] Those respondents who had the most accurate information about political persons and issues and who had been the most involved in political activities were scored highest on interest in politics. Those who had the least information about political persons and issues and who had been least involved in political activities were scored lowest in political interest. The rationale for using this index was the assumption that persons who are interested in politics will accumulate political information[12] and engage in political activities. Statistically significant intercorrelations between the three subparts of the index, and between self-ex-

[7] Campbell *et al.*, *op. cit.*, p. 193.

[8] *Ibid.*, pp. 212, 250–51, 255.

[9] John Tower, Robert Kennedy, Roy Wilkins, Harold Wilson, H. J. "Doc" Blanchard (local representative to the Texas legislature), Nicholas Katzenbach, Ralph Yarborough, Robert Welch, George Mahon, and Maxwell Taylor.

[10] Governor Connally's proposals for reorganizing higher education in Texas, provisions of Medicare, provisions of the public accommodations section of the 1964 Civil Rights Act, provisions of right-to-work laws, and whether Texas had a right-to-work law.

[11] Frequency of attendance at Young Democrat or Young Republican meetings, whether they had actively worked in a political campaign, whether they placed a bumper sticker on their own or someone else's car during the 1964 campaign, frequency with which they talked politics, and self-expressed interest in politics.

[12] This assumption is supported by the following finding: "The highly involved voter draws a much larger sample of the current information flow than does the uninvolved voter" (Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* [New York: John Wiley & Sons, 1966], p. 144). Berelson *et al.* (*op. cit.*, p. 243) show that the higher the interest in politics, the greater the exposure to political campaign material in the mass media. It is probable that factors other than interest (e.g., fam-

pressed political interest and each subpart of the index, lend construct validity to the measure of interest used.

Ideological conceptualization was measured in much the same way that it was by the authors of *The American Voter*.[13] Respondents were asked to state and explain their likes and dislikes about the Democrats, the Republicans, Senator Ralph Yarborough, a liberal, and Senator John Tower, a conservative. The response to each question was coded into one of four levels according to the extent to which it manifested ideological conceptualization.[14] The first level consisted of those responses which made reference to the liberal-conservative continuum. The second level consisted of responses which evaluated political objects in terms of group interest. The third level consisted of evaluations expressed in terms of goodness or badness of the times and relating these, at least nebulously, to political issues. The fourth level consisted of responses which were completely void of issue content. Care was taken to avoid classifying a response at a particular level because of the presence or absence of factual information.

Self-reported grade-point average was taken as a measure of ability to use abstract ideas. Each respondent was asked his specific grade-point average. For purpose of analysis, these responses were divided into two categories—1.00 to 2.49, and 2.50 to 4.00. It was felt that this grouping would dissipate the small amount of bias anticipated in the reporting of grade-point averages.

Consistency of opinion was measured in the following way. Indexes were developed to measure liberal-conservative opinions toward civil rights, labor unions, welfare legislation, and government intervention in the economy.[15] Liberalism and conservatism

ily reading habits, peer-group discussion content) also affect the amount of political information one will accumulate. To the extent that this occurs, our index of political interest is contaminated with other variables.

[13] Campbell *et al.*, *The American Voter*, *op. cit.*, pp. 222–49.

[14] Coding procedures followed those used in *The American Voter* (*ibid.*). The reader wishing more information concerning coding criteria is referred to that source. This coding was completed by two trained coders. As a reliability check, twenty interviews were coded independently by the two coders. The coders agreed on 97.5 per cent (117 of 120) of the coding decisions involving the classification of responses into levels of ideology.

[15] The items used in the opinion indexes were carefully chosen so that index scores would not be affected by the amount of information possessed. For example, if a question required the evaluation of factual information, this information was provided in the statement of the question.

Civil Rights: Do you think civil rights problems should be settled by state and local governments or by the national government? Do you think President Johnson was wrong in using federal troops and the National Guard in protecting the marchers in Alabama? Do you agree or disagree with President Johnson's proposed civil rights bill that guarantees more voting privileges? Do you think that Martin Luther King was deserving of the Nobel peace prize? Do you feel that the civil rights cause is being pushed too rapidly?

Labor unions: Would you like to see labor unions become stronger in the state of Texas? In general, do you agree or disagree with the following statement? The way they are now, labor unions do this country more harm than good. Do you think the working man is better off when he bargains as an individual with the employer for wages and working conditions, or if he bargains with the employer through a union? Do you think that labor unions should be prohibited by law from using union dues to support political candidates? Which do you feel should be the main purpose of the best labor union: to protect the interests of the employees from the employers or to co-operate with management to increase productivity? Do you think that the power of labor unions should be increased, decreased, or remain at the present level?

Welfare: In general, do you disapprove or approve of the President's War on Poverty? In your opinion, is poverty more often the result of lack of initiative or more often the consequence of circumstances beyond the individual's control? Do you think that social security should be put on a voluntary basis or should it remain as it is? The national government presently pays for approximately two-thirds of the cost of urban renewal projects. Do you think this ratio should be increased, decreased, or remain the same? If cities and towns around the country need help to build more schools, the govern-

were defined according to their usage in contemporary American politics. Each individual was scored as either liberal or conservative on each index. The indexes were dichotomized, with the median serving as the division between liberals and conservatives. An individual was scored as consistent in his opinions if he was either conservative on four issues or liberal on four issues. He was scored as inconsistent if he was liberal on two issues and conservative on two. The score of intermediate was assigned to those

TABLE 1

IDEOLOGICAL CONCEPTUALIZATION BY INTEREST IN POLITICS*

Ideology	Interest	
	High	Low
High (%)	80	22
Low (%)	20	78
Total N polled	104	104

* $\gamma = .87$, $P < .001$. Probabilities computed by a test for the significance of γ are described by Linton C. Freeman in *Elementary Applied Statistics* (New York: John Wiley & Sons, 1965), pp. 170–75. Numbers vary from table to table because of non-response to some questions.

who were liberal on three issues and conservative on one, or conservative on three issues and liberal on one.

RESULTS

Data relevant to the explanatory model appear in Tables 1 through 8. In Table 1 we see that interest in politics is related to level of ideology ($\gamma = .87$, $P<.001$). Eighty per cent of those high in interest, but only 22 per cent of those low in interest, are high in ideology. Table 3 shows that the relationship between interest and ideology remains unchanged when ability is introduced as a control. For high ability γ is .85 ($P<.001$); for low ability γ is .87 ($P<.001$). These data support the hypothesis that interest in politics leads to ideological conceptualization of political events.

TABLE 2

IDEOLOGICAL CONCEPTUALIZATION BY ABILITY TO USE ABSTRACT IDEAS*

Ideology	Ability	
	High	Low
High (%)	62	40
Low (%)	38	60
Total N polled	105	102

* $\gamma = .41$, $P < .015$.

Table 2 shows a statistically significant relationship between ability to think abstractly[16] and ideology ($\gamma = .41$, $P<.015$).

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ment in Washington ought to give them money. Do you agree or disagree?

Government intervention: The federal government contributed heavily to the construction of the Canadian River Dam. Do you think that because of this contribution the government in Washington will demand too much voice in running the project? In 1963 the major steel companies said that they were going to increase the price of steel. President Kennedy put pressure on the steel companies and forced them to leave the prices as they were. Do you think President Kennedy used good judgment in this case? Many people feel that economic planning by the national government is an unnecessary intervention in private business. Others feel that economic planning by the government is necessary to maintain economic stability. Which of these groups do you agree with?

[16] There are some limitations in the use of grade-point average as the only indicator of ability to use abstract ideas. Grade-point average reflects motivation and other factors in addition to ability. Different fields of study may require different levels of abstract reasoning. The higher a student's classification, the more training and experience he has had in the use of abstract thought. The categories used for field of study and number of students classifying themselves into each category are: agriculture, 14; home economics, 15; social science, 38; humanities, 27; business administration, 36; engineering, 17; physical or biological science, 12; education, 33; undecided, 16. *For the four fields having more than twenty-five students each, the percentages scoring high on ideology are: social science, 63%; humanities, 63%; education, 46%; business administration, 47%.* There are also some differences between fields in the percentage scoring high on interest and the percentage scoring consistent. While none of these differences is statistically significant, we cannot completely reject the possibility that controlling for major field of study would modify the results presented here.

Student classification also relates to ideology

In Table 3 interest in politics is introduced as a control. The relationship continues to be in the expected direction ($\gamma = .30$ and .37), but it is no longer statistically significant. Part of the original relationship between ability and ideology is accounted for by the fact that respondents with high ability also tend to be high in interest. Thus, the data provide only tentative support for the hypothesis that ability to use abstract ideas is important to level of ideology.

The hypothesis that level of ideology is related to consistency of opinion is supported by the data in Table 4 ($\gamma = .59$, $P.<001$). Fifty-six per cent of those high in ideology, but only 25 per cent of those low in ideology, were consistent in their opinions.

TABLE 3

IDEOLOGICAL CONCEPTUALIZATION BY ABILITY TO USE ABSTRACT IDEAS AND INTEREST IN POLITICS*

Ideology	Ability High		Ability Low	
	Interest		Interest	
	High	Low	High	Low
High (%)	84	30	74	17
Low (%)	16	70	26	83
Total *N* polled	62	43	42	60

* Between interest and ideology: ability high—$\gamma = .85$, $P < .001$; ability low—$\gamma = .87$, $P < .001$. Between ability and ideology: interest high—$\gamma = .30$, $P < .37$; interest low—$\gamma = .37$, $P < .23$.

The data presented thus are consistent with the hypotheses previously advanced. If the processes we have suggested are operating, we would expect additional patterns to appear in the data. Specifically, we would expect interest in politics and ability to use abstract ideas to relate to consistency of opinion, but we would expect these relationships to be reduced when ideology is introduced as a control.

TABLE 4

CONSISTENCY OF OPINION BY IDEOLOGICAL CONCEPTUALIZATION*

Consistency	Ideology	
	High	Low
Consistent (%)	56	25
Intermediate (%)	33	40
Inconsistent (%)	11	35
Total *N* polled	106	102

* $\gamma = .59$, $P < .001$.

TABLE 5

CONSISTENCY OF OPINION BY INTEREST IN POLITICS*

Consistency	Interest	
	High	Low
Consistent (%)	54	26
Intermediate (%)	34	39
Inconsistent (%)	12	35
Total *N* polled	104	104

* $\gamma = .55$, $P < .001$.

The data in Table 5 show that interest in politics is related to consistency of opinion ($\gamma = .55$, $P<.001$). In Table 6, where ideology is introduced as a control, we see that the relationship between interest and consistency disappears among those who are high in ideology ($\gamma = -.02$). This supports the interpretation that interest in politics leads to ideology, which then leads to consistency of opinion. Among those low in ideology, however, the relationship between consistency of opinion and interest in politics increases ($\gamma = .72$, $P<.001$). This suggests that, among persons who do not in-

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($\gamma = .44$, $P<.03$), consistency ($\gamma = .35$, $P<.08$), and interest ($\gamma = .56$, $P<.003$). While these findings might suggest the use of classification as a measure of ability, we are inclined to believe that it is even more contaminated by extraneous variables (e.g., duration of participation in political associations) than grade-point average.

Ultimately, this problem should be resolved by using standardized tests which measure ability more directly, rather than relying on the kinds of indirect indicators available in this study.

were defined according to their usage in contemporary American politics. Each individual was scored as either liberal or conservative on each index. The indexes were dichotomized, with the median serving as the division between liberals and conservatives. An individual was scored as consistent in his opinions if he was either conservative on four issues or liberal on four issues. He was scored as inconsistent if he was liberal on two issues and conservative on two. The score of intermediate was assigned to those

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	High	Low
High (%)	80	22
Low (%)	20	78
Total *N* polled	104	104

* $\gamma = .87$, $P < .001$. Probabilities computed by a test for the significance of γ are described by Linton C. Freeman in *Elementary Applied Statistics* (New York: John Wiley & Sons, 1965), pp. 170–75. Numbers vary from table to table because of non-response to some questions.

who were liberal on three issues and conservative on one, or conservative on three issues and liberal on one.

RESULTS

Data relevant to the explanatory model appear in Tables 1 through 8. In Table 1 we see that interest in politics is related to level of ideology ($\gamma = .87$, $P<.001$). Eighty per cent of those high in interest, but only 22 per cent of those low in interest, are high in ideology. Table 3 shows that the relationship between interest and ideology remains unchanged when ability is introduced as a control. For high ability γ is .85 ($P<.001$); for low ability γ is .87 ($P<.001$). These data support the hypothesis that interest in politics leads to ideological conceptualization of political events.

TABLE 2

IDEOLOGICAL CONCEPTUALIZATION BY ABILITY TO USE ABSTRACT IDEAS*

IDEOLOGY	ABILITY	
	High	Low
High (%)	62	40
Low (%)	38	60
Total *N* polled	105	102

* $\gamma = .41$, $P < .015$.

Table 2 shows a statistically significant relationship between ability to think abstractly[16] and ideology ($\gamma = .41$, $P<.015$).

ment in Washington ought to give them money. Do you agree or disagree?

Government intervention: The federal government contributed heavily to the construction of the Canadian River Dam. Do you think that because of this contribution the government in Washington will demand too much voice in running the project? In 1963 the major steel companies said that they were going to increase the price of steel. President Kennedy put pressure on the steel companies and forced *them to leave the prices as they were. Do you think President Kennedy used good judgment in this case? Many people feel that economic planning by the national government is an unnecessary intervention in private business. Others feel that economic planning by the government is necessary to maintain economic stability. Which of these groups do you agree with?*

[16] There are some limitations in the use of grade-point average as the only indicator of ability to use abstract ideas. Grade-point average reflects motivation and other factors in addition to ability. Different fields of study may require different levels of abstract reasoning. The higher a student's classification, the more training and experience he has had in the use of abstract thought. The categories used for field of study and number of students classifying themselves into each category are: agriculture, 14; business administration, 36; engineering, 17; home economics, 15; social science, 38; humanities, 27; physical or biological science, 12; education, 33; undecided, 16. For the four fields having more than twenty-five students each, the percentages scoring high on ideology are: social science, 63%; humanities, 63%; education, 46%; business administration, 47%. *There are also some differences between fields in the percentage scoring high on interest and the percentage scoring consistent. While none of these differences is statistically significant, we cannot completely reject the possibility that controlling for major field of study would modify the results presented here.*

Student classification also relates to ideology

In Table 3 interest in politics is introduced as a control. The relationship continues to be in the expected direction ($\gamma = .30$ and $.37$), but it is no longer statistically significant. Part of the original relationship between ability and ideology is accounted for by the fact that respondents with high ability also tend to be high in interest. Thus, the data provide only tentative support for the hypothesis that ability to use abstract ideas is important to level of ideology.

The hypothesis that level of ideology is related to consistency of opinion is supported by the data in Table 4 ($\gamma = .59$,

TABLE 3

IDEOLOGICAL CONCEPTUALIZATION BY ABILITY TO USE ABSTRACT IDEAS AND INTEREST IN POLITICS*

Ideology	Ability High		Ability Low	
	Interest		Interest	
	High	Low	High	Low
High (%)	84	30	74	17
Low (%)	16	70	26	83
Total N polled	62	43	42	60

* Between interest and ideology: ability high—$\gamma = .85$, $P < .001$; ability low—$\gamma = .87$, $P < .001$. Between ability and ideology: interest high—$\gamma = .30$, $P < .37$; interest low—$\gamma = .37$, $P < .23$.

$P.<001$). Fifty-six per cent of those high in ideology, but only 25 per cent of those low in ideology, were consistent in their opinions.

The data presented thus are consistent with the hypotheses previously advanced. If the processes we have suggested are operating, we would expect additional patterns to appear in the data. Specifically, we would expect interest in politics and ability to use abstract ideas to relate to consistency of opinion, but we would expect these relationships to be reduced when ideology is introduced as a control.

TABLE 4

CONSISTENCY OF OPINION BY IDEOLOGICAL CONCEPTUALIZATION*

Consistency	Ideology	
	High	Low
Consistent (%)	56	25
Intermediate (%)	33	40
Inconsistent (%)	11	35
Total N polled	106	102

* $\gamma = .59$, $P < .001$.

TABLE 5

CONSISTENCY OF OPINION BY INTEREST IN POLITICS*

Consistency	Interest	
	High	Low
Consistent (%)	54	26
Intermediate (%)	34	39
Inconsistent (%)	12	35
Total N polled	104	104

* $\gamma = .55$, $P < .001$.

The data in Table 5 show that interest in politics is related to consistency of opinion ($\gamma = .55, P<.001$). In Table 6, where ideology is introduced as a control, we see that the relationship between interest and consistency disappears among those who are high in ideology ($\gamma = -.02$). This supports the interpretation that interest in politics leads to ideology, which then leads to consistency of opinion. Among those low in ideology, however, the relationship between consistency of opinion and interest in politics increases ($\gamma = .72$, $P<.001$). This suggests that, among persons who do not in-

($\gamma = .44$, $P<.03$), consistency ($\gamma = .35$, $P<.08$), and interest ($\gamma = .56$, $P<.003$). While these findings might suggest the use of classification as a measure of ability, we are inclined to believe that it is even more contaminated by extraneous variables (e.g., duration of participation in political associations) than grade-point average.

Ultimately, this problem should be resolved by using standardized tests which measure ability more directly, rather than relying on the kinds of indirect indicators available in this study.

terpret political events in ideological terms, interest may operate independently to produce variations in consistency of opinion.

There are two ways in which interest might operate on opinion consistency without passing through ideology. First, the individual who is interested in politics,

TABLE 6

CONSISTENCY OF OPINION BY IDEOLOGICAL CONCEPTUALIZATION AND INTEREST IN POLITICS*

CONSISTENCY	IDEOLOGY HIGH		IDEOLOGY LOW	
	Interest		Interest	
	High	Low	High	Low
Consistent (%)	55	56	52	17
Intermediate (%)	34	30	33	42
Inconsistent (%)	11	13	14	41
Total N polled	83	23	21	81

* Between Interest and Consistency: ideology high—$\gamma = -.021$, $P < .92$; ideology low—$\gamma = .72$, $P < .001$.

compared to one who is not, is more likely to identify himself with either the liberal or the conservative political camp and to learn the constellation of opinions appropriate to that group. He may adopt a pattern of opinions which we have labeled consistent to support his self-image as a liberal or conservative. Or he may learn a consistent pattern of opinions as a result of socialization into group norms. Second, the person who is interested in politics, compared to one who is not, is more likely to discuss political opinions and issues; we would suggest that in this discussion process he is likely to have "unconventional" combinations of opinion pointed out to him.[17]

Table 7 shows a relationship between ability to use abstract ideas and consistency ($\gamma = .29$, $P<.13$). When ideology is used as a control (see Table 8), the relationship is reduced among those who are low in ideology ($\gamma = .12$). This finding is compatible with the interpretation that ability leads to ideology, which in turn leads to consistency. Among those high in ideology, however, the relationship between ability and consistency remains about the same as it was without controls ($\gamma = .30$). This suggests that, among individuals who interpret political events in the light of ideological principles, ability to use abstract ideas may have an independent effect on consistency of opinion.

One possible interpretation of the data in Table 8 is that those who are low in ideology exert little effort in working out a system of political opinions. Among them, therefore, those who are high in the ability to use ideas show very little advantage over those who are low. Those who are high in ideology make an effort to work out a system of political opinions. This effort involves

TABLE 7

CONSISTENCY OF OPINION BY ABILITY TO USE ABSTRACT IDEAS*

CONSISTENCY	ABILITY	
	High	Low
Consistent (%)	48	33
Intermediate (%)	34	38
Inconsistent (%)	18	28
Total N polled	105	102

* $\gamma = .29$, $P < .13$.

relating ideas or events to each other and to general principles. We would expect those high in the ability to use abstract ideas, compared to those low in ability, to be more successful and more thorough in this process. That is, among those high in ideology

[17] For further elaboration of this point see Herbert Hyman and Paul B. Sheatsley, "'The Authoritarian Personality'—A Methodological Critique," in *Studies in the Scope and Method of "The Authoritarian Personality,"* eds. Richard Christie and Maire Jahoda (Glencoe, Ill.: Free Press, 1954), p. 62; Robert Lane, *Political Ideology* (New York: Free Press, 1962), pp. 102–3; Campbell *et al.*, *The American Voters*, *op. cit.*, pp. 144–45; and M. Brewster Smith, Jerome S. Bruner, and Robert W. White, *Opinions and Personality* (New York: John Wiley & Sons, 1965), p. 43.

the ability to use abstract ideas *facilitates* the development of opinion consistency.

It should be noted that interest in politics is a more powerful predictor of both ideological conceptualization and consistency of opinion than is ability to use abstract ideas.

The findings reported above provide tentative support for the explanation advanced. However, interest in politics and ability to use abstract ideas each show some independent relationship to consistency of opinion. This suggests the desirability of determining how much of the variation in consistency the three variables taken together can predict. The most appropriate technique for such an analysis is multiple correlation. Inasmuch as the measures being used here are only ordinal scales, however, standard multiple correlation techniques are not applicable. The statistic koppa (ϱ)[18] can be used to compute a multiple correlation with ordinal data.[19]

Since ϱ was computed on two-by-two tables, the intermediate and inconsistent categories of the dependent variable were combined to form one category. In the formula for multiple ϱ presented below, c = consistency of opinion, i = ideology, t = interest in politics, and a = ability to use abstract ideas:

$$\varrho\, c \cdot ita = \frac{(135 + 21 + 20) - 31}{207} = .70 \,.$$

A ϱ of .70 means that error in the prediction of opinion consistency is reduced by 70 per cent with knowledge of values on ideology, interest in politics, and ability to use ideas. Eighty-five per cent of the cases (135 + 21 + 20)/207 are predicted accurately by the three independent variables. This finding indicates that, whether or not the explanatory model advanced above is correct in detail, the independent variables used are highly predictive and must be considered in any attempt to explain liberal-conservative consistency of political opinion.

TABLE 8

CONSISTENCY OF OPINION BY IDEOLOGICAL CONCEPTUALIZATION AND ABILITY TO USE ABSTRACT IDEAS*

Consistency	Ideology High		Ideology Low	
	Ability High		Ability Low	
	High	Low	High	Low
Consistent (%).....	60	49	28	23
Intermediate (%)....	31	37	40	39
Inconsistent (%)....	9	15	33	38
Total N polled.....	65	41	40	61

* Between ability and consistency: ideology high—γ = .30, P < .23; ideology low—γ = .12, P < .92.

SUMMARY AND CONCLUSIONS

We have advanced an explanation of variations in liberal-conservative consistency of political opinion involving the following hypotheses. Interest in politics and ability to use abstract ideas lead to ideological conceptualization of political events. Ideological conceptualization leads to consistency of opinion. Data presented show tentative support for the model.[20]

18 William H. Kruskal, "Ordinal Measures of Association," *Journal of the American Statistical Association*, LIII (December, 1958), 818–22.

19 The following explanation of the use of koppa in multiple correlation was prepared by Professor Louis N. Gray of Washington State University. A multiple ϱ may be defined whenever all cases of a dependent variable may be classified as either correct or incorrect according to some hypothesis. This may be accomplished by determining the number of cases of the dependent variable which may be correctly predicted by each of the independent variables, taken in order, when those cases correctly predicted by previously applied independent variables have been deleted from the analysis. Koppa may then be calculated:

$$\varrho = \frac{\text{No. cases predicted correctly minus No. cases predicted incorrectly}}{\text{Total no. cases}} \,.$$

20 An obvious limitation of the present data is its inability to demonstrate time order between variables.

However, interest in politics and ability to use abstract ideas show some independent relationship to consistency of opinion. A multiple reduction of error prediction (using ρ), with interest, ability, and ideology as independent variables and consistency as the dependent variable, demonstrates the importance of these three variables in predicting liberal-conservative consistency of opinion.

The explanatory model presented perhaps needs some modifications. The processes by which interest in politics and ability to use abstract ideas independently affect consistency of opinion need to be elaborated and tested. It may be necessary to incorporate other variables[21] into the model. Alternative solutions may fit the data.[22] We believe, however, that in spite of the somewhat selective characteristics of the subjects and issues used, the arguments and data presented here constitute an important tentative step toward an explanation of one aspect of opinion organization.

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[21] Esp. cross-pressures (see n. 2 above).

[22] E.g., during the socialization process individuals may learn to express a certain amount of interest in politics, to conceptualize political events in a certain way, and to hold a given combination of opinions. Thus, the relationship observed could result from a common dependence of the variables we have discussed on some other variable.

Ethnic and Racial Segregation in the New York Metropolis, 1960[1]

Nathan Kantrowitz

ABSTRACT

A review of previous research leads to the conclusion that there has been only a minimal decline in interethnic segregation (e.g., Irish from Italian immigrants) in U.S. cities since 1930. Moreover, an analysis of the New York–Northwestern New Jersey Standard Consolidated Area census tract statistics indicates that interethnic segregation remains relatively high into the second generation. This suggests that white resistance to racial integration may but compound the strong separatism of ethnic populations from one another.

It appears to be both a fact of city life and a common indicator of cultural differences that people with similar traits tend to live together, apart from other groups, whether in a "Chinatown" or "on the better side of the tracks." In the modern American metropolis, ethnic and racial neighborhoods can be analytically abstracted from those with other bases of separation, such as rich-poor or Protestant-Catholic. In this mélange, such ethnic segregation as the separation of Catholic Irish from Catholic Italians is usually seen as succumbing to a process of assimilation, as the enclaves of immigrant nationalities are replaced by religious and racial neighborhoods distributed along an economic axis from central-city slums to affluent suburbs.[2] Moreover, it would seem to be a reasonable extension of this idea to conclude that, since the multiplicity of ethnic neighborhoods is disappearing, opposition to Negro integration is almost entirely racial. We do not agree. We think voluntary ethnic segregation is still a viable force.

Our own study of New York in 1960, and our examination of research done for other cities has led us to believe that, although ethnic segregation has indeed weakened, the reports of its demise are exaggerated. Indeed, even among those white ethnicities that are similar to one another—such as the Protestant Swedes and Norwegians—we judge that ethnic separatism remains fairly high into the second generation. This conclusion has pessimistic implications for the problem of Negro desegregation. Especially in the central city, where Negroes are most concentrated and ethnicities are most distinct, we suspect that the major opposition to Negro dispersion tends to come, not from whites in general, but from specific white ethnic populations. As a consequence, the cause of racial desegregation must bear the burden, not only of an often noted racial prejudice, but also of a usually overlooked ethnic separatism.

In support of the contention that the segregation of European ethnic populations

[1] Revision of a paper prepared for discussion at the Columbia University Seminar on Population and Social Change, December, 1967. This research was supported by the Columbia University School of Social Work–Mobilization for Youth Research Project (Richard A. Cloward, director), under a grant from the National Institute of Mental Health. I wish to thank many people for their assistance, particularly John Beresford, Patricia Golden, and members of the staff of the U.S. Bureau of the Census, who programmed and calculated the segregation index numbers; and Nathan Glazer, Stanley Lieberson, Donnell M. Pappenfort, and members of the Population Seminar.

[2] Noel P. Gist and Silvia F. Fava, *Urban Society* (New York: Thomas Y. Crowell Co., 1964); Scott Greer, *The Emerging City* (New York: Free Press, 1962). Nathan Glazer and Daniel P. Moyhihan, "Introduction," *Beyond the Melting Pot* (Cambridge, Mass.: M.I.T. Press, 1963). Much of the literature on ethnicity and assimilation focuses on such factors as voting or intermarriage. In large measure, our conclusions are an extension of these ideas to residential segregation.

remains strong, we can present only limited new evidence. Even though this is the first intensive study of ethnic and racial segregation in the nation's largest metropolis, and one of the few such studies of any metropolis to include the suburbs as well as the central city, it is still but a single case study. Moreover, the data and methods are limited to ethnic (foreign stock and Puerto Rican) and racial (Negro) segregation index numbers, calculated from the census-tract statistics of the 1960 New York–Northeastern New Jersey Consolidated Statistical Area.[3] So that we may reach a conclusion with this limited amount of original evidence, our argument will splice together several parts of our own and others' findings. In order to connect the Consolidated Area of 1960 with the recent past for other American cities, we shall first evaluate in detail the evidence for a decline in ethnic segregation in these other cities, and then, in order to create a parallel, we shall show how New York's segregation patterns resemble those of Chicago.

Having indicated why we consider that New York is similar to other cities, where the decline of ethnic segregation has been minimal, we can focus on this metropolis, presenting new data from the most recent census, which includes both suburb and city. But before analyzing segregation in New York, we must dispose of the argument that our results are trivial because they are based on only immigrant populations which constitute a small proportion of the total population of the metropolis. We shall show, on the contrary, that, because the census classification "foreign stock" is so heavily weighted by the second generation, our 1960 indexes measure more than just immigrant enclaves; in fact, the data include the great majority of the area's population. Finally, we will discuss the metropolis and its component cities and suburbs, to assess their levels of interethnic segregation and the extent to which Negro segregation is but a racial reinforcement of existing ethnic separatism.

As a prefatory methodological note to our argument, it should be mentioned that the segregation index number we have used, the Index of Dissimilarity (or delta), has been closely studied and extensively applied by earlier researchers.[4] However, a major problem in its evaluation is the matter of determining what values of delta between 0 (no segregation) and 100 (total segregation) constitute "high" and "low" segregation, and what magnitude of numerical change can be taken as indication of an important change in segregation level. In theory, there is no answer to this problem, but in practice we can resolve it by considering deltas upward of 70 (which usually separates Negroes from

[3] Census-tract statistics for 1960, published in the U.S. Bureau of the Census, *U.S. Censuses of Population and Housing: 1960 Census Tracts* (PHC [1] series), were obtained for this study from the census bureau's computer tract tally tapes 29I and 29B. The seventeen counties of the Consolidated Area are defined and outlined in U.S. Bureau of the Census, *1960 Census of Population: 1960*, Vol. I: *Characteristics of the Population*, Parts 32 (New Jersey) and 34 (New York), Tables 11, 11a. The census bureau's definition of "foreign stock" and "Puerto Rican" is based on the respondent's place of birth (or his parents' birthplaces). Consequently, it does not differentiate ethnicity among those whose parents are native-born. The European ethnicities presented here include all those tabulated by the census bureau for the Consolidated Area. For definitions of race and of European or Puerto Rican ethnicity, see U.S. Bureau of the Census, *U.S. Census of Population, 1960*. Vol. I: *Characteristics of the Population*, Part 1, U.S. Summary, "Introduction."

[4] A major study comparing delta with other segregation indexes is Otis Dudley Duncan and Beverly Duncan, "A Methodological Analysis of Segregation Indexes," *American Sociological Review*, xx (April, 1955), 210–27; for more recent applications of delta which contain theoretical discussions and guides to the literature, see Stanley Lieberson, *Ethnic Patterns in American Cities* (New York: Free Press, 1963), chap. ii; and Karl E. Taeuber and Alma F. Taeuber, *Negroes in Cities* (Chicago: Aldine Publishing Co., 1965), Appendix A. Some scholars have other methodological preferences. See, for example, James M. Beshers, Edward O. Laumann, and Benjamin S. Bradshaw, "Ethnic Congregation-Segregation, Assimilation, and Stratification," *Social Forces*, XLII (May, 1964), 482–89. Any segregation index number is, of course, a limited statistical summary device, which is useful for analysis. It reflects "real segregation" no more than the I.Q. reflects "real intelligence" or the U.S. Bureau of Labor Statistics' Consumer Price Index reflects the "real cost of living."

whites) as "high," deltas of 30 or less as "low," and variations in level of less than five points as unimportant, unless they are otherwise correlated.[5]

Returning to our argument, our first step is to reassess the evidence for a decline in ethnic segregation. Our central concern is a comparison which emphasizes the segregation of specific ethnicities from one another, such as Irish from Italian. The major relevant study is Lieberson's *Ethnic Patterns in American Cities*, an analysis of segregation from 1910 to 1950 in ten northeastern and midwestern central cities. Lieberson found convincing evidence of assimilation among the foreign-born; the segregation of immigrant groups from one another as well as from all native whites consistently declined as their socioeconomic status rose. From his larger study, we should like to discuss one small part, the segregation of foreign-born whites by country of birth in city wards between 1910 and 1920 and in census tracts between 1930 and 1950.

For each of his ten cities, Lieberson calculated the segregation index of each ethnic group from each of the others, so that he derived perhaps fifty deltas per city for such comparisons as Irish with Italian, German with Irish, and Italian with Russian. There was a small decline in the (equally weighted or, as we shall call them, unweighted) average of the deltas, city by city. For example, Cleveland's average decreased, by wards, from 50.6 in 1910 to 43.5 in 1920; by census tracts, from 60.8 in 1930 to 54.3 in 1950.[6] In the remaining nine cities, the declines were similarly consistent and small. This decline of approximately five points between 1930 and 1950 in the average of segregation indexes is not very impressive when one realizes that it was wrought by depression, war, the maturation of a new generation, and a postwar expansion of settled areas within and beyond the cities' legal boundaries.

The decline in average delta continued to be consistent but small when Lieberson made the common distinction between those of the foreign-born who came from northern Europe—the "old" immigrants, whose greatest immigration wave preceded the final quarter of the nineteenth century—and those from southern and central Europe—the "new" immigrants, whose greatest immigration wave was more recent. If we again take Cleveland as an example, the average segregation index among northern European ethnicities fell from 35.5 to 30.3 between 1930 and 1950; between northern and southern Europeans it fell from 59.8 to 53.9; and among southern Europeans, from 65.4 to 58.1. If the segregation indexes themselves reflect real differences between, for example, Irish and Italians, then these consistent but small declines indicate the existence of relatively stable immigrant enclaves.[7]

The data shown in Table 1 permit us to draw a parallel between foreign-born segregation in the ten central cities in 1950, and foreign-stock segregation in the New York metropolis in 1960. Although the data are based on different years, geographical units, and definitions of ethnicity, so that it is

[5] These cutting points reflect my judgment as to how delta has been interpreted in the literature. Moreover, the data lend plausibility to this decision. For example, consider the segregation index numbers for the Consolidated Area in Table 6 (below). The deltas range from 21.4 to 70.7, with a median of 45.8 (as compared with our interpretation of 30 as "low" and 70 as "high"); the median has a semi-interquartile range of 6.6 (as compared with our five-point "unimportant" variability).

[6] Lieberson, *op. cit.*, Table 9, p. 57. Lieberson points out (pp. 58–59) that the various deltas for a given city in any one year are not independent of one another. The larger size of Cleveland's average delta calculated by census tracts in 1930 (60.8) over that calculated by ward boundaries in 1920 (43.5) is an artifact of the statistical characteristics of delta. In general, the use of a smaller geographic unit results in a larger delta. Since census tracts are smaller than wards, deltas calculated from census-tract statistics are usually larger than deltas calculated from ward statistics.

[7] *Ibid.*, Table 22, pp. 84–85. A similar conclusion can be drawn from the segregation of the foreign-born from native whites (see Lieberson, Table 4, p. 46, and Table 13, pp. 66–67). We are emphasizing a point already recognized by Lieberson; see his comment on "dynamic equilibrium" (p. 46).

impossible to draw fine conclusions, we can see one broad similarity: New York's averages resemble those of other places. Turning again to Cleveland as an example, we see that its over-all mean of 54.3 is higher than New York's 46.4. But once we examine northern and southern European ethnicities, the advantage vanishes: Cleveland's "old versus old" average of 30.3 is lower than New York's 40.5, its "new versus new" average of 58.1 is higher than New York's 40.8, and the 53.9 index for "old versus new" approximates New York's 51.2. All in all, then, it is apparent that, when we consider Lieberson's ten northeastern and midwestern central cities, the decline in foreign-born segregation, although real, is too small to be interpreted as a destruction of ethnic enclaves. Moreover, the 1950 foreign-born levels bear at least a broad resemblance to the 1960 foreign-stock levels of the New York metropolis.

TABLE 1

UNWEIGHTED AVERAGES OF INTERETHNIC SEGREGATION INDEXES AMONG THE FOREIGN-BORN WHITES OF TEN CENTRAL CITIES IN 1950* AND AMONG PERSONS OF FOREIGN STOCK IN THE NEW YORK-NORTHEASTERN NEW JERSEY STANDARD CONSOLIDATED AREA IN 1960†

Place	Total	Old vs. Old	Old vs. New	New vs. New
	Foreign-born White			
Central cities, 1950:*				
Boston	50.6	28.5	55.0	61.5
Buffalo	52.0	33.2	55.4	62.3
Chicago	50.1	34.0	52.1	53.3
Cincinnati	48.4	35.8	51.3	51.0
Cleveland	54.3	30.3	53.9	58.1
Columbus	49.3	36.3	49.7	52.4
Philadelphia	53.0	32.9	56.9	53.4
Pittsburgh	51.6	32.0	53.4	54.8
St. Louis	49.9	35.6	48.5	56.0
Syracuse	53.8	39.6	56.5	57.0
	Foreign Stock			
New York Consolidated Area, 1960†	46.4	40.5	51.2	40.8

* Stanley Lieberson, *Ethnic Patterns in American Cities* (New York: Free Press, 1963), Tables 9 and 22.

† From Table 5 (of this paper), second column, last three rows.

Lieberson's comparative study was made before 1960 census statistics were available. The only more recent report is a case study of changes from 1930 to 1960 in Chicago city segregation between individual ethnic foreign stocks (immigrants and their children combined)[8] and the totality of all native whites of native parents. This, of course, differs from the interethnic segregation we have just considered in our discussion of Lieberson's work, for the Taeubers compare an ethnicity with an average baseline. Thus, if we compare, say, Irish foreign stock against a base line of native whites of native parents, we are comparing a single ethnicity against an average base line composed of those whose immigrant forebears were no more recent than their grandparents, an average base line which is

[8] Karl E. Taeuber and Alma F. Taeuber, "The Negro as an Immigrant Group: Recent Trends in Racial and Ethnic Segregation in Chicago," *American Journal of Sociology*, LXIX (January, 1964), 374–82.

itself weighted to an unknown extent by a changing mix of grandchildren of immigrants from different countries—including, of course, the grandchildren of Irish immigrants. Obviously, such a comparison does not measure changes in interethnic segregation, as between, for example, Irish and Italians.[9]

The Taeubers' purpose was to demonstrate the decline in ethnic segregation in contrast to the high, and stable, Negro-white segregation figure. As we did with Lieberson's work, we wish to focus on a small part of these data in a different context. If we consider the "old versus new" dichotomy, the data document the 1930–60 changes: among ethnicities from northern Europe, levels of segregation remained stable and low, while among southern Europeans, segregation levels, originally higher, declined precipitously.[10]

Comparison of our 1960 data for the New York metropolis and data for the city of Chicago (Table 2) indicates an over-all congruence. For example, in 1960, Chicago's delta between Negroes and native whites born of native parents is 82; in the Consolidated Area, between Negroes and non-Puerto Rican whites born of native parents, it is 80.2. Moreover, we find a correlation of .87 between Chicago and New York when we rank each of twelve ethnicities and races in their segregation from the base population.[11] We think it reasonable to conclude that neither Chicago nor any other city differs substantially from New York in the pattern of ethnic segregation.

Now that we have demonstrated that ethnic segregation in general has declined very little, and that ethnic segregation in New York resembles that of other cities, we can focus on just the Consolidated Area of 1960. We wish to show that we are not considering just some quaint immigrant fragments of a bygone past, now scattered through a much larger population. First, we should emphasize that the 1960 census data on the foreign stock are weighted not so much by the immigrants themselves as by the second generation. Table 3 shows that the percentage of foreign-born ranges from 27.3 for the Irish to 43.4 for the Norwegians. On the average, only about a third of any ethnic population are themselves

[9] We cannot give precise reasons for the differences between segregation index numbers calculated between individual ethnicities (such as Irish and Italian) or between an ethnicity and a base line (such as Irish immigrants and all native whites). The latter is more sensitive to changes in the city as a whole. For example, a case study of Chicago has concluded, from map inspection, that the dispersion of its Polish and Italian communities to approximate more closely the average base line during 1898–1920 in large part resulted from the construction of new housing within the city after 1899 (see David Wallace, "Residential Concentration of Negroes in Chicago" [unpublished Ph.D. dissertation, Harvard University, 1953], pp. 197–210).

[10] Taeuber and Taeuber, "The Negro as an Immigrant Group," *op. cit.*, Table 1, pp. 376–77. We concur with the Taeubers' observation that ethnicities have become less segregated over time. Our purpose is to highlight the fact that segregation between specific ethnicities differs from segregation of a specific ethnicity from a base-line population. The latter is a weighted average, so that, for example, all native whites are weighted differently in 1960 than in 1930.

[11] Coefficient of Spearman rank correlation. Within this broad pattern of similarity, some differences can be noted. For example, the slight but consistently higher level of segregation in the Consolidated Area as compared with Chicago city probably stems from the use of different spatial units: the Consolidated Area is divided into more than 4,000 census tracts, the city of Chicago into but seventy-five "community areas." Other variations which probably derive from the changing mix of the base line populations are the slightly lower indexes for entire Standard Metropolitan Statistical Areas (SMSA) than for just their central cities and the slightly higher indexes for the New York SMSA's than for the New Jersey ones. The higher segregation of Negroes from every other population in New York (as well as in nearly every other study of American cities) is so constant throughout every part of the metropolis that we need not point it out repeatedly. We shall not discuss the segregation of Puerto Ricans here because we have described their adjustment in the following: "Socio-Economic Segregation Among the Nonwhite, Puerto Rican, and Non-Puerto Rican White Populations, New York, 1960," a paper presented at the annual meeting of the Population Association of American, 1968; and "Social Mobility of Puerto Ricans: Education, Occupation, and Income Changes Among Children of Migrants, New York, 1950–1960," *International Migration Review*, II (Spring, 1968), 52–72.

immigrants. Moreover, both generations are old enough to consist of adults living in their own homes. We do not know the age distributions for the Consolidated Area as a whole, but we do know it for each state. In New York, for example, the median age for the foreign-born was 57.3 years; for the native-born of foreign parentage, it was 43.7; and for the native-born of mixed native and foreign parentage, it was 31.0.[12] In effect, we can consider our ethnic population to be primarily the children of immigrants who left their parental homes and began their own families.

Moreover, the ethnicities usually number large enough populations to fill neighborhoods, although individual foreign stocks range considerably in size. As Table 3 indicates, except for the Swedes, who constitute only 0.4 per cent of the Consolidated Area's total population, and the Norwegians, who constitute 0.5 per cent, all others exceed 100,000: they range from the Czechs, who constitute 0.9 per cent, to the Italians, who constitute 10.4 per cent.

[12] U.S. Bureau of the Census, *U.S. Census of Population: 1960*, Subject Reports: *Nativity and Parentage*, Final Report (PC [2]-1A), Table 15, p. 103.

TABLE 2

INDEXES OF RESIDENTIAL SEGREGATION BY RACE AND BY SELECTED ETHNICITIES (FOREIGN STOCK) FROM A BASE POPULATION OF NATIVE WHITES OF NATIVE PARENTS,* NEW YORK–NORTHEASTERN NEW JERSEY STANDARD CONSOLIDATED AREA (CA) FOR 1960, CITY OF CHICAGO FOR 1930–60†

Ethnicity and Race	Consolidated Area (CA), 1960							City of Chicago, 1930–60		
	Total CA	New York Part (New York SMSA)		New Jersey Part				1930	1960	Change
				Newark SMSA		P-C-P‡ SMSA	JC§ SMSA			
		Total	City Only	Total	City Only					
"Old":										
United Kingdom	21.2	22.1	25.0	16.2	20.9	15.3	32.9	11‖	18	+7
Ireland (Eire)	37.4	38.0	37.1	27.9	28.1	20.4	21.4	23	31	+8
Norway	48.2	50.2	62.5	42.6	72.5	33.8	39.1	44	37	−7
Sweden	34.1	35.0	41.6	31.3	53.2	27.4	38.2	26	30	+4
Germany	26.7	28.3	31.9	20.6	26.0	16.5	30.4	22	19	−3
"New":										
Poland	42.5	44.3	42.5	38.3	39.6	40.0	34.9	63	38	−25
Czechoslovakia	41.6	42.8	43.5	39.4	41.0	40.5	40.3	59	37	−22
Austria	40.4	41.2	38.2	34.0	42.8	30.9	25.2	30	16	−14
Hungary	42.7	42.8	40.8	32.0	36.3	38.0	29.2	#	#	
U.S.S.R.	51.9	50.7	47.5	45.4	54.0	39.0	34.2	51	44	−7
Italy	37.7	38.7	39.2	37.2	33.4	31.6	23.9	52	32	−20
Negro†	80.2	81.8	84.4	75.5	64.5	79.1	72.1	84	82	−2
Puerto Rican	82.2	81.6	78.5	75.6	59.6	69.2	68.1	#	67	.....
White†:										
Total	12.9	14.2	13.2	7.9	8.7	7.4	5.4	#	#	.....
Native of foreign or mixed parentage	19.8	20.5	17.1	16.7	13.4	14.0	8.5	#	#	.....
Foreign-born	35.8	36.0	29.5	25.8	23.0	24.6	18.2	#	#	.....

* Non-Puerto Rican. Data are from Table 5, below, second column, last three rows.

† From Taeuber and Taeuber, "The Negro as an Immigrant Group," *op. cit.*, Table 1.

‡ Paterson-Clifton-Passaic.

§ Jersey City.

‖ England and Wales.

\# Not available.

A final clarification emerges from Table 4, where we see that the census data for just interethnic segregation include a significant part of the population of the metropolis. The excluded population—non-Puerto Rican, native-born whites with two native-born parents—ranges from a high of 52.5 per cent in the Paterson-Clifton-Passaic SMSA to a low of 36.2 per cent in New York. As we go from the SMSA to the inner city, the excluded population drops still more; for example, it is 28.8 per cent in New York City, and even lower in such residential areas as the Bronx (26.7 per cent). Moreover, this omitted population tends to contain a large proportion of children and very young adults. For example, we estimate that in New York City 40 per cent of our excluded population is under 15 years of age. If we assume that all these youngsters live with their parents, so that their residential distribution would not affect the housing pattern, and that many of the parents themselves are second generation, then our ethnic and racial segregation indexes should include at least 75 per cent of New York City's population. In summary, we can reasonably conclude that the Consolidated Area's 1960 segregation patterns are not unique to a minor part of a fragmented and minuscule immigrant population. Rather, New York's segregation pattern is similar to that of other cities which have had relatively stable

TABLE 3

PERCENTAGE OF FOREIGN-BORN AMONG SELECTED FOREIGN STOCK, NEW YORK–NORTHEASTERN NEW JERSEY STANDARD CONSOLIDATED AREA, 1960*

Ethnicities	Total Foreign Stock (in Thousands)	Foreign-born as a Percentage of Total
Norway	68	43.4%
Germany	654	40.2
Hungary	183	40.0
Czechoslovakia	128	36.9
United Kingdom	413	36.3
Poland	668	35.9
Sweden	64	34.1
Austria	342	33.1
U.S.S.R.	787	32.5
Italy	1,531	29.6
Eire	504	27.3

* U.S. Bureau of the Census, *U.S. Census of Population*, Vol. I: *Characteristics of the Population*, Part 34 (New York, U.S. Government Printing Office, Washington, D.C., 1963), Table 99.

TABLE 4

PERCENTAGE DISTRIBUTION OF POPULATION BY RACE AND NATIVITY, NEW YORK–NORTHEASTERN NEW JERSEY STANDARD CONSOLIDATED AREA AND COMPONENT SMSA'S, 1960*

	Consolidated Area	New York SMSA		Newark SMSA		Paterson-Clifton-Passaic SMSA	Jersey City SMSA
		Total	City	Total	City		
Total population (in thousands)	14,759	10,695	7,782	1,689	405	1,187	611
Race and nativity:							
Negro†	10.6%	11.5%	14.0%	13.3%	34.1%	3.6%	6.8%
Other nonwhite†	0.5	0.6	0.7	0.2	0.3	0.1	0 2
Puerto Rican	4.6	5.9	7.9	0.8	2.4	0.8	2.4
Non-Puerto Rican whites:							
Native-born of native parents	40.4	36.2	28.8	49.5	29.3	52.5	45.5
Foreign stock	43.9	45.8	48.6	36.2	33.9	43.0	45.1
Total	84.3	82.0	77.4	85.7	63.2	95.5	90.6
Total	100.0%	100.0%	100.0%	100.0%	100 0%	100 0%	100.0%

* 1960 census of population (Nathan Kantrowitz and Donnell M. Pappenfort, "1960 Fact Book for the New York–Northeastern New Jersey Standard Consolidated Area: The Non-White, Puerto Rican, and White Non-Puerto Rican Populations," *Social Statistics for Metropolitan New York*, No. 2 [March, 1966]; U.S. Bureau of the Census, *U.S. Census of Population: 1960*, Vol. I, Parts 32, 34, Tables 72, 79).

† Includes nonwhite Puerto Ricans.

ethnic segregation; moreover, an analysis of New York includes the great majority of the residents of that metropolis.

Turning to the segregation index numbers of the Consolidated Area, we want to show, first, that the deltas for the individual SMSA's resemble one another closely enough so that the total for the Consolidated Area summarizes characteristics of the metropolis as a whole. Table 5 indicates make the point that interethnic segregation can be regarded as relatively high.

We see here that the European ethnic populations are almost equally segregated from Negroes, whether we consider "old" stock (82.0) or "new" (80.1). Moreover, an equivalent ethnic segregation appears to apply to the Puerto Ricans, again whether we consider their segregation from the "old" (82.4) or "new" (78.3) European stock.

TABLE 5

UNWEIGHTED MEAN INDEXES OF SEGREGATION FOR NEGROES, PUERTO RICANS, AND FOREIGN STOCK, NEW YORK–NORTHEASTERN NEW JERSEY CONSOLIDATED AREA AND COMPONENT SMSA'S, 1960

Populations Compared	Number of Comparisons	Total CA	New York		Newark		Paterson-Clifton-Passaic SMSA	Jersey City SMSA
			SMSA Total	City Only	SMSA Total	City Only		
Negro × Puerto Rican....	1	66.0	63.8	62.2	55.8	48.7	54.9	69.4
Negro × foreign stock:								
Negro vs. all foreign stock..............	11	81.0	81.7	83.7	76.9	71.6	79.8	75.2
Negro vs. old stock.....	5	82.0	82.7	84.8	78.8	72.2	81.2	79.2
Negro vs. new stock....	6	80.1	80.9	82.8	75.4	71.2	78.7	71.8
Puerto Rican × foreign stock:								
P.R. vs. all foreign stock	11	80.2	79.2	77.9	77.5	70.3	68.9	72.8
P.R. vs. old stock......	5	82.4	81.6	80.0	80.5	71.3	73.1	74.8
P.R. vs. new stock......	6	78.3	77.2	76.1	75.1	69.5	65.3	71.1
Interethnic foreign stock:								
Total..............	55	46.4	46.2	49.3	42.6	53.3	38.4	42.7
Old vs. old...........	10	40.5	41.1	46.2	37.6	51.9	27.5	43.4
New vs. new..........	15	40.8	38.9	40.3	38.4	46.0	34.1	38.0
Old vs. new...........	30	51.2	51.6	54.9	46.4	57.4	44.2	44.9

that differences among the four SMSA's are relatively minor. The largest range of differences—15.9 points, between the Jersey City and the Paterson-Clifton-Passaic SMSA's for the "old versus old" interethnic foreign-stock average—is not even approached by any of the other ranges. More typical is the 9.1 difference between the total New York and Jersey City SMSA's for the "Negro versus new" stock comparison. Generally, the SMSA's vary by only seven or eight points, in no particular order, except that New York's tend to be higher than New Jersey's.[13] Consequently, we shall refer to the average for the total Consolidated Area, shown in the second column of Table 5, to It is only speculation, but we think that the appreciably lower (66.0) segregation between Negroes and Puerto Ricans stems in

[13] There are only small differences between the average deltas of the total Newark or New York SMSA's and their central cities; we guess that some of this stems from continued ethnic separatism in the suburbs. We suspect also that work-residence patterns may affect segregation levels. We have seen that the New York SMSA has higher levels; the relatively minor variations among New Jersey SMSA's have been ignored because commuter-stream analysis indicates that these SMSA's are segments of a large but unfocused urban tangle which is interwoven with a highly centralized New York SMSA. (See Nathan Kantrowitz, "The Organization of Commuter Flows, New York, 1960" [The Research Center, Columbia University School of Social Work, 1968]).

large measure from the recent date of both their migrations: the greatest number of European migrants came prior to World War I; most Negro migrants came since World War I; and most Puerto Ricans, since World War II.

But whatever reasons we posit for the very large segregations between European ethnicities and Negroes or Puerto Ricans, we assume that a guide to the lower bounds of these segregations will be found in the segregation between European ethnicities. We can see in Table 5 that the level of average segregation among northern Europeans in the Consolidated Area (40.5) is virtually identical with that among southern Europeans (40.8). But the average segregation between northern and southern Europeans is markedly higher (51.2). We think it reasonable to consider this average delta of 51.2 as the lower bound for 1960 segregation of foreign stock from Negroes or Puerto Ricans.[14] In effect, nearly forty years after the end of large-scale European migration, a segregation index number encompassing both the migrants and their children (but primarily the children) in a highly suburbanized metropolis indicates that, on the average, 51.2 per cent of the population of southern European origin would have to be redistributed in order to achieve full integration with the northern European population. Consequently, we expect that any given Negro-white segregation index number will exceed this base. How much of a differential it is reasonable to "expect," is, of course, a subjective matter. But whatever judgment one makes, we think it likely that any reduction of racial segregation will involve the reduction, not of just Negro-white segregation, but rather of Negro-white European ethnic segregation—a task made difficult by the high level of interethnic segregation.

To enable us to focus more closely on just this interethnic segregation, Table 6 presents the indexes for eleven selected ethnicities, plus Negroes and Puerto Ricans. It is instructive to consider two ethnicities which together comprise but 0.9 per cent of the population. The segregation index between the Norwegians and Swedes, 45.4, indicates a separation between two Protestant Scan-

TABLE 6

INDEXES OF RESIDENTIAL SEGREGATION IN 1960, BETWEEN SELECTED ETHNICITIES (FOREIGN STOCK), TOTAL NEW YORK–NORTHEASTERN NEW JERSEY STANDARD CONSOLIDATED AREA (ABOVE THE DIAGONAL), NEW YORK STANDARD METROPOLITAN STATISTICAL AREA (BELOW THE DIAGONAL)*

Ethnic Population	1	2	3	4	5	6	7	8	9	10	11	12	13
1. United Kingdom	.....	30.9	49.7	31.6	25.9	44.9	42.3	40.6	42.5	51.6	43.3	79.8	81.4
2. Eire	28.1	.....	56.1	41.8	33.8	50.2	47.5	45.5	47.4	55.4	45.5	79.0	76.9
3. Norway	51.4	58.7	.....	45.4	52.4	65.9	64.1	64.5	66.4	70.7	58.9	87.7	88.2
4. Sweden	31.8	41.3	45.8	.....	37.4	56.9	52.2	52.6	53.8	61.3	51.5	83.4	84.9
5. Germany	25.6	33.3	56.4	38.2	.....	45.8	41.2	39.0	40.8	51.4	42.9	80.2	80.6
6. Poland	45.0	51.7	67.9	57.9	47.1	.....	40.1	23.3	34.4	27.7	50.6	78.6	76.6
7. Czechoslovakia	39.5	44.5	65.6	51.1	39.5	41.7	.....	41.3	34.4	53.2	51.3	81.6	80.8
8. Austria	40.2	47.1	68.0	54.2	40.4	20.3	39.9	.....	30.2	21.4	50.4	80.2	77.2
9. Hungary	39.1	44.2	68.3	52.9	38.7	31.3	33.9	24.7	.....	39.9	54.0	80.4	78.8
10. U.S.S.R.	50.2	57.1	72.9	62.2	52.1	20.0	49.0	19.0	32.7	.....	59.4	81.1	78.1
11. Italy	44.9	48.0	60.2	51.9	45.6	52.7	51.6	53.0	53.9	60.5	.....	78.9	78.2
12. Negro	80.3	80.3	88.4	83.7	80.6	79.7	81.9	81.1	80.4	81.8	80.5	.....	66.0
13. Puerto Rican	79.8	76.5	88.2	83.9	79.7	75.5	78.6	76.6	76.3	78.1	77.8	63.8	.....

* New York SMSA indexes are included to present a more complete record for reference.

SOURCE: U.S. Bureau of the Census, *1960 Census of Population* (see text).

[14] Alternative approaches to studying the effects of non-racial factors are outlined in Taeuber and Taeuber, *Negroes in Cities*, chap. iv.

dinavian populations which have partially intermarried and even have at least one community in common (the "Bay Ridge" neighborhood in Brooklyn). But the high delta does represent ethnic separation, for each national group still maintains its own newspaper, and each lives in neighborhoods separate from those inhabited by the other. If Swedes and Norwegians are not highly integrated with each other, it is likely that they are even less integrated with other ethnic populations. And this is the case, for the Swedish-Norwegian delta is the lowest for the Norwegians and the fourth lowest for the Swedes.

We can carry out a more general analysis of the Consolidated Area's interethnic segregation if we focus on those ethnicities which are relatively integrated—say, with deltas of less than 35.0. Such a comparison highlights the usual "old versus new" European dichotomy, which is most likely attributable to the differential mixture of Jews, and to common periods of migration. Although we admit that we are being outlandishly speculative, we are intrigued by the observation that the segregation among the "new" ethnicities reflects the map of central Europe in the nineteenth century. For example, if we consider these ethnic populations in order of size of delta, we would entirely exclude the Italians and encompass the ethnicities of the old Austro-Hungarian Empire and their cousins, the Slavs.[15] Most closely connected are the Austrians and the Russians (21.4), followed by the Austrians and the Poles (23.3), the Poles and the Russians (27.7), the Hungarians and the Austrians (30.2), the Hungarians and the Poles (34.4), and the Czechs and the Hungarians (34.4). We do not entirely discount the influence of the Old World; despite the fact that many of these groups were in conflict, there are forces which bring people together—perhaps shopping districts, perhaps nationality-group realtors.[16] Similarly, a second cluster combines the Anglo-Saxons—the United Kingdom and Germany (25.9) each of these with the Irish, and the Swedes with the United Kingdom.

Two ethnicities remain, highly segregated from all others: the Italians and Scandinavians. We might expect that the Italians, a southern European or "new" stock, would remain highly segregated but we would have no reason to anticipate that they are more integrated with the "old" northwestern Europeans than with the "new" southern

[15] C. Grant Robertson and J. G. Bartholomew, *An Historical Atlas of Modern Europe from 1789 to 1922* (London: Oxford University Press, 1924); Carlton J. H. Hayes, *A Political and Social History of Modern Europe* (New York: Macmillan Co., 1929), II, 427.

[16] Attempts to partition ethnicities into Jew and gentile (or Catholic and Protestant) result only in guesses. For example, Herbert Seidman, Lawrence Garfinkel, and Leonard Craig conclude: "In New York City the Russian-born are heavily Jewish. The Polish-born, the Austrian-born, and the Hungarian-born are largely Jewish. The Italian-born and Irish-born are heavily Catholic. The Scandinavian-born and British-born are predominantly Protestant. The German-born and the natives are distributed with large proportions in each of the three major religious groups" ("Death Rates in New York City by Socio-economic Class and Religious Groups and by Country of Birth, 1949–1951," *Jewish Journal of Sociology*, IV, No. 2 [December, 1962], 264). See also Ira Rosenwaike, "The Utilization of Census Tract Data in the Study of the American Jewish Population," *Jewish Social Studies*, XXV (January, 1963), 42–56. This pattern appears to be unique to New York. In contrast to the Consolidated Area in 1960, Chicago's pattern of foreign-born white segregation in 1930 and 1950 shows separate England-Eire and Germany-Austria connections becoming linked with each other through the English (and the Swedes through the Irish) in 1950 (Otis Dudley Duncan and Stanley Lieberson, "Ethnic Segregation and Assimilation," *American Journal of Sociology*, LXIV [January, 1959], 364–74). We do have some fragmentary evidence of ethnic clustering and persistence. Robert Park and Herbert Miller, in *Old World Traits Transplanted* (Chicago: University of Chicago, Society for Social Research, 1925), particularly in the discussion of the map facing p. 146, point out how the Italian colony in New York City's Bowery re-created the provinces and villages of southern Italy on contiguous blocks of the city. David Wallace, in *Residential Concentration of Negroes in Chicago*, p. 209, judges that in Chicago, nationality-group real-estate operators have manipulated home buildings and loans in order to control and preserve their market.

and central Europeans.[17] Even less expected is that the Norwegians are highly segregated from other ethnic groups. We have already noted that their lowest segregation is from the Swedes (45.4). Perhaps, then, it should not surprise us that their highest segregation, from the U.S.S.R. (70.7) (primarily Slavic Jews), approaches their segregation from Negroes (87.7). If we assume that residential segregation numbers reflect degrees of cultural acceptance, we think it a fair speculation that Norwegian segregation from Negroes differs in degree, but not in kind, from their separatism from Slavic Jews.[18]

[17] This appears to be unique to the Consolidated Area: in our Table 5, the average delta for the fifteen "new vs. new" interethnic deltas is 40.8; if we abstract from these fifteen the five interethnic deltas which include the Italians, we find that the average among just these five (53.1) is much higher than among the remaining ten (34.5). However, when we consider the city of Chicago (from Duncan and Lieberson, "Ethnic Segregation and Assimilation," *op. cit.*, Tables 2, 3, 4), we find that the Italians are similar to other ethnic groups: the over-all "new vs. new" segregation for foreign-born whites in 1930 (55.8) and in 1950 (53.5) compares with the analogous "Italian vs. new" average segregation in 1930 (59.5) and 1950 (54.6).

Some of these New York patterns are, of course, unique to the 1960 Consolidated Area. But we judge, from this first study of the New York metropolis, that its 1960 ethnic segregation patterns are similar to those of Chicago and most other northeastern and midwestern cities and that its ethnic segregation index numbers reflect a social reality which will remain viable at least for the foreseeable future. Certainly for the present, the strong prejudice against Negroes on the part of whites only compounds an existing separatism, for if Protestant Norwegians hesitate to integrate with Protestant Swedes, and Catholic Italians with Catholic Irish, then these groups are even less likely to accept Negro neighbors.

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[18] Duncan and Lieberson present evidence that ethnic residential segregation is correlated with some attitude scales used by Bogardus to study the acceptance or rejection of ethnic minority groups, at least by the general population ("Ethnic Segregation and Assimilation," *op. cit.*, pp. 272–73). We do not know whether the Consolidated Area's Norwegian segregation is peculiar to this area, since data for other cities are lacking.

The Conception and Measurement of Asymmetric Monotonic Relationships in Sociology

Robert K. Leik and Walter R. Gove

ABSTRACT

Theoretical propositions in sociology are frequently stated in a form such as "the greater the A, the greater the B," which implies only monotonic relationships. Appropriate indexes are seldom used to test this type of proposition since those available are either too stringent (linear correlation, for example) or they ignore monotonicity altogether (e.g., χ^2). This paper discusses two types of asymmetric monotonic indexes using a proportional reduction in error logic; one based on γ and one based on the correlation ratio. Weak, intermediate, and strong forms of the γ-type indexes are developed (two of which are currently in the literature), a general equation for the three is provided, and criteria for choosing relatively weak or strong measures are discussed. The index based on the correlation ratio agrees closely with the γ-type indexes (for ordinally treated interval level data) but possesses the advantage of relating directly to analysis of variance significance tests. The form of tests for all indexes is indicated.

An important step in improving the link between a theory and the research which is intended to test that theory would be to require that the form of the empirical test be identical with the form used in the theoretical statements. Unfortunately, the two forms are frequently not comparable.

Much of current sociological theory construction consists of statements of relationships patterned after Zetterberg's "the greater the A, the greater the B."[1] Although many statements are not precisely of that form, most are clearly intended to imply covariation of the two variables under consideration and are assumed to provide greater theoretical precision because they allow what is referred to as axiomatic theory: the precise formulation of some "primitive" relationships, or axioms, and the derivation of other relationships as logical consequences of these axioms. Note, however, that a statement such as the one quoted above does not specify anything about the form of the relationship other than positive concomitant variation.

If two assertions stated in the above form are used as premises, then, following Zetterberg, a conclusion may be drawn from them by the "sign rule." According to this rule, the product of the signs of the premises should be the sign of the (valid) conclusion. For example,

the greater the A, the greater the B

(+relation),

the greater the B, the smaller the C

(−relation),

should yield

the greater the A, the smaller the C

([+][−] = −relation).

It has been demonstrated elsewhere, however, that the sign rule will not necessarily provide valid conclusions.[2] The reason for the sign rule inadequacy, when it seems intuitively correct, is that the rule implicitly assumes perfect relationships, whereas observed relationships seldom reach unity. When observed relationships are *very* high,

[1] Hans L. Zetterberg, "On Axiomatic Theories in Sociology," in *On Theory and Verification in Sociology* (Stockholm: Almquist and Wiksell, 1954). See also Zetterberg, *On Theory and Verification in Sociology* (*A Much Revised Edition*). (Totowa, N.J.: Bedminster Press, 1963.)

[2] Herbert L. Costner and Robert K. Leik, "Deductions from Axiomatic Theory," *American Sociological Review*, XXIX (December, 1964), 189–835.

however, it is possible to obtain valid conclusions using the sign rule alone, although the longer the chain of subsequent deductions, the more nearly perfect the axiom relationships must be.

Other assumptions, when appropriate, may be used to simplify deductions. In particular, it has been shown that the assumption of asymmetric causal relations gives valid conclusions when using the sign rule.[3] By asymmetric causality is meant that change in the independent variable (cause) will induce change in the dependent variable (effect). The converse will not obtain; that is, change in the dependent variable need not induce change in the independent variable. Why asymmetric causality implies valid sign rule deductions will not be restated here, but the importance of the deductive strength gained when asymmetric causal relations can be assumed should be emphasized. To rhetorically assert asymmetric causality for ease of logical derivation, however, gains nothing. There must be either cogent reasons for such assumptions or tests of these assumptions, or both, to make the theoretical derivations more than empty verbiage.

One way to test the assumptions would be to gather data directly bearing on them. Alternatively, as Zetterberg has stressed,[4] if valid derivations from those assumptions are consistently borne out empirically, the assumptions themselves gain credence. Since deductions from asymmetric causal relations are generally also asymmetric causal in form,[5] either of these ways of determining the appropriateness of the assumptions will require empirical tests of asymmetric causal statements. Our questions, then, as implied by the introductory comments of this paper, are (1) What is implied, operationally, by an asymmetric causal relation? and (2) What statistical indexes are appropriate for testing the extent to which data substantiate assumptions of asymmetric causality?

First, it should be evident from the form of the proposition that at least ordinal level of measurement is required in order to assess concomitant variation. Second, the relationship specified in a statement of the form "the greater the A, the greater the B" is monotonic, but not necessarily more precise than that. Third, as stated, the relationship is asymmetric. Although usage varies in this regard, we are specifically concerned with asymmetric causal statements, as noted above. Finally, the conditions of causality are required. Because causal inference has been discussed extensively elsewhere,[6] the focus of the present paper will remain on the first three aspects cited; that is, on the question of asymmetric monotonic relationships.

There is very little use of statistical measures which are specifically asymmetric and monotonic, but not more stringent, in the sociological literature. Many reasons exist for this fact. Perhaps paramount is a lack of measures of this sort from which to choose when analyzing data. Some suggested indexes will be developed in this paper. A more troublesome reason, though, appears to be a lack of understanding of the necessity for modes of analysis to match theoretical assumptions if the assumptions are really to be tested. It is very likely that many rejected hypotheses have fallen under an unduly sharp statistical ax, whereas others have been left standing by instruments which are too dull for the job at hand.

MEASURING ASYMMETRIC, MONOTONIC RELATIONSHIPS

This paper will be concerned with appropriate forms which association indexes

[3] *Ibid.*

[4] Zetterberg, *op. cit.*

[5] This type of deduction results from transitivity and postulates of the form $A \rightarrow B$ and $B \rightarrow C$. It is also possible, given postulates of the form $A \rightarrow B$ and $A \rightarrow C$ to deduce a correlation between B and C which is neither causal nor directional.

[6] See, for example, Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill, N.C.: University of North Carolina Press, 1964) as well as numerous related articles; Otis Dudley Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, LXXII (July, 1966), 1-16; Herbert A. Simon, *Models of Man* (New York: John Wiley & Sons, 1957), chap. ii.

may take so that they can adequately assess asymmetric monotonic assertions. Specifically, it will discuss measures which are (1) asymmetric, (2) tailored to the level of measurement available for the variables under consideration, and (3) have an operational interpretation which Costner has called proportional reduction in error (PRE).[7] Such a PRE measure requires three rules. Those rules are: first, a way of predicting the dependent variable without use of the independent variable, second, a way of predicting the dependent variable using the independent variable, and third, an appropriate way of measuring error.

Assuming that the data gathered to test an asymmetric monotonic theoretical formulation are necessarily at least ordinal in character, the question is: What kind of statistic is appropriate for assessing monotonicity? Usually, analysis of data gathered to assess axiomatic formulations, or other theoretical formulations using similar forms of statement, violates the monotonicity of the theoretical statements in one of two ways. First, the monotonic form of the relationship may be entirely ignored, as would occur when such statistics as χ^2 and related association indexes, or the correlation ratio, η^2, are used.[8]

The second type of inappropriate analysis involves unduly stringent indexes, such as r, intraclass correlation, or Robinson's A. The least stringent of these, r, requires not only interval level measurement but also linear regression of the dependent variable on the independent variable. Linearity is much more demanding than simple monotonicity, hence the use of r may cause rejection of hypotheses of a monotonic form when those hypotheses are in fact valid. It should be evident, therefore, that χ^2 and related measures and η^2 are inappropriate for the task at hand, while r, intraclass correlation, and A are frequently too stringent.

One other statistic fairly often used for testing monotonic relationships is γ. Although γ does not have the problems associated with the previously mentioned statistics, since it is essentially a measure of consistent order in the data which does not impose interval assumptions or specific regression forms, it does have two related faults. First, there is no asymmetric predictability, hence the priority implied in the theoretical form is not employed in the analysis of data. Second, many cases are ignored due to ties. To surmount the first problem, it is possible for the data analyst to determine which variable is the independent variable, by postulate or hypothesis, and then to require that a change in this variable be accompanied by a change in the dependent variable. There are different rationales for resolving the problem of ties. As will be seen below, these rationales result in various related but numerically different indexes.

Two indexes which will be discussed are already in the literature and the rest are simple adaptations of existing procedures. After presentation of the indexes, we will discuss the extent of variation among them due to different prediction and error-measuring rules. Since monotonic formulations are so common in the literature, it is reasonable to assert that measures of these relationships should occupy a central place in our statistical kit bag. The fact, which will be demonstrated, that certain patterns of data cannot be analyzed by all of these indexes means that we have to focus considerably greater attention on the implications of the prediction and error-measuring rules which we use when assessing the validity of our theoretical propositions.

[7] Herbert L. Costner, "Criteria for Measures of Association," *American Sociological Review*, XXX (June, 1965), 341–53.

[8] For a recent discussion of this problem see Thomas J. Duggan and Charles W. Dean, "Common Misinterpretations of Significance Levels in Sociological Journals," *American Sociologist*, III (February, 1968), 45–46. Although these authors properly point up the inadequacy of χ^2 analysis for testing linear relationships, they do confuse the question of monotonicity versus linearity. See also Somers' discussion of this type of inappropriate analysis. (Robert H. Somers, "A New Asymmetric Measure of Association for Ordinal Variables," *American Sociological Review*, VI [December, 1962], 800–802.)

FORMULATING MONOTONIC INDEXES: PRE PREDICTION RULES

The form that a monotonic index must take depends upon the level of measurement of the variables being used. Since the level of measurement of each variable must be at least ordinal, the following diagram indicates all cases which must be considered.

		Dependent Variable (Y)	
		Ordinal	At Least Equal Interval
Independent Variable (X)	Ordinal	Case 1	Case 3
	At Least Equal Interval	Case 2	Case 4

Let us assume, for convenience, that the relationships we are discussing are all positive; that is, if X_j is greater than X_i then Y_j is greater than or equal to Y_i. Note that this statement specifies weak monotonicity. It is also possible to strike the words "or equal to," leaving a strong monotonic assertion. With precise, hence generally untied scores, this distinction is not usually very important as long as there has been ample time for the expected causal effect to occur. The question of weak or strong form cannot be lightly dismissed, however, when there are many ties on Y.

For case 1 (strict ordinality for both variables) it will be necessary to assume data arranged in contingency table form. The only exception to this requirement seems to be rank correlation, which is, in fact, based directly on Pearsonian product-moment correlation with its attendant equal-interval measurement assumptions. To remain with strictly ordinal treatment, we shall not discuss rank correlation. The prediction rules for case 1 may be operationalized in terms of pairs of points, where one anticipates that change from Y_i to Y_j for each pair will be predictable from the change of X_i to X_j.

Given case 2 data (the independent variable is at least equal interval in level, but the dependent variable is only ordinally measured), we will not be able to exceed the ordinal properties in predicting the dependent variable. Consequently, we will not be able to use the higher properties of the independent variable, X. In this instance, then, it will be necessary to treat both variables as ordinal and use the approach indicated above for ordinal data.

For case 3 (the dependent variable is equal interval or greater, but the independent is ordinal), the higher level of measurement of the dependent variable may be used for assessing monotonicity more precisely than is possible for ordinal dependent data. When the independent variable is ordinal only it should be possible to examine variation of the Y values around the mean Y within each category of the independent variable X. The only requirement, in this case, which will distinguish a monotonic measure from the familiar correlation ratio will be that the Y values around which "within" variances are computed must themselves be monotonically arrayed across the ordered categories of X.

Case 4 data (measurement of both variables is at least equal interval level) make it possible to treat the pairs of X, Y observations in the usual Cartesian coordinate system, treating each point separately. The question then becomes that of using any theoretically useful function, $Y = f_m\ (X)$, where f_m denotes a monotonic function. Examples of such monotonic functions are a log curve ($Y = \log X$) and the normal ogive.

MEASURING ERROR

Measuring error for the situations considered previously will again depend upon the measurement level available. If data are case 1 (ordinal on both variables), and predictions are made in terms of pairs, as in γ,[9] the only change from the standard γ form will be to include all pairs not tied under variable X. The question then becomes whether to treat those pairs which are tied on variable Y as errors (strict monotonicity) or as correct predictions

[9] See Leo Goodman and William Kruskal, "Measures of Association for Cross Classifications, I," *Journal of the American Statistical Association*, XLIX (December, 1954), 740.

(weak monotonicity), or to compromise in some way. One possible basis for decision is to assume that ties are a consequence of measurement inaccuracy. If precise discrimination could be made, it would be reasonable to assume that for half of the tied pairs $Y_i > Y_j$ and for the other half $Y_j > Y_i$, hence ties should be equally divided between correct and incorrect predictions (i.e., between concordant and discordant pairs). Following this procedure, we find that an appropriate asymmetric form of γ is precisely Somers' d_{yx}.[10] Derivation of this form via PRE logic will be demonstrated later.

Assuming that ties are the consequence of measurement procedures is not always defensible or desirable. Other factors may have resulted in pairs tied on Y. In particular, many sociologically relevant causal processes may require considerable time to run their course. If, for a sufficient number of cases, measurement has occurred after an individual case has changed on X but before any effects have developed sufficiently to record a change on Y, then those cases will appear to be mislocated in the resulting contingency table. If the process under study is one of abrupt changes in the dependent variable (e.g., standard system performance until, with sufficient external pressure, a gradually internally generated overload condition is reached, giving abruptly erratic or terminated performance), no matter how precisely the dependent variable is measured there will be no discernible change prior to the occurrence of the abrupt change. Thus X will have changed but Y will have remained constant. If continuous, lagged change is under consideration, apparent ties on Y are a joint consequence of the lag (small change on Y for large change on X) and imprecision of measurement. Thus the time of measurement rather than its accuracy becomes the reason for ties on Y. When there is adequate theoretical or empirical basis for expecting delayed effects, it would be appropriate to use a weak monotonic form, treating all pairs tied on Y as correct predictions.

There are instances, contrary to the above, in which neither inaccurate measurement nor truncation of a causal process can be assumed, nor can any other explanation of ties be found which is satisfactory. In such instances, the only proper solution would be to choose a strong monotonic index, treating all ties on Y as errors. Because such a procedure will produce lower index values than either of the preceding, it is unlikely to be chosen very often. We have a (perhaps understandable) predilection for high coefficients. It should be realized, however, that dependable knowledge will develop to the extent that we really test what we say we are testing.

For data involving an interval-level dependent variable, regardless of the level of the independent variable (cases 3 and 4), it will be appropriate to measure error in terms of variance. Thus, for case 3 data, the mean squared error around $\overline{Y}$ (σ_y^2) will be the amount of error under rule 1; "within" variance around monotonically arrayed means will be the amount of error under rule 2. Again following PRE form, a variant of the correlation ratio will be defined.

It is also possible to treat the increased precision of interval level of measurement (of the dependent variable only) as a way of avoiding ties when using the γ-type of index. If, as may be expected, some ties still arise in this manner, the same considerations presented earlier apply to their interpretation and the manner in which they are incorporated into the index. Because an ordered-pair logic seems to be as justifiable as a regression model, the γ form and the correlation-ratio form would appear to be equally valid. They may, however, give different results. For example, the correlation ratio is based on a least-squared-error model, which will be sensitive to extreme values, whereas an ordered-pair logic is known to be subject to marginal frequency variations (skew). Some practical aspects of this problem will be examined after computing forms of the indexes have been presented.

[10] Somers, *op. cit.*

Finally, error measurement for indexes based on specific monotonic regression curves, when both variables are at least equal interval in measurement level, will again be in terms of variance about a least-squared-error curve. First, parameter estimation for curve-fitting frequently utilizes a least-squared-error criterion.[11] Second, partition of total variance into explained and unexplained, via the theoretical curve, readily allows analysis of variance tests.[12] Again, PRE-type indexes may be easily constructed from total variance and unexplained variance.

COMPUTATION OF THE INDEXES: GAMMA VARIANTS

The γ-type indexes can be readily defined for computational purposes by use of Somers' notation: let P denote the number of concordant pairs, Q the number of discordant pairs, X_o the number of pairs tied on X only, Y_o the number of pairs tied on Y only, and Z the number of pairs tied on both X and Y.[13] Then, simply:

$$\gamma = \frac{P - Q}{P + Q}. \qquad (1)$$

It will be helpful to define PRE forms for the other indexes, however, so the PRE basis of γ will be briefly restated:[14] Predict only for pairs without tie on either variable; predict $Y_j > Y_i$ for rule 1 (prediction without use of X), but concordance for rule 2 (prediction using X). The amount of error will be 50 per cent of all predictions, or $\frac{1}{2}(P + Q)$, for rule 1, and the number of discordant pairs, or Q, for rule 2. Following Costner, then, γ may be defined in PRE terms as

$$\gamma = \frac{\frac{1}{2}(P + Q) - Q}{\frac{1}{2}(P + Q)} = \frac{P - Q}{P + Q}, \quad \text{as above}. \qquad (2)$$

Somers' d_{yx} may be similarly defined if those pairs tied on Y are included, to provide asymmetry, and one-half of these pairs are treated as erroneously predicted. Prediction rules remain the same as for γ. Error under rule 1 is then $(P + Q + Y_o)/2$ and error under rule 2 is $Q + (Y_o/2)$. In the form of equation 2, Somers' index is

$$d_{yx} = \frac{\frac{1}{2}(P + Q + Y_o) - [Q + (Y_o/2)]}{(P + Q + Y_o)/2} = \frac{P - Q}{P + Q + Y_o}, \quad \text{as he has defined it}. \qquad (3)$$

Clearly, $\gamma \geq d_{yx}$, with the equality obtaining only if there are no pairs tied on Y only (i.e., $Y_o = 0$).

For a strong monotonic index, all pairs tied on Y will be treated as erroneously predicted. For rule 1 predict $Y_j > Y_i$ and for rule 2 predict concordance. Error under rule 1 will equal $(P + Q)/2 + Y_o$, or 50 per cent of all pairs not tied on Y plus all of those which are tied. Error under rule 2 will equal $Q + Y_o$. Consequently, letting d'_{yx} designate a strong monotonic variant of Somers' index, we find

$$d'_{yx} = \frac{(P + Q)/2 + Y_o - (Q + Y_o)}{(P + Q)/2 + Y_o} = \frac{P - Q}{P + Q + 2Y_o} \quad \text{(strong monotonic form)}. \qquad (4)$$

It is evident that $d_{yx} \geq d'_{yx}$, with the condition of $Y_o = 0$ for the two to be equal. Given that $Y_o > 0$, the three indexes thus far discussed have the following order of magnitude: $\gamma > d_{yx} > d'_{yx}$.

Finally, for a weak monotonic index,

[11] This is one of two principal procedures, the other being maximum likelihood estimation. See, for example, Paul G. Hoel, *Introduction to Mathematical Statistics* (New York: John Wiley & Sons, 1956), chap. iii; Alexander M. Mood, *Introduction to the Theory of Statistics* (New York: McGraw-Hill Book Co., 1950), chap. viii.

[12] Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill Book Co., 1960), chap. xviii.

[13] Somers, *op. cit.*

[14] This is a slightly different statement from that given by Costner, but follows his suggested approach. Costner, *op. cit.*

predict $Y_j > Y_i$ for rule 1, and concordance for rule 2. All Y_o pairs will be treated as correctly predicted. Error under rule 1 will equal $(P+Q)/2$ or 50 per cent of all pairs not tied on Y, and error under rule 2 will equal Q only. Note that these are exactly the error frequencies used in computing γ itself. It becomes evident, therefore, that γ may be interpreted as a weak monotonic asymmetric index. Depending upon the magnitude of Y_o relative to P and Q, these indexes may differ grossly or minutely. Choosing the appropriate index is of paramount importance for adequate interpretation of the data.

It should be noted that the three indexes presented, γ, d_{yx}, and d'_{yx} are just specific forms of the general formula

$$\frac{P-Q}{P+Q+kY_o}.$$

The issue of deciding which index to use is then the issue of deciding what is the appropriate value for k, in the case of γ, $k = 0$; of d_{yx}, $k = 1$; and of d'_{yx}, $k = 2$. In general, determination of k for any value in the range $0 \leq k \leq 2$ should be dependent upon the extent to which such factors as measurement inadequacy, delay in anticipated effects, etc., can be expected to affect the probability of ties on Y.

THE CORRELATION RATIO VARIANT

Computation of a monotonic variant of the correlation ratio will be comparable to that for the ordinary correlation ratio with the exception of any adjustments needed to retain monotonicity of the values of Y around which the "within" variance is computed. Let us consider that aspect first. If measurement is continuous it allows theoretically infinitesimal differences, the limit of the difference between any two points being zero. The question, therefore, of a weak or strong monotonic form becomes meaningless *unless discrete data* are used (rather than rounded measures from continuous variables). To simplify the discussion, continuity will be assumed. Let the monotonic requirement be stated in terms of a series of points, Y_{mj}, one for each X, category. These points will be analogous to, and as close as possible to the category mean, $\bar{Y}_j$. The measure may be defined as:

$$\eta_m^2 = 1 - \frac{1}{n\sigma_Y^2}\sum_{ij}(Y_{ij} - Y_{mj})^2 .$$

Two conditions need to be satisfied:

Condition 1: $\sum_{ij}(Y_{ij} - Y_{mj})^2$ is a minimum, and

Condition 2: $Y'_{mj} > Y_{mj}$ if $X'_j > X_j$.

The first requirement produces least-squared error and would yield category means for the Y_{mj} if no other condition was imposed. The second condition assumes monotonicity.[15]

Assume, initially, that the $\bar{Y}_j$ values are monotonic across categories of X. Then these category means will satisfy both equations, and η^2 may be computed directly. If, however, there is an inversion in the progression of $\bar{Y}_j$ values, they will no longer satisfy condition 2. Consider the case of only one inversion: $\bar{Y}_j < \bar{Y}_{j-1}$. If the associated value, Y_{mj}, is set equal to the preceding category mean, or if Y_{mj-1} is set equal to $\bar{Y}_j$, condition 1 will be violated. A point between these two values is needed. Remembering that a mean does satisfy the least-squared-error criterion, we may deduce that the desired point for both categories is the mean of all Y values in both categories combined. This point is the same as the weighted average of the original category means. Whenever only one inversion of category means occurs, we need only combine those two categories and proceed with the computation of η^2 on a reduced set of categories. Note, however, that combining categories is a procedural convenience.

[15] Olsen, for example, has used η^2 to test monotonic relationships, and has appropriately noted its inadequacy when means are not monotonically arrayed. He suggests no alternative, however. Marvin E. Olsen, "Perceived Legitimacy of Social Protest Actions," *Social Problems*, XV (Winter, 1968), 297–310.

What is accomplished is the determination of a value of Y_m for each category in such a way that these values satisfy conditions 1 and 2 simultaneously. We will return to the question of the appropriateness of combining categories following a more complete discussion of the determination of specific Y_m values.

Should more than a small inversion of category means occur, a new problem arises. Suppose, for illustration, that the distribution of the $\bar{Y}_j$ is U-shaped. Generalizing from the previous situation, one might attempt to combine a series of adjacent categories, starting at one end of the distribution. At first, there will still be a U-shaped pattern, but as more and more categories are combined the mean of the new macro-category can eventually be made less than that for the next category. All remaining means will be monotonic increasing, and the resultant value of η_m^2 may be moderately large in spite of an initial distribution which was clearly non-monotonic even assuming considerable measurement error. Figure 1 illustrates this approach.

That this result may produce erroneous values becomes evident upon consideration of the idea of random error around a regression line or around the means of samples

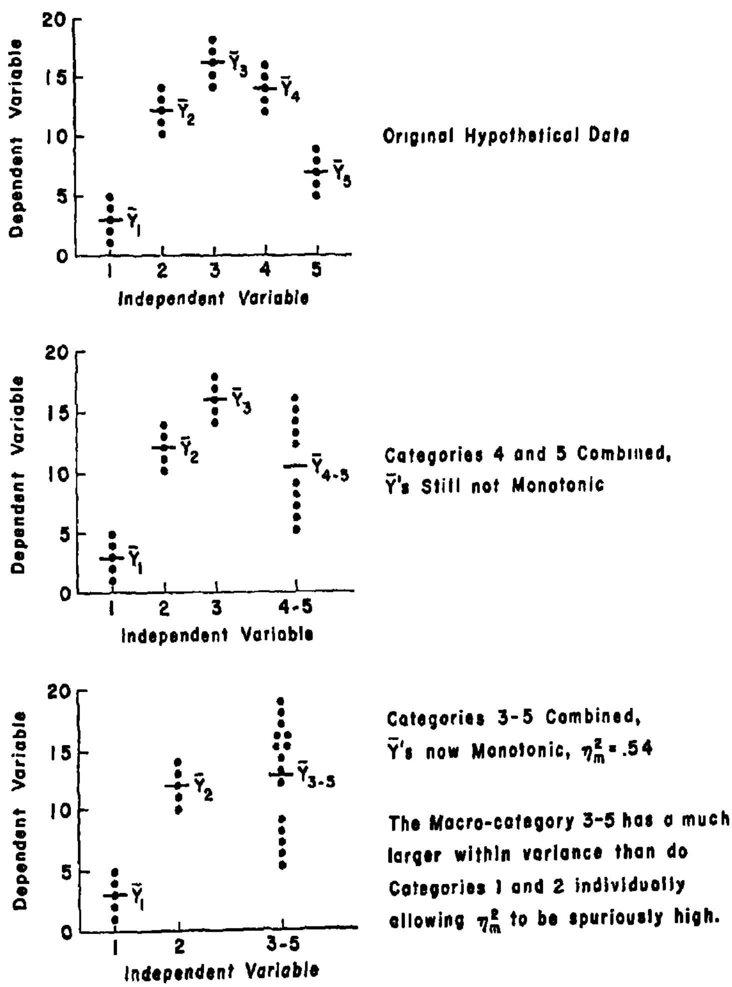


FIG. 1.—Inappropriate combination of categories for η_m^2

from the same population. Specifically, for F-ratio significance tests for the correlation coefficient and for analysis of variance, it is necessary to assume homoscedasticity. The procedure suggested above, however, results in one macrocategory with a large variance, while the remaining categories have noticeably smaller variances. Stated another way, the procedure could produce values of Y_m which require, for some categories, that all error $(Y_{ij} - Y_{mj})$ be positive, and for other categories, that all error be negative. That such a condition would occur by sampling happenstance is exceedingly unlikely. Thus, although the means meet the requirement of monotonicity, they present a situation which is highly improbable given independent random samples from the same population (null hypothesis).

The implication of homoscedasticity considerations for η_m^2 is that dispersion of within-category variances must be kept small. Such a requirement may be made specific by computing F-ratios for testing the hypothesis that category variances are equal within sampling variability. For the U-shaped distribution of Figure 1, combining successive categories at one end would soon produce such unequal variances that a significant F would be reached. Consequently, other macrocategories would have to be established before further enlarging of the first macrocategory could occur. The likely conclusion would be a set of categories with approximately equal means, producing weak monotonicity but little "between" variance. A final requirement for η_m^2, then, is that the following null hypothesis not be rejected for the k macrocategories on which η_m^2 is finally computed.

$$\text{Condition 3: } \sigma_1^2 = \sigma_2^2 = \ldots = \sigma_k^2 .$$

To facilitate the necessary computations for separate category variance estimates, it is preferable to compute the sums of squares of Y for each of the original categories. Combining a set of categories, then, will result in the following variance estimate for the macrocategory:

$$\sigma_m^2 = \frac{1}{\sum_j n_j - 1} \sum_j [SS_j + n_j(\bar{Y}_j - \bar{Y}_{mj})^2] ,$$

where n_j denotes the number of observations in category j, SS_j is the sum of squared deviations from the mean of category j, and $\bar{Y}_j - \bar{Y}_{mj}$ is the difference between the means of category j and the macrocategory, k. The formula represents the combination of "within" and "between" sums of squares, from analysis of variance format, to give total sum of squares divided by total degrees of freedom (d.f.).

It is relatively simple to reconstruct F-ratios from the above estimates each time a new category combination is considered, and to determine whether that combination will be acceptable under the third criterion. This procedure requires more time, unfortunately, than does computation of a γ-type index. If the independent variable has at least equal interval level of measurement, however, the accompanying availability of F-ratios for determining, for example, the statistical significance of non-linearity, may justify the added work.

It may seem at first as if combining categories begs the question of monotonicity. This is not so, however, because as categories are combined, "within" variance is increased and the correlation ratio diminishes. In the extreme case of combining all categories, all variance is within, and the index equals zero. In fact, the difference between the ordinary correlation ratio (computed on the original data) and the monotonic form of that ratio provides a precise statement of the extent to which the relationship is monotonic rather than "free form." It should be noted that there is nothing in the computation of η_m^2 that indicates the direction of the slope of the monotonic relationship. Whether the relationship is positive or negative, however, can readily be decided by inspection.

One further point should be considered

regarding η_m^2. It may, on rare occasions, be impossible to combine categories in such a way that conditions 2 and 3 are both satisfied. If this occurs, it is necessary to conclude that η_m^2 is an inappropriate index, and to turn instead to γ, d_{yx}, or d'_{yx}. Because these latter indexes are based on pairs of points rather than a regression model, they are appropriate regardless of a lack of homoscedasticity. An example of data which do not admit computation of η_m^2 may be found in set E, Figure 3.

SIGNIFICANCE TESTS

Four indexes have been discussed. Appropriate significance tests for the null hypothesis may be constructed for γ, d_{yx}, and d'_{yx} (as Somers has already noted regarding the first two measures) by virtue of the fact that their numerators are all Kendall's S.[16] Somers[17] indicates that the best discussion of the test developed by Kendall is by Burr.[18] An analysis of variance test for η_m^2 is exactly the same as that for η^2 itself,[19] with the exception of a reduced number of d.f. involved in the explained variance ("between" variance) when categories are combined to obtain monotonicity. As noted earlier, analysis of variance tests are also appropriate when specific curves (e.g., a log curve) are fitted to the data.

COMPARISON OF INDEXES

Dummy data were developed for the purpose of comparing the various measures discussed above. For each of the sets of data developed, two values for the γ-type indexes were computed: the first value being obtained by treating the dependent variable as an ordinal measure, and the second value by treating the dependent value as an interval measure. In Figure 2 three sets of data are presented, each of which has an "obvious" but imprecise monotonic form, with the monotonicity being the clearest in set A and least clear in set C. Such data might occur if the basic relationship is monotonic but the data are subject to extraneous influences and/or measurement error. A comparison of the γ-type indexes and η_m^2 shows the values to be of the same general magnitude. Furthermore, the value of each index diminishes as one shifts from set A to set B to set C. This suggests that the γ-type indexes and the variance-based index both reflect the form of the data fairly well in the cases where there is an obvious monotonic relationship. The primary difference between the two types of measures is that η_m^2 is particularly sensitive to the dispersion of the dependent variable, whereas the γ-based indexes are not.

A comparison of the different γ-type indexes illustrates that when there are few ties, as generally occurs when the dependent variable is measured on an interval scale, it makes very little difference if one uses a strong measure of monotonicity (d'_{yx}) or a weak measure (γ). However, when there are a number of ties on the dependent variable, as will usually occur with an ordinal measure, the choice of one's measure becomes important. It might be noted at this point that when a set of data has a general, although not perfect, monotonic form, collapsing the dependent variable into relatively few categories will usually result in somewhat *inflated* γ values. This can be seen in Figure 1 by comparing the γ values obtained by using an interval measure with those obtained using an ordinal measure. The difference between the two would be even greater if the categories of the dependent variable were collapsed further. Taking the extreme case of only two categories for the dependent variable, $\gamma = 1.0$ for set A, 1.0 for set B, and 0.8 for set C (with a cutting point being established between 11 and 12). If, however, instead of γ, a strong

[16] Maurice G. Kendall, *Rank Correlation Methods* (New York: Hafner Publishing Co., 1955), chap. iii.

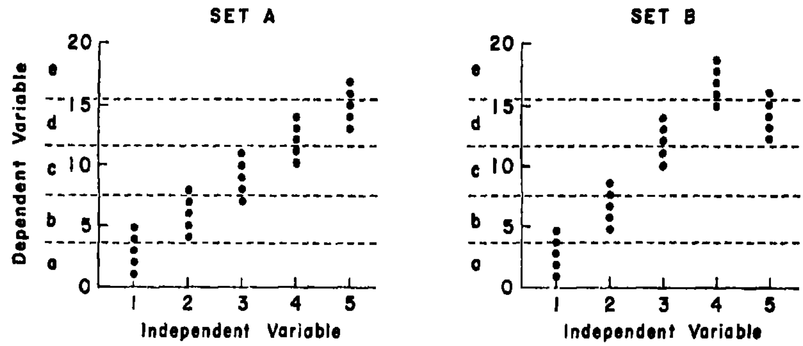
[17] Somers, *op. cit.*

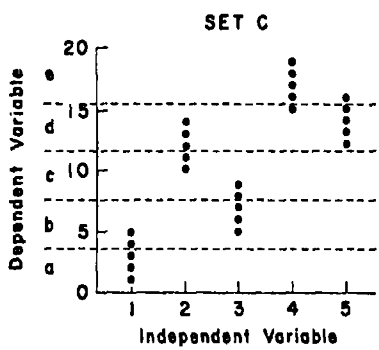
[18] Edward J. Burr, "Distribution of Kendall's Score S for a Pair of Tied Rankings," *Biometrika*, XLVII (June, 1960), 151.

[19] Blalock, *Social Statistics*.

measure of monotonicity is used (d'_{yx}), collapsing the dependent variable will tend to *diminish* the value of the relationship instead of strengthening it.

In Figure 3, two sets of data are presented that do not have an obvious monotonic form. In set D the γ-type indexes all yield very small positive values, while in used. In set E when categories 3, 4, and 5 are combined, the requirement for monotonically arrayed means is met; but this macrocategory now has too great a variance vis-à-vis the other two categories ($F = 8.4, p < .05$). It is therefore necessary to combine categories 1 and 2 to obtain approximately equal variance for each cate-





	SET A		SET B		SET C	
	Ordinal	Interval	Ordinal	Interval	Ordinal	Interval
γ	99	.88	85	.81	.66	.59
d_{yx}	84	.85	.74	.79	.57	.58
d'_{yx}	.74	82	.65	.76	.51	.56
η^2_m		.90		.89		.59

FIG. 2.—Comparison of indexes using data obviously monotonic in form

contrast it is impossible to jointly meet the requirements of η^2_m that the means be monotonically arrayed and that the variance of the categories all be approximately equal.[20] Therefore, for this set of data only the γ-type indexes may be appropriately used gory ($F = 1.2, p > .05$). When this is done η^2_m produces a small positive value that is approximately equal to the value obtained with the γ-type indexes.

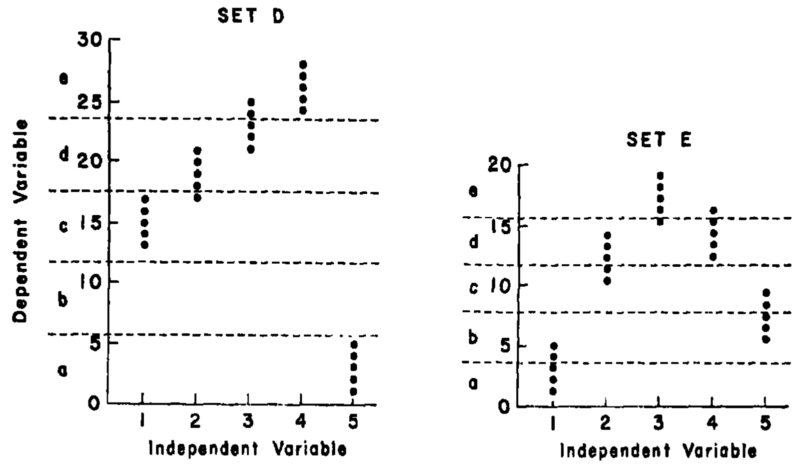
[20] For example, if categories 1, 2, 3, and 4 are combined to obtain a monotonic form, the variance of this combined category is much larger than that of category 5 ($F = 8.1, p < .05$). If, instead, categories 1, 2, and 3 are combined into one new category and 4 and 5 into another new category, the difference in variance between the two new categories is still too large ($F = 11.0, p < .05$)—and so forth.

DISCUSSION

Two noteworthy facts emerge from the preceding discussion. First, an index based on predicting the order among pairs of observations yields quite similar values to one based on variance of error if the distribution being examined is clearly monotonic. Although some differences are evident (for

set B there is a discrepancy of 0.17 between η_m^2 and d'_{yx}), they appear small enough to warrant the conclusion that the indexes are measuring the same type of property in a comparable manner. To base substantive conclusions on a comparison of η_m^2 from one study with, say, d_{yx} from another study would be unwise, since the indexes are not that nearly identical. Either index, however, represents an appropriate choice if data are of the requisite level of measurement.

The question of which type of measure to use with an interval-level dependent variable is not easily answered. Values of γ, d_{yx} and d'_{yx} are more readily computed, particularly with less clearly monotonic distributions, suggesting a preference for these measures. On the other hand, if measurement properties of the independent variable permit, η_m^2 provides an opportunity to divide variance into segments due to linear regression, monotonic but non-linear regression, and variance of unordered means (non-monotonic regression). Consequently, it is possible to conduct a variety of analysis-of-variance tests regarding monotonicity. The choice in this instance, then, appears to depend, in part, on related analyses which the research requires.



	SET D		SET E	
	Ordinal	Interval	Ordinal	Interval
γ	10	.19	20	.18
d_{yx}	.09	.19	.17	.18
d'_{yx}	.08	.19	.15	.17
η_m^2		*		23

* η_m^2 cannot be computed for SET D because it is impossible to jointly meet the requirements of monotonicity and homoscedasticity.

FIG. 3.—Comparison of indexes using data not obviously monotonic in form

Should the γ-type measures be chosen, as is necessary for strictly ordinal dependent variables, the second major point becomes crucial. It is possible to make differing assumptions about the meaning of tied obser-

vations. Consequently, a monotonic association index may take on widely differing values for the same data. The data for set A in Figure 1 vary from $\gamma = 0.99$ to $d'_{yx} = 0.74$, and much greater divergence will occur as the number of ties increases. Furthermore, the extent to which adjacent categories are combined to reduce the size of the table of observations will differentially affect the association index depending upon what assumptions have been made. A review of those assumptions appears warranted.

First, if ties on Y are assumed to be primarily a consequence of insufficiently refined measurement, it is reasonable to divide the ties equally between those to be treated as $Y_j > Y_i$ and those to be treated as $Y_i > Y_j$. Consequently, the appropriate index is Somers' d_{yx}. The entire question of the source of ties is usually ignored, however, and the weaker index, γ, computed.

A second treatment of ties would be to assume that they represent cases for which there has been inadequate time for a change in the causal or independent variable to produce a change in the dependent variable. Careful defense of the assertion that his data involve this type of tie on the dependent variable should entitle the researcher to use a less stringent form of asymmetric measure, namely, γ. Of course, for some research there will be little doubt that a tie is simply an instance of no concomitant change when it should have occurred. In the absence of arguments such as those discussed above, we should insist on the most stringent assessment of an asymmetric hypothesis: d'_{yx}.

Aside from these major questions, the discussion has pointed to two other related issues relevant to the appropriate analysis of any data. In the comparison of γ, d_{yx} and d'_{yx}, the collapsing of categories of the dependent variable into less precise intervals was found to affect the magnitude of those indexes in divergent ways. What is at issue, then, is the appropriateness of the frequent and convenient reduction of a contingency table to 2×2 format. When γ is computed this procedure can usually be relied upon to increase (inflate) the values of the coefficient, and thereby make the results look more impressive. On the other hand, were theoretical considerations to demand a strong index, d'_{yx}, the convenient collapsing would appear typically to decrease the value of the index. Our examples only suggest this divergence. Systematic exploration of the problem of collapsed categories would be a most helpful extension of our awareness of the consequences of such common practices.

The related issue concerns combining categories of the independent variable. As noted in the discussion of η^2_m, it is possible to obtain a high and invalid value for an index by collapsing the X categories *if* questions of homoscedasticity are ignored. This raises a question of general importance, for much of our operational interpretation of association indexes relies on their similarity to the correlation model with its implied random scatter about a regression line.

This problem may arise even with a 2×2 table. For example, Edwards[21] provides an interpretation of ϕ as a dichotomous form of the correlation coefficient, r. However, when 2×2 tables are produced which have virtually all cases in three of the four cells, the dispersion within each of the two categories of the independent variable is likely to be quite dissimilar. That is, there will be little or no dispersion in one category, but there may be a maximal dispersion (50–50 split) of cases in the other category. Association indexes may be quite high for such a table. If their interpretation rests on an underlying regression model they are not valid measures for the particular table. In general, when a theory predicts any type of concomitant change (not necessarily monotonic), and analysis is based on a regression model, then collapsing categories of the independent variable should be accompanied by checks to prevent widely discrepant dispersions of Y within categories of X. Otherwise, results will be spuriously high and consequently misleading.

[21] Allen L. Edwards, *Statistical Methods* (New York: Holt, Rinehart, & Winston, 1967), 126–28.

For monotonic prediction, as noted, the use of a γ-type index, with its ordered-pair logic, will avoid the regression model—homoscedasticity difficulty. It must be remembered, though, that these indexes pose other problems for the justification of collapsing tables. The necessary conclusion of these considerations is that we need either independent criteria for justifying collapsing contingency tables or a norm of presenting index values based on original as well as collapsed tables in order to avoid dressing up results by misleading and inappropriate presentation.

In sum, this paper has argued that we need to improve the co-ordination of our data analysis with our theoretical statements. For asymmetric causal theories, in particular, analysis must be concerned with monotonic relations. Toward that end, some monotonic indexes have been presented and discussed, with the result that more general questions of statistical procedure have been raised. Hopefully, the answers to these questions will help salvage useful hypotheses while aiding in the early re-examination of those which have thus far rested primarily on inadequate statistical foundations.

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Juvenile Entrepreneurs[1]

Leroy C. Gould

ABSTRACT

This paper examines the relationship between delinquency and four variables: delinquent associations, the perception of opportunity, social class, and achievement motivation. While delinquency is related to all four variables, multivariate analysis suggests that two of the variables, delinquent associations and perceived opportunity, are more likely to be consequences of delinquency than causes of it. Achievement motivation, which apparently precedes delinquency and is positively related to it, is so related only for lower- and working-class boys. The author argues that this relationship probably results from the sensitivity of law enforcement agents to high achievement motivation related behavior rather than from any inclination of highly motivated lower-class youths to commit excessive amounts of crime.

A bad omelet spoils a good egg. Any parent knows this and, acting accordingly, does not let his children play with other children who are bad. He does not even let them play with those whom he just suspects of being bad.

Every parent knows too that ambition is good but that thwarted ambition can be dangerous. Therefore, the ideal parent trains his children to be ambitious and then does all he can, often at considerable personal sacrifice, to make sure that these ambitions can be fulfilled.

This is American folk theory, and while not all Americans hold to it, large numbers surely do. At least most American parents behave as if these were two of the propositions in their theory of child rearing, and, to the extent that they want their children to grow up respectably, they try to influence their children's choice of associates and to structure their pattern of opportunities and rewards.

These two propositions, that bad associates can make a good child go bad and that thwarted ambition leads to frustration and deviance, have been cornerstones of American criminological theory for many years. The notion that bad associates spoil a good child has been articulated most clearly by Edwin Sutherland in his now well-known theory of "differential association,"[2] which, in its simplest form, states that a person will be delinquent in proportion to the duration, priority, and intensity of contacts which he has with people who are carriers of delinquent norms and criminal techniques. The idea that frustrated ambition leads to deviant behavior, and to delinquency as one form of deviant behavior, has been stated forcefully by Robert Merton[3] and has been reiterated and elaborated in what are two of the most important theories of delinquent gangs.[4] The proposition states that delinquency (as a special class of deviance) results from a social situation in which certain persons are induced to hold aspirations

[1] I wish to express my appreciation to the Ford Foundation who made this study possible through its small-grants program. I also wish to thank Clarence Schrag, who directed the research project; Albert Cohen, Kai Erikson, J. Zvi Namenwirth, and Roy Treadway, who read and criticized an early draft of the paper; and Andrew Walker and Delbert Elliott, who rendered research assistance.

[2] Edwin H. Sutherland and Donald P. Cressey, *Principles of Criminology* (7th ed.; Philadelphia: J. B. Lippincott Co., 1966), pp. 77–98.

[3] Robert K. Merton, *Social Theory and Social Structure* (rev. ed.; Glencoe, Ill.: Free Press, 1957), chaps. iv and v.

[4] Albert K. Cohen, *Delinquent Boys* (Glencoe, Ill.: Free Press, 1955); Richard Cloward and Lloyd Ohlin, *Delinquency and Opportunity* (Glencoe, Ill.: Free Press, 1960).

which the social system is not geared to fulfill.[5]

The amount of empirical evidence in support of the differential association hypothesis, while not overwhelming, is nonetheless impressive.[6] There is also some indirect evidence (although surprisingly little direct evidence) in support of the thwarted aspirations hypothesis.[7]

This evidence, however, shows only that there is an association between delinquency and differential association and thwarted aspirations; it does not show, necessarily, that the causal direction implied in the two propositions is correct.[8] The association found between delinquency and these two variables could be spurious, the direction of causality could be the opposite of the one stated in the propositions, or there could be any number of more complex interactions involved.

That delinquency might not be the dependent variable in these systems is implied by the works of Kai Erikson and Howard Becker.[9] These two authors, although not particularly concerned with the causes of delinquent behavior, are concerned with its consequences, and it is clear from their arguments that two likely consequences of getting into trouble with the law are that a youth's pattern of associations will be affected and that his chances to get ahead in society will diminish. In other words, while Erikson and Becker do not claim, necessarily, that thwarted aspirations and differential associations will not lead to delinquency, their argument does imply that once a youth has become labeled "delinquent" there will likely be few people with whom to associate except others who have been similarly labeled, and once a youth has gotten into trouble with the law he is likely to find that this has closed social doors to the realization of his aspirations.

Since two theories claim that delinquent associations and thwarted aspirations lead to delinquency while another theoretical position suggests that these may be consequences of delinquency,[10] it becomes important to know just how these variables and delinquency are related—not just whether or not they are associated with delinquency, but whether they are causes of it, consequences of it, or both.

Ideally, a test of this issue would involve data gathered by a longitudinal or experimental study. Unfortunately, such data are not presently available, and it would take some time to develop them. In the mean-

[5] All authors do not agree about what the most important aspirations are. Cohen, *op. cit.*, for example, speaks of status as being the important goal which is restricted, while Cloward and Ohlin, *op. cit.*, speak in terms of socially prescribed goals of success. While it is conceivable that these subtle differences may be important, it still seems reasonable to proceed on the assumption that, in general, all of these authors employ the same proposition regarding the consequences of thwarted aspirations. (In this same light, it is interesting to note that one of the major propositions employed by psychologists to explain deviance is of a very similar form, that is, that deviance results from socially induced inhibitions on innate human drives such as sex or aggression.)

[6] See Sutherland and Cressey, *op. cit.*

[7] Good summaries of indirect evidence may be found in Cohen, *op. cit.*, pp. 21–49, and Cloward and Ohlin, *op. cit.* One relatively direct test of the hypothesis is reported in: Erdman Palmore and Phillip Hammond, "Interacting Factors in Juvenile Delinquency," *American Sociological Review*, XXIX (December, 1964), 848–54.

[8] For a detailed discussion of the distinction between theoretical propositions and their mathematical expression, see Hubert M. Blalock, Jr., *Causal Inferences in Non-experimental Research* (Chapel Hill: University of North Carolina Press, 1964), chap. ii.

[9] Kai Erikson, *Wayward Puritans* (New York: John Wiley & Sons, 1966); Howard S. Becker, *Outsiders* (Glencoe, Ill.: Free Press, 1963).

[10] It should be pointed out that the term "delinquency" means different things in these different theories. Conventional delinquency theory has defined this term to mean all acts which are against the law, while Erikson and Becker use the term to refer to those acts which someone in society (particularly the society's appointed agents of social control) has perceived as being against the law. Since all of the evidence in support of the differential association and differential opportunity systems hypotheses is derived from data representing the perceptions of social agents of control (e.g., police or juvenile court records), the two concepts become operationally very similar.

time, however, a somewhat weaker statistical analysis of cross-sectional data can be conducted which may shed at least some light on the issue. This is the kind of analysis which this paper presents. The cross-sectional data used in the analysis were gathered in 1961 with the specific purpose of testing Merton's and Sutherland's propositions.[11] No attempt was made at that time to trace the likely causal sequences in the relationships; such an analysis did not then seem important.

It turns out that the current analysis needs more variables than the three mentioned so far. In particular, it needs some variables which are known to be antecedent to delinquent associations, thwarted aspirations, and delinquency. Two variables are used for this purpose: achievement motivation and social class. Social class has long played an important role in delinquency theory; achievement motivation is new.

GATHERING INFORMATION ABOUT DELINQUENTS

The data in this study were provided by 217 boys attending two high schools in the central part of Seattle, Washington. At the time of the study, 119 of the boys had juvenile court records; ninety-eight did not.[12] Those with court records include all the boys from those two schools who had come before the court for other than traffic violations or matters involving dependency or child neglect. Those without a court record are a random sample of the 1,603 boys in these two schools who were not listed in the juvenile court's files.

Geographically, the two high school districts encompass a heterogeneous population including sizable portions of the major Negro and Oriental residential areas as well as a large white population, a significant portion being middle and upper class. In general, all major racial and socioeconomic groups from the city are represented in the sample, although Negroes, Orientals, and lower-class whites are overrepresented.

Those in the sample completed an anonymous questionnaire during school time under the direction of research personnel from outside the schools.[13] The boys were given all assurances that school personnel would never see the completed questionnaires, that all answers would be handled with the strictest of confidence, and that questionnaires of individual persons would not, and could not, be identified once they had been completed.[14]

Five of the variables gathered by these procedures are included in this analysis: (*a*) social class, determined from the occupation of the head of household and classified as (1) professionals and semiprofessionals; (2) proprietors; (3) clerical, sales, and kindred workers; (4) craftsmen, foremen, and kindred workers; (5) protective service workers, operatives, and kindred workers; and (6) farm laborers, service workers, and laborers;[15] (*b*) achievement motivation, measured by a graphic projection form;[16]

[11] Leroy C. Gould, "Delinquency and Community Opportunity Structure" (Seattle: unpublished M.A. thesis, University of Washington, 1962). See also Delbert Elliott, "Delinquency and Perceived Opportunity," *Sociological Inquiry*, XXXII (Spring, 1962), 216–26.

[12] This number excludes three boys, one with a court record and two without, who did not provide usable questionnaire information.

[13] Questionnaires were administered to groups of less than ten and not in regular classroom groupings. Each respondent was paid $1.00 for his efforts.

[14] Information gathered from other than questionnaire sources (e.g., juvenile court record) was assembled before administering the questionnaires, precoded in numerical form, and entered on each respondent's questionnaire. Once a respondent got the correct precoded questionnaire, his name was never again associated with his data.

[15] Occupational classifications were made according to the classifications suggested by C. C. North and Paul K. Hatt, "Occupational Status and Prestige," in Robert O'Brien *et al.* (eds.), *Readings in General Sociology* (Boston: Houghton Mifflin Co., 1957), pp. 352–58.

[16] Elliot Aronson, "The Need for Achievement as Measured by Graphic Expression" (unpublished M.A. thesis, Wesleyan University, Middletown, Conn., 1956). Achievement motivation may be defined as the drive to compete against a standard of excellence (see David McClelland, *The Achieving Society* [Princeton, N.J.: Van Nostrand Co., 1961], esp. chap. ii).

(*c*) delinquent associations, measured by a Guttman scale including items involving the number of juvenile and adult criminals known and how well they were known;[17] (*d*) perceived opportunity, measured in terms of educational aspirations and expected educational attainment;[18] and (*e*) delinquency, determined by a juvenile court record, or the self-admission that the respondent had been "in trouble with the police for other than a traffic violation."

THE STRATEGY OF ANALYSIS

Estimating population parameters.—These data were gathered for the purpose of comparing differences between delinquents and non-delinquents (that is, between those with and without a juvenile court record). Therefore, the sampling design which included all boys with a court record, but only a random sample of those without a record, presented no particular statistical problems and had the advantages of reducing the large non-delinquent population to manageable research proportions while retaining full information on the smaller delinquent population.

While such a design is appropriate for analyzing differences between delinquent and non-delinquent populations, it is not appropriate for analyzing relationships within the total population which do not involve delinquency as one of the variables in the relationship.[19] Because such an analysis is now necessary, in order to test the likely causal sequences of the six variables, some alterations have to be made in the original sample.

The simplest alteration would be to draw a random sample of those with a court record (who originally constituted a complete enumeration), using the same sampling ratio used for those without a court record. This procedure has the disadvantage, however, of reducing the total number of cases for analysis to less than the number which would be required for computing reliable partial relationships, the procedure to be employed in this analysis. Because of this limitation, an alternative procedure has been used: those data which represent a sample have been inflated by a factor which is the inverse of the original sampling ratio (1/16). While this procedure makes it almost impossible to compute appropriate tests of statistical significance to guard against sampling error,[20] the procedure does give a good estimate of the population parameter. It is this estimate of the total popu-

[17] The scale ranged from a low of zero, those who knew no juvenile or adult offenders, to a high of four, those who knew both juvenile and adult offenders at least well enough to know them by name. The coefficient of reproducibility, using the Goodenough technique of scale construction, was .94, and the minimum marginal reproducibility was .73. For a discussion of the Goodenough techniques of Guttman scale construction, see Allen L. Edwards, *Techniques of Attitude Scale Construction* (New York: Appleton-Century-Crofts, 1957), chap. vii.

[18] If the respondent indicated that he expected to achieve the educational level which he said was necessary for "success in life," he was rated as having high perceived educational opportunity; if he indicated that he thought he would have to leave school before attaining this level of schooling, he was rated low. The study also included measures of expected occupational opportunity and measures of expected opportunity within the school system. This information, for a variety of technical reasons, is difficult to analyze and present in the restricted space of this article. Since the findings using these other measures of perceived opportunity are comparable to the findings using the educational measure, I am assuming that this one measure will suffice as a general index of perceived opportunity.

[19] The strength of γ, the measure of association which is used in this analysis, is not affected so long as one of the variables in the comparison is the variable which defined the populations which were sampled according to different sampling ratios. Whenever this variable is not one of the variables being compared, however, γ is affected to the extent that the variable which defined the sampling ratios is related to the variables being compared.

[20] Since the number of cases in any one cell of a table would include both cases which represent a complete enumeration and cases which represent a sample, and since the ratio of the two would vary from cell to cell in any table, an appropriate test of significance becomes very difficult. Since I am dealing with a sample of 217 cases, however, and with γ's of fairly large size, it seems unlikely that any of the findings reported in this paper would not in fact be statistically significant at the conventional .05 level.

lation of the two high schools which provides the data for the analysis to follow.

Testing for causal sequence.—Of a number of techniques which have been developed to test causal sequences using survey data, I have chosen one which relies on partial relationships and is in keeping with the ordinal level of measurement used in this study.[21] In general, two variables are considered at a time, partialling on a third variable. Whenever partialling on the third variable eliminates the relationship between the other two variables, it can be assumed that the control variable is likely either antecedent to both of the other variables or is intervening between them. If the control variable reduces, but does not eliminate a relationship, it can be assumed that the control variable is likely either to be the first in a three-variable causal chain or that the relationship between the two remaining variables is "partially" spurious.

TABLE 1

THE DEGREE OF ASSOCIATION (γ) BETWEEN DELINQUENT ASSOCIATIONS, PERCEIVED OPPORTUNITY, AND DELINQUENCY

	Perceived Opportunity	Delinquency
Delinquent associations	−.23	.53
Perceived opportunity...	....	−.64

While this type of analysis cannot by itself completely specify the causal sequences between all five variables, it will make some possibilities seem less likely. This, added to the fact that two of the variables, social class and achievement motivation, are already known to be antecedent to the others,[22] makes it possible to reach some reasonably firm guesses regarding the likely causal pattern of these five variables. What is this probable pattern?

DELINQUENCY: CAUSE OR EFFECT?

The first issue to be considered is whether delinquency seems to result from delinquent associations and a lack of perceived opportunity or to lead to these conditions. As Table 1 shows, there is a fairly strong association between delinquent associations and delinquency and between perceived opportunity and delinquency, but only a weak association between delinquent associations and perceived opportunity.[23] This could mean either that delinquent associations and perceived opportunity, which are weakly associated with each other, lead to delinquency, or that delinquency leads to both and the degree of association found between them is spurious.

These alternatives can be tested by computing the relationship between delinquent associations and perceived opportunity, partialling for delinquency. If it is the case that the relationship is spurious, then it should disappear under these conditions of control; if it is not spurious, the control should have no effect. Partialling does have an effect; the partial relationship[24] between

[21] Paul Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," in Paul F. Lazarsfeld and Morris Rosenberg (eds.), *The Language of Social Research* (Glencoe, Ill.: Free Press, 1955), pp. 115–25. See also Blalock, *op. cit.*

[22] That social class (especially as determined by occupation of head of household) is antecedent seems self-evident. That the same is true for achievement motivation is not so apparent, and I rely on the findings of others who have worked with achievement motivation who find that, in general, it is developed in the first years of life through interaction with parents (see David C. McClelland, *op. cit.*, chap. ix).

[23] Throughout this study γ is the measure of association used. For discussion and interpretation of γ, see Leo A. Goodman and William H. Kruskal, "Measures of Association for Cross Classification," *Journal of the American Statistical Association*, XLIX (December, 1954), 732–64; and Herbert L. Costner, Criteria for Measures of Association," *American Sociological Review*, XXX (June, 1965), 341–53.

[24] James A. Davis, "A Partial Coefficient for Goodman and Kruskal's Gamma," *Journal of the American Statistical Association*, LXII (March, 1967), 189–93. Davis acknowledges that the appropriateness of this measure is questionable under circumstances where interaction is present, but he offers no test for interaction. Lacking clear criteria for testing against interaction, I have had to proceed by inspection alone. In some cases there is interaction; in other cases the inner action appears to be minimal. It is only under conditions in which in-

delinquent associations and perceived opportunity, controlling for delinquency, is .08, almost zero.

While this seems to contradict the proposition that both delinquent associations and perceived opportunity lead to delinquency, it does not eliminate the possibility that one or the other of these variables leads to delinquency and that the remaining one is caused by delinquency. Delinquency, in other words, might be an intervening variable between delinquent associations and perceived opportunity. The likelihood of this possibility cannot be tested using only these three variables, but it can be tested when social class and achievement motivation are included in the system.[25] Social class and achievement motivation, although

TABLE 2

THE DEGREE OF ASSOCIATION (γ) BETWEEN ACHIEVEMENT MOTIVATION, SOCIAL CLASS, DELINQUENCY, DELINQUENT ASSOCIATIONS, AND PERCEIVED OPPORTUNITY

	Delinquent Associations	Perceived Opportunity	Delinquency
Achievement motivation	.30	−.17	.44
Social class	−.42	.39	−.34

not related to each other ($\gamma = -.05$), are related to delinquent associations, perceived opportunity, and delinquency (see Table 2).[26]

teraction appears to be a relatively small factor that I have computed partial γ.

[25] For a similar type analysis, see Peter Blau, "Determining the Dependent Variable in Certain Correlations," *Public Opinion Quarterly*, XIX (Spring, 1955), 100–105.

[26] It might be assumed that race also could serve this analytic role, but it cannot. Race, at least in this study, was not related very strongly to either delinquency or perceived opportunity, and the small relationship which did exist vanished when social class and achievement motivation were introduced as control variables. Race was, however, strongly related to achievement motivation and social class.

In the case of achievement motivation, the pattern of zero-order relationships itself makes it seem unlikely that either delinquent associations or perceived opportunity leads to delinquency. If either did, then the relationship between that variable and achievement motivation should be stronger than the relationship between achievement

TABLE 3

PARTIAL RELATIONSHIPS (PARTIAL γ) BETWEEN ACHIEVEMENT MOTIVATION, SOCIAL CLASS, DELINQUENT ASSOCIATIONS, AND PERCEIVED OPPORTUNITY, CONTROLLING FOR DELINQUENCY

	Delinquent Associations	Perceived Opportunity
Achievement motivation	.23*	.05
Social class	−.35	.37

* Controlling for delinquency and race, simultaneously, reduces this partial relationship to .03.

motivation and delinquency.[27] As Table 2 indicates, achievement motivation is most strongly related to delinquency, which implies that the main causal sequence must run from achievement motivation to delinquency and thence to delinquent associations and perceived opportunity.

This interpretation is supported when delinquency is used as a control variable. The partial relationships between achievement motivation and perceived opportunity and between achievement motivation and delinquent associations, controlling for delinquency, are weaker than the zero-order relationships between the same variables (see Table 3).

Where social class is used as the antecedent variable, this interpretation is not so well supported. The zero-order relationship between social class and delinquency is not stronger than the zero-order relationships between delinquency and perceived opportunity and between delinquency and delinquent associations. Also, using delinquency as a partialling variable does not reduce the relationships between social class and delin-

[27] See Blalock, *op. cit.*, chap. iii.

quent associations and social class and perceived opportunity nearly so much as it did in the case of achievement motivation.[28]

In general, however, the over-all pattern of association and partial relationships suggests that delinquent associations and perceived opportunity are probably more likely to be consequences of delinquency than causes of it. While this does not rule out the possibility of more complicated causal mechanisms (for example, some kind of reciprocal interplay between the variables), the study makes it seem likely that the major causal effects run from delinquency *to* delinquent associations and from delinquency *to* perceived opportunity, rather than the other way around.

To the extent that this interpretation is correct, it would seem to diminish the importance of differential association and thwarted aspirations as causes of delinquency while at the same time increasing interest in other variables which seem more clearly to lead to delinquency. The two variables in this study which seem to play this role are social class and achievement motivation.

DELINQUENTS AS ENTREPRENEURS

That social class is a causal factor in delinquency has long been assumed.[29] That boys who are endowed with high achievement motivation are also likely to get into trouble with the law comes as somewhat of a surprise. Achievement motivation, after all, is usually thought of as a valued quality and is usually associated with valued forms of behavior. Those with high achievement motivation are more likely to be inquisitive and aggressive, they are more likely to be successful in their work, and they are likely to choose entrepreneurial occupations.[30] Why should it be the case that they are also more likely to be delinquents?

Before attempting to answer this question, it is important to point out that the relationship between achievement motivation and delinquency is not uniform for all social classes. The relationship (γ) is .54 for lower-class boys, .33 for working-class boys, but −.17 for boys who come from middle- or upper-class homes.

The differences in this relationship by class suggests that there is an interaction between achievement motivation and social class in causing delinquency. This interaction can be seen more clearly by comparing the rates of delinquency for boys in each of the categories of class and achievement motivation (see Table 4). The highest rate of delinquency is found among lower-class boys with high achievement motivation. Working-class boys with high achievement motivation have the next highest rate of delinquency. The third highest rate is found among middle- and upper-class boys with low achievement motivation. Lower- and working-class boys with low achievement motivation have relatively low rates of delinquency, and middle- and upper-class boys with high achievement motivation have the lowest delinquency rate. Of all these groups, lower- and working-class boys with high achievement motivation stand out above all others in terms of their high rates of delinquency.

The most immediate conclusion to draw from these findings is that high achievement motivation in combination with lower-class status, or something related to social class—like opportunities for legitimate success—causes boys to commit delinquent acts. This, in fact, was the hypothesis which led me to measure achievement motivation initially. If thwarted aspirations lead to

[28] It is evident that there is considerable interaction between social class, delinquency, perceived opportunity, and delinquent associations, probably too much interaction to be able to interpret partial γ with any degree of confidence. I have retained social class in the analysis despite these difficulties because social class plays an important historical role in delinquency theory and because I want to examine this variable more extensively later in the analysis.

[29] Cf. Donald R. Cressey, "Crime," in Robert Merton and Robert Nisbet (eds.), *Contemporary Social Problems* (2d ed.; New York: Harcourt, Brace & World, 1966), p. 157.

[30] McClelland, *op. cit.*

pressures to deviate, as Merton and others argue, then those boys who are the most highly motivated to achieve their aspirations should be the boys who will feel these pressures the most and consequently engage in the most delinquent activity.

This hypothesis, however, was not confirmed. Not only was social class not strongly related to the perception of opportunity, but the study raised doubts about the proposition that thwarted aspirations led to delinquency. In addition, the specific hypothesis involving the interaction between achievement motivation, perceived opportunity, and delinquency was not confirmed. The relationship between perceived opportunity and delinquency is almost exactly the same for those with high ($\gamma = -.63$) as for those with low ($\gamma = -.64$) achievement motivation scores. Had achievement motivation operated as predicted, that is, had it exacerbated the tensions caused by thwarted aspirations, perceived opportunity and delinquency should have been more highly related among those with high achievement motivation scores.

This hypothesis failing, I am compelled to search for another explanation for the relationship of achievement motivation and social class to delinquency. Rather than continue searching for an explanation of how these two variables might affect delinquent behavior, however, I am inclined to ask another question: how might achievement motivation and social class affect the responses of law enforcement agents to boys who have committed delinquent acts (or who are suspected of having committed delinquent acts)?

Technically, the data of this study are more appropriate for answering this question than they are for answering the question posed earlier (i.e., why do some boys commit delinquent acts?). Delinquency, as operationally defined in this study, is not really a measure of the incidence of delinquent acts but, instead, a measure of whether the boys were ever in trouble with the law. Delinquency was defined as an official juvenile court record or a positive response to the question, "have you ever been in trouble with the police for other than a traffic violation?"

TABLE 4

THE INTERACTION OF SOCIAL CLASS AND ACHIEVEMENT MOTIVATION IN PRODUCING DELINQUENCY

Social Class	Achievement Motivation	Delinquents*	Non-Delinquents	Proportion Delinquent
Low	High	204	192	.52
Working	High	81	208	.28
Middle and upper	Low	23	96	.19
Low	Low	68	304	.18
Working	Low	58	304	.16
Middle and upper	High	19	112	.15

* Includes those with juvenile court record and those who reported having been in trouble with the police.

There is a large literature which questions the use of "official" crime statistics to measure the incidence and distribution of delinquent acts.[31] Since the measure of delinquency used by this study is an "official" measure (i.e., police and court action), I must conclude that this research has only demonstrated that achievement motivation and social class are related to whether a boy will *get into trouble with the law*. Whether

[31] For a review of this literature, see Leroy C. Gould, "Who Defines Delinquency: A Comparison of Self-reported and Officially-reported Delinquency for Three Racial Groups," *Social Problems*, XVI (Winter, 1968), in press.

these two variables are also related to the commission of delinquent acts remains unknown.

That social class is related to whether a boy will get into trouble with the law is well established. Study after study has found that lower-class boys have the highest "official" rates of delinquency. This study confirms this finding, but it confirms it only for boys who have high achievement motivation. Lower- and working-class boys who have low achievement motivation get into trouble with the law at about the same rate as middle- and upper-class boys. Why should this be?

An answer to this question is not provided by the data of this study, but a reasonable answer is suggested by other studies which have examined the responses of law enforcement officials to juvenile offenders. In particular, the study by Piliavin and Briar[32] is revealing, as it finds that deportment is one of the major items that the police consider in deciding how to handle a juvenile case (and the range of discretion available to the police in juvenile cases is wide).[33] Specifically, Piliavin and Briar find that the police are most likely to detain and refer to juvenile court those juvenile suspects who are the most recalcitrant. While this practice is somewhat offset by the seriousness of the offense, police, in general, act most harshly toward boys who are disrespectful of the policeman's authority.

While there is no direct research linking achievement motivation to recalcitrant behavior, such a link does seem possible and worthy of further investigation. Some of the qualities which have been associated with high achievement motivation, in particular aggressiveness and independence, could very easily be defined as recalcitrance.

This might be especially true for lower- or working-class boys (as well as for other minority status persons). These boys are not supposed to be assertive or defiant; they are supposed to be submissive and know their place. Middle- and upper-class families, on the other hand, presumably not only have more power, but it is probably expected that their children will stand up for their rights more forcefully than lower-class children. At least, if they do stand up for their rights, it is not out of keeping with their character.[34]

SOME CAUSES AND CONSEQUENCES OF DELINQUENCY

Whether or not this interpretation is correct, the fact remains that social class and achievement motivation, at least in this study, are related to whether a boy will get into trouble with the law. Indeed, the risk of getting into that unhappy condition is very high for strongly motivated lower-class boys (52 per cent) and quite high for strongly motivated working-class boys (28 per cent).

Whether delinquent associations and thwarted aspirations also lead to delinquency must remain, in the absence of further data, an open question. It does seem to be the case, however, that at least some of the association found between these two variables and delinquency is due to the impact of delinquency on a youth's pattern of associations and on his chances to achieve his legitimate goals.

Should these findings hold up in other studies, this would have important implications for delinquency theory and social policy. First of all, it would require some modification or elaboration of Sutherland's and Merton's theories of delinquency, and second, it would indicate that young, lower-class entrepreneurs in our society face a difficult situation.

[32] Irving Piliavin and Scott Briar, "Police Encounters with Juveniles," *American Journal of Sociology*, LXX (September, 1964), 206–14.

[33] *Ibid.*

[34] For a classic discussion of how the same behavior may be viewed quite differently in people from different social positions, see Robert K. Merton, "The Self-fulfilling Prophecy," *Antioch Review*, VIII (Summer, 1948), 193–210.

While the theoretical modifications would, presumably, be simple enough, modification of the class system might be considerably more complicated. If it is the case that high achievement motivation helps middle- and upper-class boys get ahead in life but causes lower- and working-class boys to run a higher risk of getting into trouble with the law, high achievement motivation would seem to be self-defeating for the lower- and working-class boys, especially since getting into trouble with the law seems to restrict the chances of attaining the legitimate social goals that highly motivated boys are presumably inclined to pursue. It would seem to be a risky business, but it would add credence to one bit of mercurial wisdom expressed by more than one successful businessman when viewing the plight of a delinquent boy: "There, but for the grace of God [and the class system], go I."

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Changing Courtship Patterns in the Popular Song[1]

James T. Carey

ABSTRACT

A content analysis of popular lyrics drawn from song magazines and accepted rankings reveals a change in perspective on boy-girl relationships over the past eleven years. The new orientation prizes autonomy in personal relations and is most clearly portrayed in rock and roll lyrics. The career of the affair is traced from its inception to its final dissolution. Proportionally fewer lyrics deal with boy-girl relationships today than in the mid-1950's. A wider range of concerns are evident and raise questions about the individual's relation to the social order. The questions may reflect the preoccupations of a growing number of disaffected young people who constitute the audience for the new lyrics, or the emergence of more democratic controls in songwriting.

Radio disc jockeys and rock and roll fans date the emergence of rock and roll as a distinct musical style with Bill Haley and the Comets' 1955 recording of "Rock Around the Clock." This musical style was a blend of blues and country western and for the first time introduced mass white audiences to a Negro folk musical form. More recently folk rock and psychedelic rock have appeared as variations within the same basic musical style.[2] The emergence of rock and roll music has provided a vehicle for the development of a new perspective on boy-girl relationships. A look at the changes in song content over the past eleven years reveals marked changes in orientation, not only in the relationship between the sexes but also in the relationship of young people to the larger social order.

Any analysis of lyrics must take into account that only the verbal content, a secondary aspect of the music, lends itself to social examination. A superficial comparison of the mood of popular music in the fifties to that of the sixties suggests that the earlier music was languid, searching, "sweet"; the music of the late sixties is more sensual, direct, sexual, and "gutsy."[3] Lyrics may reveal general values, but this is not necessarily the stated reason for listening to them.

[1] Grateful acknowledgment is made to Charles Bordin, Richard Janvier, and Anthony Poveda for their technical assistance in the preparation of this paper.

[2] The rock and roll songs of the late 1950's, characterized today as *old-style rock*, emphasized standard band or orchestra instruments like the trumpet or violin. There were no background group effects. Tommy James's "Hanky Panky" and Nancy Sinatra's "These Boots are Made for Walking" are more recent examples of old-style rock. *Rock and roll, popular style*, is a direct descendant from old-style rock. There is a noticeable or emphasized use of electronic instruments, especially guitars and organs. There is also an interplay between backup groups and the lead singer in vocal pieces and between the lead instruments and backup instruments in instrumentals. The Lovin' Spoonful's "Summer in the City" and the Standell's "Dirty Water" are examples of popular style. *Folk rock* also uses electric sound, but without heavy effects and without the backup vocal groups and interplay that occur in popular-style rock and roll. The Mamas and the Papas' "I Saw Her Again" and Simon and Garfunkel's "I Am a Rock" are examples of folk rock. *Psychedelic rock* emphasizes electric, amplified instruments. Speaker effects may be used, especially the organ. There is no vocal interplay between the backup and lead. There is interplay between electronic effects and vocal. "Over Under Sideways and Down" by the Yardbirds and "You're Gonna Miss Me" by the 13th Floor Elevator are examples of psychedelic rock. For the purposes of this analysis, rock and roll includes all four styles.

[3] See C. Keil, "Motion and Feeling through Music," *Journal of Aesthetics and Art Criticism* XXIV No. 3 (Spring, 1966), 337-50, for a suggested classification of musical styles based on the combination of techniques used.

It seems to be the nature of the music itself, and not the vocal or lyrical aspects alone, which accounts for its popularity. The words are only part of the total sound and are responded to as such.

The first serious social-scientific inquiry into popular music was conducted by Adorno in 1941.[4] He analyzed the fundamental character of popular songs and the role of musical agencies in standardizing certain musical forms. Peatman[5] differentiated themes in popular song lyrics through the use of content analysis. He found that all dealt with "love" and could be classified into three major types of lyrics: the "happy in love" ballad, the "frustrated in love" song, and the "novelty song with sex interest." Several years later, Riesman[6] explored the way in which music and musical opinions are used as a form of communication. Horton[7] analyzed the themes of popular songs as they related to the relationship between boys and girls. Johnstone and Katz[8] concluded that preferences in popular music among teen-age girls varied according to popularity among peers and neighborhood norms. They pointed to the role and explored the influence of adolescent peer groups on tastes in popular music. They concluded that the peer group may influence and restrict preference in disc jockeys and, consequently, in the music to which its members will listen.

Content analyses of lyrics conducted by Peatman in 1944, by Horton in 1957, and the one discussed in this paper reveal that the overwhelming majority of songs are concerned with courtship and the boy-girl relationship. The courtship situation is viewed differently, however. This becomes clear by noting the difference in perspective between the lyrics analyzed by Horton and the present study.

We will rely on Horton's analysis of popular song lyrics for the verbal content of 1955 songs. His data were drawn from the June, 1955, issue of four magazines: *Hit Parader*, *Song Hits Magazine*, *Country Song Roundup*, and *Rhythm and Blues*. We have tried to repeat Horton's analysis by looking at the same magazines during a two-month period in the summer of 1966.[9] This was not completely possible, since *Rhythm and Blues* had discontinued publication several years earlier.[10] In addition, we have used *Billboard* for the national top thirty listings in the same period and the top thirty listings of one radio station in San Francisco for the Bay Area. Both the national listing and the Bay Area listing rate songs in terms of popularity. Our analysis should provide us

[4] T. W. Adorno, "On Radio Music," in *Studies in Philosophy and Social Science* (New York: Institute of Social Research, 1941), IX, 17–48.

[5] J. Peatman, "Radio and Popular Music," in *Radio Research: 1942–43*, eds. P. Lazarsfeld and F. Stanton (New York: Duell, Sloan & Pearce, 1944), pp. 335–93.

[6] D. Riesman, "Listening to Popular Music," in *Mass Culture: The Popular Arts in America*, eds. B. Rosenberg and D. M. White (Glencoe, Ill.: Free Press, 1957), pp. 408–17.

[7] D. Horton, "The Dialogue of Courtship in Popular Songs," *American Journal of Sociology*, LXII (May, 1957), 569–78.

[8] J. Johnstone and E. Katz, "Youth and Popular Music: A Study in the Sociology of Taste," *American Journal of Sociology*, LXII (May, 1957), 563–68.

[9] The songs included in these magazines are drawn partly from a national popularity listing and partly from staff view of what songs are likely to gross well in retail sales. Hence, in any given issue, at least 30 per cent are not drawn from any popularity chart.

[10] *Billboard* does include a separate rhythm and blues rating which lists songs popular among Negroes, based on radio-air play from Negro radio stations and retail sales in Negro neighborhoods. A preliminary check of the content of the songs, despite the distinctively different musical style, indicates that the themes do not differ substantially from the three song magazines, with one important qualification. There are few lyrics which suggest that women are on a pedestal or that sexual interests are anything less than normal. Horton, *op. cit.*, noted that there was nothing distinctive about the stages of the courtship relationship in his analysis of rhythm and blues lyrics. However, a more careful examination might reveal that the double standard characteristic of white popular music in the fifties was absent in the rhythm and blues lyrics of the same period. In short, a case could probably be made that the "new" notions about boy-girl relationships in today's lyrics represent a general acceptance of Negro notions by a white audience.

with a comparison of courtship patterns at two different time periods based on a content analysis of song magazine listings. Further, it should indicate, through the use of two more popularity listings in addition to the song magazines, the dramatic shift in value preferences of young people.[11] These value preferences relate not only to idealized boy-girl relationships but also to a wider range of concerns.

Table 1 describes the sample. It shows the distribution of songs by musical style in the 1966 song magazines and popularity listings. The distribution is somewhat skewed by including *Country Song Roundup*, since this magazine focuses overwhelmingly on one musical style—country western. If we excluded that, the proportion of rock and roll lyrics from the other four sources would increase from 52 to 62 per cent.

TABLE 1

LYRIC MUSICAL STYLE, 1966

STYLE	SONG MAGAZINES AND POPULARITY LISTINGS					
	Country Song Roundup	*Hit Parader*	*Song Hits*	*Billboard*	KYA	Total (Excluding Duplication)
Rhythm and blues	....	20% (19)	22% (14)	23% (13)	22% (12)	17% (38)
Rock and roll	....	60% (56)	64% (41)	62% (35)	62% (34)	52% (119)
Country western	90% (55)	7% (7)	7% (4)	3% (2)	4% (2)	24% (54)
Other	10% (6)	13% (12)	7% (4)	12% (7)	12% (6)	7% (16)
Total	100% (61)	100% (94)	100% (63)	100% (57)	100% (54)	100% (227)

A total of 227 different songs were analyzed, of which 52 per cent were classified as rock and roll.

[11] The inclusion of the national popularity ratings is an attempt to overcome the limitations of the song magazines as an indicator of youth preferences. The ratings are based on radio-air play and retail sales. The inclusion of the San Francisco ratings is an attempt to suggest that the new values being celebrated are related to particular groups, particular musical styles, and particular centers of popular culture. C. Hall, in "Detroit and L.A. Sales 'Happening Places,'" *Billboard*, LXXVIII (July 2, 1966), 1, reports that San Francisco had fifteen original record breakouts, i.e., records that first hit the top-thirty chart there, during the first six months of 1966, of which two went on to reach *Billboard*'s "Hot 100" chart. This was in contrast to New York, with sixteen of which six went on to reach the chart, and Los Angeles, with sixteen original record breakouts of which eight went on to reach the hit chart. Hall goes on to say: "In a similar survey last year, New York took all the honors, not only having the most original breakouts —17—but having the most that reached the charts —19. San Francisco had been second with 18 breakouts that reached the chart" (*ibid.*). *Billboard* classifies the country into twenty-two major markets, which include all the major cities in five major regions: East, South, Midwest, Southwest, and West.

Lyrics were classified in a general way in terms of whether they reflected "older" or "newer" values. Older values were found in lyrics which enjoined, implicitly or explicitly, the acceptance of conventional values, for example, romantic notions about boy-girl relationships or fatalistic acceptance of the demands placed on one by the larger community. Lyrics that expressed anxiety over social change were also classified as representing older values. Newer values were found in lyrics which seemed to advocate or imply a more autonomous relationship between the sexes and/or criticized conventional society because of its misplaced preferences.[12] If we look at all the

[12] Three judges sorted the lyrics into three categories: old values, new values, and other. Those classified as "other" included lyrics whose value content was neutral, e.g., Barry McGuire's "On A Cloudy Summer Afternoon," or songs which did not have enough verbal content to permit classification as old or new, e.g., Robert Parker's "Barefootin'." Disagreement among the judges occurred on those lyrics which were mixed, partly expressing old values, partly new. The final decision on whether the lyrics expressed old or new values was made on

lyrics in our 1966 sample, we note that the new values are being communicated primarily through rock lyrics.

Table 2 dichotomizes lyrics into two general categories for illustrative purposes. It shows that the emphasis on autonomy in personal relations and in relation to the larger community is distinctively related to one musical style. The largest proportion of rock and roll lyrics representing older values is part of the subclassification "old style rock." If we separated them from the general rock and roll category, the point of Table 2 would be made even more dramatically: the proportion of lyrics representing newer values increases from 68 per cent to 83 per cent.[13] Our illustrations of the shift in perspective on boy-girl relationships will be primarily drawn from rock and roll lyrics, since it is here that the new outlook is most clearly stated.

Horton found that 83.4 per cent of the popular songs in 1955 were conversational songs about love.[14] The proportion of conversational songs about love in three of the same magazines dropped in 1966 to 64.7 per cent, as shown in Table 3.

The decline in conversational songs about love is more strikingly portrayed in looking at the national popularity rating reported by *Billboard* for the same period and at San Francisco's KYA listing.

Radio air play combined with retail sales forms the basis for the *Billboard* and KYA listings. Therefore, Table 4 highlights not only the declining interest in love lyrics from 1955 to 1966 but also the preferences of the youth audience. As we get closer to the center of popular culture, the proportion of love lyrics shows a marked decline to little

TABLE 2

COMPARISON OF VALUES REPRESENTED IN MUSICAL STYLES

	Country Western	Rhythm and Blues	Rock and Roll	Other*
Old values...	100% (54)	65.8% (25)	29.9% (35)	75% (12)
New values..	....	29.0% (11)	68.1% (80)	19% (3)
Other.......	....	5.2% (2)	2.0 (2)	6% (1)
Total.....	100% (54)	100% (38)	100% (117)	100% (16)

* Includes "easy listening, popular music" sound; songs in jazz or swing style of the early fifties, chorales, and straight folk songs.

TABLE 3

PROPORTION OF LOVE SONGS IN 1955 AND 1966*

	1955	1966
Love songs in the conversational mode.........	83.4% (196)	64.7% (108)
Love songs not in the conversational mode....	3.8% (9)	4.8% (8)
Themes other than love and courtship	12.8% (30)	30.5% (51)
Total.........	100.0% (235)	100.0% (167)

* $\chi^2 = 20.02$, significant beyond .001 level.

the basis of which value was *most* represented in counting the lines.

[13] A further indication that a specific message is being communicated through rock and roll lyrics is the fact that slightly over 65 per cent of the songs were written by members of the group which recorded them. Lyrics which are not rock and roll, those celebrating older, more conventional values, were characteristically not written by the groups which recorded them. Only 13.3 per cent of this type of lyrics were written by members of the group which sang them.

[14] Horton, *op. cit.*, distinguishes between lyrics in the conversational mode and those not. By the former he refers to songs written in the mode of direct address. The content can be an appeal, request, demand, complaint, or reproach, soliciting some kind of response as though the songs were fragments of dialogue. Love songs not in the conversational mode are primarily narrative and descriptive ballads on love.

more than half of the songs represented.[15]

Horton arranged his themes in terms of various stages of the love relationship. He called these stages the "drama of courtship." Basically, there were five:

1. The prologue to the courtship expresses the anticipations and longings of those who seek love affairs.

TABLE 4

PROPORTION OF LOVE SONGS BY POPULARITY RATING IN 1966

	*Billboard**	KYA*
Love songs in the conversational mode..........	62.9% (36)	52.0% (28)
Love songs not in the conversational mode..........	....	....
Themes other than love and courtship	37.1% (21)	48.0% (26)
Total..........	100.0% (57)	100.0% (54)

* *Billboard* and KYA were combined and compared with the 1955 data in computing $\chi^2 = 40.50$, significant beyond the .001 level.

2. The initiation of courtship (Act I) describes the explosive beginning of the affair.

3. The honeymoon (Act II) portrays the euphoric phase of the courtship.

4. The downward course of love (Act III) depicts the appearance of forces hostile to love's happiness.

5. The all-alone stage (Act IV) laments over lost love and the ensuing loneliness.

How adequately can these phases characterize the 1966 songs? If we compare the two periods, we note that most of the lyrics about love can, with some difficulty, be classified using Horton's categories, that is, there are discernible stages in the relationship from the prologue through the lover's final isolation.[16] However, the characteristics of each stage or their content are different. The main difference is in terms of activity-passivity. The actors in the newer lyrics control their own destinies and are not fatalistic about the affair. To make a comparison between 1955 and 1966 lyrics requires a slight reinterpretation of the earlier scheme to include a more active outlook on the love affair at each phase. Even with some modification, however, it was not possible to include 24 per cent of the lyrics drawn from the 1966 song magazines (see Table 5).

If we look at the top thirty songs in the national listing included in *Billboard*'s popularity rating and San Francisco's KYA ranking, we note that an even larger proportion of lyrics do not fit into Horton's categories (See Table 6).

The shift in preoccupation is more dramatically noted in looking at San Francisco's KYA popularity rating for the same period. It is in the national popularity listings and rankings selected from key centers of popular culture that we would expect the new orientation toward boy-girl relationships to be most sharply drawn.

[15] J. Simmons and B. Winograd, in *It's Happening: A Portrait of the Youth Scene Today* (Santa Barbara, Calif.: Marc Laird Publications, 1966), p. 163, note the importance of San Francisco (and the West Coast generally) as an innovator in popular music: "A survey of the record buying public's tastes indicates that except for a few distinct styles ... the music of today goes from West to East, no longer traveling from New York's Tin Pan Alley to the ... hills of San Francisco."

[16] See Horton, *op. cit.*; to eliminate the possibility that the difference between the 1955 and 1966 data was simply a matter of reliability between two judges, a lengthy interview was conducted with one of Horton's judges. The author was instructed in the code originally used by Horton, and a sample of 1966 lyrics were sorted using the 1955 scheme. This constituted a reliability check of the author's assignment to specific categories. With the assistance of one of Horton's researchers, a further check was made in reclassifying the 1955 data into the new code. Almost 50 per cent of Horton's lyrics could not be classified. A count of the 1955 and 1966 lyrics revealed the following breakdown using the revised code:

	1955	1966
Active search......	19.7% (38)	32.9% (57)
Happy stage	9.7% (19)	17.1% (29)
Breakup..........	8.6% (17)	33.5% (58)
Isolation..........	14.7% (29)	16.4% (28)
Other............	47.4% (93)	....
Total..........	100.0% (196)	100.0% (172)

COURTSHIP IN 1966

The large proportion of song lyrics not included in Horton's categories suggests a reformulation of the phases of the love relationship to take into account a 1966 view of it.

Our sequence starts not with the longing for a relationship or the reveries associated with love but with looking for an affair. The lover is not wishing and dreaming for something to happen to him; he is actively seeking it. The sequence begins with Act I, the *active search*, which includes finding a partner.

Act II portrays the *happy stage* and seems somewhat related to Horton's honeymoon phase. It is different in that there is no implication that the state is a permanent one, nor is it expected to be. The lover is less sentimental and romantic during this happy stage than previously. He is celebrating his enthusiasm over the physical relationship which has been established.

TABLE 5

COMPARISON OF COURTSHIP STAGES IN 1955 AND 1966 SONG MAGAZINES

	1955	1966
Prologue*	5% (9)	1% (1)
Act I: courtship	39% (76)	25% (27)
Act II: honeymoon	9% (19)	14% (15)
Act III: downward course of love	17% (34)	19% (21)
Act IV: all alone	30% (58)	17% (18)
Other	....	24% (26)
Total	100% (196)	100% (108)

* Prologue and Act I were combined in computing $x^2 = 58.52$, significant beyond .001 level.

Act III describes the *breakup*, which occurs when something contaminates the relationship between two people. The breakup occurs because the relationship is not a healthy one and the lovers decide to end it.

Act IV depicts the *isolation* phase, which is viewed as an opportunity to discover one's real self.

Since most of the lyrics which could not be classified under Horton's stages were rock and roll lyrics, it is to these songs we must turn to describe in more detail the new orientation toward boy-girl relationships.

In Act I people actively seek out lovers. They meet semicasually, but with the intention of being lovers. The looking is preceded by a recognition that one needs some release, presumably through a physical relationship. The Mamas and the Papas describe this condition in "Somebody Groovy":

I need somebody groovy,
Someone who really can move me.
Yeah, they gotta move me like they should
And when I find somebody, yeah, I'm
  gonna treat 'em good.
Oh, please, please believe me,
I need someone to relieve me,
Yeah, they gotta move me like they should
And when I find somebody, yeah, I'm
  gonna treat 'em good.

TABLE 6

ADEQUACY OF HORTON'S CATEGORIES TO 1966 POPULARITY LISTINGS

	*Billboard**	KYA*
Songs that fit	67.7% (24)	66.6% (19)
Songs that do not fit	32.3% (12)	33.4% (9)
Total	100.0% (36)	100.0% (28)

* *Billboard* and KYA were combined and compared with the 1955 data in computing $x^2 = 70.96$, significant beyond .001 level.

When love is discussed in rock and roll poetry, it refers to a different phenomenon from romantic involvement. Love seems to be reduced simply to physical desire. This is not viewed as a bad thing—it is a reality that everyone must face. The description of love by the Supremes in "Love Is Like an Itching in My Heart" finds one in the same kind of search as in the previous lyrics:

Love is a nagging irritation
Causing my heart complication
Love is growing infection
And I don't know the correction
Got me rocking and a-reeling and I can't
  shake the feeling
Love is like an itching in my heart
Tearing it all apart
Just an itching in my heart, baby
I can't scratch it

Keeps me sighing, oh, oh,
Keeps me yearning,
Keeps me burning, keeps me tossing,
Keeps me turning,
Keeps me yearning.

The protagonist at first glance seems to be telling someone that she has been bitten by the love bug, that she is in love with him; but she is speaking only about her state of desire, which is not focused on anyone at this point.

The ideology of freedom is spelled out in the song, "Go Where You Wanna Go," and enjoins one to seek out relationships with "whoever you wanna do it with." One enters a relationship with someone who moves him. The Troggs' "Wild Thing" suggests the character of the relationship:

Wild thing, you make my heart sing,
You make everything groovy
Wild thing [*spoken*], Wild thing,
I think you move me,
[*sung*] But I wanna know for sure.
[*spoken*] Come on and hold me tight.
You move me . . .

The girl in these lyrics is viewed instrumentally. Love is depicted as basically physical. People are animals, but happy animals. Romantic involvement is not a necessary ingredient in the relationship. Indeed, it can actually be a hindrance to open, free enjoyment. Dusty Springfield expresses this sentiment in "You Don't Have to Say You Love Me":

You don't have to say you love me,
Just be close at hand.
You don't have to stay forever,
I will understand.

And, a little later in the song:

But, believe me, I'll never tie you down.

Sam the Sham and the Pharaohs, in their "Lil' Red Riding Hood," also emphasize that physical attraction is the main reason for getting involved with someone. They describe her as very desirable, with big eyes and full lips, and they end by saying:

Hey there, Lil' Red Riding Hood,
You sure are looking good
You're everything that a big bad
wolf could want.

The next stage of the relationship, Act II, discusses what occurs after two people have found each other. The affair at this point is very happy. The lovers spend a great deal of time with each other, as Petula Clark states in "I Couldn't Live Without Your Love":

I couldn't live without your love.
Now I know you're really mine
Gotta have you all the time.

The ecstatic happiness that characterizes the early stages of the new relationship is celebrated by the Shades of Blue in their "Oh, How Happy":

Oh, how happy you have made me,
Oh, how happy you have made me.
I have kissed your lips a thousand times
And more times than I can count I have
called you mine.

The *happy stage* is soon followed by disturbing elements which enter the relationship. Alan Price, in "I Put a Spell on You," describes the lover's reaction to the beloved's behavior:

I put a spell on you
Because you're mine
You better stop the things that you do
I ain't lyin', no I ain't lyin'
I just can't stand it, babe
The way you're always running' around
I just can't stand it
The way you always put me down
I put a spell on you
Because you're mine.

When unhappiness enters a relationship, then one must try to recover the original happiness which it offered. In this case, the lover tries to recreate the earlier ecstasy by "putting a spell" on the girl. But, the relationship continues to deteriorate, almost

to the point of desperation. The Animals describe this stage in their song "Don't Bring Me Down":

When you complain and criticize
I feel I'm nothing in your eyes.
It makes me feel like giving up
Because my best just ain't good enough.

Women are portrayed as unreasonable. This relationship, like all others, is seen as undesirable. The Animals go on to say:

Girl, I want to provide for you
And do all the things you want me to
Oh, but please,
Oh, darling, don't bring me down,
Oh, no, don't bring me down
I'm beggin' you, baby
Please, don't bring me down.

The fundamental danger in this and other relationships is being "brought down." In love affairs, as in mental excursions with drugs, the idea of bringing someone down who is high is disparaged. In this case, the idea that the girl could bring the man down indicates that he is somehow "high" in terms of the relationship. The positive things he sees about the relationship are illusory. The necessity for the plea—"don't bring me down"—indicates that there is grave danger that the feared event will occur. The lyrics continue:

Girl, I know I can keep you satisfied
Just as long as you give me back my pride.
Baby, sacrifices I will make
I'm ready to give as well as take.
One thing I need is your respect.
One thing I can't take is your neglect.
More than anything I need your love
Then troubles are easy to rise above.

The lover needs the girl's response and is not getting it. In this vicious circle he cannot come to terms with the present state of things, and she holds the power. He is unable to make the choice for detachment and independence. This is why there is danger of being brought down. The deteriorating relationship leads to the conclusion that certain kinds of involvement can be very bad. The Outsiders, in "Time Won't Let Me," philosophize on this point:

I can't wait forever
Even though you want me to
I can't wait forever
To know if you'll be true
Time won't let me, oh no
Time won't let me, oh no
Wait that long.

The key line is: "Even though you want me to." The implication is that a person has only one life and that involvement with someone else complicates it. At this point in the relationship the protagonist is urged to disengage. The girl in this song is insisting on continuing the relationship, even though the conditions are unfavorable to the man. The girl is informed that time won't let him wait that long.

The deterioration of the relationship is followed by the *breakup* in Act III. The breakup occurs because the protagonist comes to the realization that the relationship was bad in the first place. Since one cannot know this in advance, the injunction is to terminate the affair once this becomes known. The Syndicate of Sounds, in "Little Girl," describe this breakup:

You can leave little girl
I don't want you around no more.
If you come knockin' you won't get past
  my door.
You got nothin' to hide
Everybody knows it's true
Too bad little girl, it's all over for you.

The decision to break up is difficult but must be made. The initiative seems to rest with the male to begin the relationship and terminate it. The Grass Roots, in "Where Were You When I Needed You," describe some of the difficulties involved:

Don't bother cryin', don't bother crawlin',
It's all over now, no use in stallin'
The love I once felt I don't feel any more
  for you.

What is revealed here is that it is undesirable to lose one's dignity. The boy is putting the girl down, but he doesn't want to

see her cry or crawl, not for her sake but because he doesn't like watching such things.

When the affair is over, one realizes that the involvement was unhealthy. The Cyrkle, in "Red Rubber Ball," describe this realization:

I should have known
You did mean farewell.
There's a lesson to be learned from this
And I learned it very well.
Now I know you're not the only starfish in the sea.
If I never hear your name again it's all the same to me.
[*Chorus*] And I think it's gonna be all right.
Yeah, the worst is over now,
The morning sun is shining like a red rubber ball.

The lesson is: "Don't get involved." The choice for detachment ends the bad experience. The love affair was a big thing, but now it's a little thing. Happiness is equated with contentment and evenness, that is, keeping whole, rather than with risky involvements. If one is going to take chances they should not be taken in interpersonal relationships.

The final stage in the sequence is Act IV, *isolation*. This is the inevitable result of the vicissitudes of human relationships. Isolation is an acceptable way out. In "Solitary Man," by Neil Diamond, a young man's bad experience with promiscuous women leads him to accept isolation as a way of life:

Don't know that I will but until I can find me
A girl who'll stay and won't play games behind me
I'll be what I am,
A solitary man.

He says: "Don't know that I will." Presumably he is prepared to accept isolation for the rest of his life rather than be hurt in love again.

In Simon and Garfunkel's "I Am a Rock," the young man seems to be certain that total withdrawal is the answer to his problem. It is ironic. Throughout the song runs an undertone indicating that he knows his position to be impossible.

I have my books and my poetry to protect me.
I am shielded in my armor.
Don't talk of love, I've heard the word before.
It's sleeping in my memory.
I won't disturb the slumber of feelings that have died.
If I'd never loved, I never would have cried.
I am a rock, I am an island.

He is describing the result of a bad experience. There is no cry that he wants his love back. There is no wish to see her. There is no specification of how he has been hurt. There is no relationship with anyone else at all. The isolation can be interpreted in a more general sense than that due to having been hurt. There is the process of deliberately cutting oneself off, which is experienced as a way of establishing one's own identity.

So ends the stylized love affair—from active searching through various stages of involvement to isolation.

The new perspective of the love affair which is depicted in the 1966 rock lyrics seems to support a new view of the courtship sequence. The change has not involved a shift in interest away from the boy-girl relationship but rather a shift in orientation toward the relationship. In the ensuing discussion we will examine more thoroughly the nature of this change.

A NEW PATTERN

A comparison of the "drama of courtship" in 1955 and 1966 suggests a number of significant changes in the boy-girl relationship in the lyrics of songs over the last eleven years. One of the most conspicuous changes is in the conception of love.

The popular song lyrics of 1955 portrayed love as a deep, romantic involvement. The rock and roll lyrics of 1966 usually refer to a different phenomenon when they discuss love. Love often seems to have been reduced to physical attraction. Romantic love has been rejected as the exclusive requirement

for engaging in a sexual relationship. While permissiveness with affection was acceptable eleven years ago, though entailing certain risks, today's songs legitimize permissiveness without affection in relations. The Mamas and Papas endorse this position in "Go Where You Wanna Go":

> You gotta go where you wanna go,
> Do what you wanna do
> With whoever you wanna do it with.

The new pattern, then, is that you can sleep with someone whether you have affection for him or not—*either* is acceptable.

One of the tragedies voiced in the 1955 lyrics was that everyone wants love but that the prerequisites make it difficult to find. The rock lyrics of today indicate that these prerequisites and preconditions to love have been reduced. Songs no longer dwell on approaches to courtship but focus more on an active search and involvement with a partner.

Similarly, on the downward course of love, rock and roll lyrics do not dwell for any length of time on the deteriorated relationship but rather value a quick break when the involvement is viewed as unhealthy and pervaded with guilt. This kind of involvement impedes one's freedom of action, and a choice for detachment and independence ends the bad experience. The questions posed in the lyrics no longer focus on "Are you still mine" or "I'm afraid that you'll get careless, and someone will steal a kiss," but center closer to the theme the Grass Roots express in "Where Were You When I Needed You":

> Don't bother cryin', don't bother crawlin',
> It's all over now, no use in stallin'
> The love I once felt I don't feel any more
> for you.

The new outlook on the love affair does not include the expectation of permanence. Freedom is celebrated for both partners. This highlights one of the dilemmas in the relationships portrayed in rock and roll lyrics. One is free not to become involved, but others have that freedom also. The relationship where each partner tries to maximize the other's freedom is a fragile arrangement which can be quickly terminated. Emphasis is on a rapid culmination of the affair as well as a quick ending when the involvement is an unhealthy one. The temporary quality of a relationship is expressed in Dusty Springfield's "You Don't Have To Say You Love Me."

These sentiments seem far removed from those of 1955 where a relationship was expected to be permanent. It would be unusual to encounter lyrics today similar to those voiced in the 1955 song "As Long as I Live," in which the singer states that he will remember and love his partner forever.

The popular songs of the middle fifties seem to have placed love in the hands of fate. Love is not something actively controlled by the lovers but something which happens to them. One "falls in love," and later the downward course of love is initiated by hostile forces external to one's control. Because of the lack of control in the relationship, falling in love involves taking considerable risks; the outcome is not predictable. Since love in the 1955 song lyrics is experienced as externally controlled, it is often perceived as an object or a commodity rather than something the lovers mutually create. Love frequently appears in popular song lyrics as something to be "won" or something which is subject to theft.

The rock lyrics of 1966 reject this passive orientation toward the boy-girl relationship. Love is mutually created by the partners and is not perceived as external to their relationship. The affair is actively sought by the lovers rather than passively longed for.

Two other changes have occurred in the courtship patterns. The girls portrayed in the 1966 rock lyrics have been removed from their pedestal in boy-girl relations. "Wild Thing" by the Troggs reduces the girl to an object for sexual gratification. Furthermore, the boy is no longer at the mercy of the girl in the love affair. The 1955 lyrics often show the boy powerless and helpless in the face of the girl, who appears to hold the key to the

relationship. The rock lyrics of today seem to have reversed this power situation. The initiative rests with the male to initiate the relationship and terminate it. The Syndicate of Sounds, in "Little Girl," capture this new pattern:

> You can leave little girl
> I don't want you around no more.
> If you come knockin'
> You won't get past my door.

The major thrust of the 1966 rock and roll lyrics is toward an expansion of the boy-girl relationship to include a wider range of behavior and not just the limited "romantic love affair" which characterizes the 1955 lyrics. The possibility that the relationship between young people might be so quickly terminated today seems to suggest that different levels of involvement between two persons, in addition to the traditional romantic love affair, are now called for.

This whole development seems to reject the older double standard and opt for a more honest and open boy-girl relationship and has the effect of widening the range of choices for a satisfactory relationship. The net impact is to emphasize personal autonomy and eschew the kind of dependency revealed in earlier lyrics.

WIDER RANGE OF THEMES IN 1966

Horton found that 83 per cent of the song magazine lyrics he analyzed dealt with boy-girl relationships. This proportion dropped to 65 per cent in the 1966 song magazines and even less in the popular listings. The content of the rock and roll lyrics in 1966 deals with a wider range of themes than the popular songs of the preceding decade. Of the rock lyrics, 30 per cent are concerned with themes other than love and courtship, in contrast to almost 13 per cent of the 1955 lyrics.

The concerns expressed in the 1955 lyrics, apart from those of love and courtship, range widely and show no clear-cut focus. They include song dances, general narrative ballads on love themes, religious songs, comic songs, and others which could not be classified.

The 1966 lyrics reveal more specific concerns. The major preoccupation of rock and roll lyrics seems to be with *choice*. Choice permeates the themes, relating to many areas of interest beside those of boy-girl relationships. Choice is exercised in terms of freeing one's self from external constraints. The exercise of choice is enjoined in two crucial areas: (1) personal relations, and (2) the kind of society in which one will become involved. Choice in both relates to the individual's autonomy.

We have already seen how the impact of the choice theme operates in the relationship between boy and girl. It functions to maximize the personal freedom of the persons in the relationship by widening the range of alternatives available to those involved.

The second crucial area in which choice operates is the individual's autonomy in relation to the conventional world. Will he become part of the conventional world, or will he choose to drop out and create his own scene? The decision to do something about one's life, to think for one's self, no matter what the consequences, is generally enjoined.

SUMMARY

The major difference in orientation toward courtship in 1955 and 1966 is the active character of the boy-girl relationship. The affair is created by the partners, and they can determine its outcome. If a position can be inferred from an analysis of the 1966 lyrics, it is that one makes his own choices. The value of existential choice is celebrated. It gives one the freedom to change, to become what he wants to be, to make of an affair what two people want it to be.

In the idealized sequence revealed by rock and roll lyrics today, one actively searches out and becomes involved with someone else rather than passively waiting for an affair. Relationships are initiated on the basis of mutual attraction, which in-

cludes both physical and "spiritual" elements. When these elements disappear, the expectation is that the relationship will be terminated. Relationships can also be ended if one or both of the parties diagnose it as "unhealthy." Usually this means that it is tainted with dishonesty. Love is not placed in the hands of fate but is actively controlled by the lovers. Consequently, rock and roll lyrics are not likely to talk about "falling in love," since that phrase refers to a romantic conception of boy-girl relationships which is rejected.

Another striking difference between the 1955 and 1966 lyrics is in the attitude toward being alone—the isolation at the end of the affair. It is viewed negatively in 1955, more positively in 1966. There is pain and suffering associated with the dissolution of an affair in 1966, but it can also be the first step in exploring those facets of the self which can only be explored when one is alone. The description of the isolation phase of the courtship cycle suggests the Zen influence operative among the more recent songwriters.

The fact that there is a distinctive set of beliefs associated with a large proportion of 1966 lyrics may reflect the growing disaffection among younger people who constitute the audience for the new lyrics, or it may simply reflect a change in those who write them.

The significance of the changing orientation depicted here can be established more clearly by a further inquiry into two related areas: the emergence of more democratic controls over songs written since 1955, and the extent to which the new belief system is incorporated ideologically into the social movements of young people today.

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COMMENTARY AND DEBATE

A Critique of Karl O'Lessker's "Who Voted for Hitler?"

There are some very "casual inferences" drawn in Karl O'Lessker's paper ("Who Voted for Hitler? A New Look at the Class Basis of Naziism," *American Journal of Sociology*, LXIV [July, 1968], 63–69), (see his note 8, p. 66). Although his attempt to analyze more rigorously the Nazi voting records is meritorious, the major errors in his use of some statistical techniques and the subsequent inferences render his findings of questionable validity.

The main problem with this work centers on the use and interpretation of multiple regression/correlation analyses.[1] In particular, there are considerable restrictions on interpretations of stepwise regression analyses such as O'Lessker used (see Table 2 in his paper). This is especially true when the predictor variables are highly intercorrelated. In such case, part of the explained variance (often the largest part) cannot be partitioned to a single predictor but can only be termed the variance explained *jointly* by the several predictors. Hence each of the intercorrelated predictors contributes in two ways to the variance explained by the regression: an incremental effect and a joint effect. The first measures its contribution over and above that offered by all the other predictors; the latter, the contribution it makes *in conjunction with* (or, using causal language, "acting with" or "acting through") the other predictors. This latter portion of the explained variance cannot then be assigned to any one of the variables acting jointly. These factors and the associated caveats regarding their interpretation are clearly spelled out in recent works by Robert A. Gordon[2] (1967; 1968) and Otis Dudley Duncan and associates.[3]

In O'Lessker's Table 2, the "nationalists'" vote change alone can explain 38.5 per cent of the variance in the change in Nazi votes. The increased turnout adds 31.6 per cent to this level of prediction; the change in the "non-Catholic middle" parties' vote adds another 23.1 per cent to this explanatory power; and the changes in the votes for all the other parties combined add still another 5.0 per cent to the explanation of the Nazi vote changes. Since O'Lessker himself reports some rather high intercorrelations among these predictors (pp. 66–67), this most definitely means that we cannot report the predictive powers of the above four variables as 38.5 per cent, 31.6 per cent, 23.1 per cent and 5.0 per cent, respectively, as he attempts to do (p. 66 and in footnote 8). The order in which these predictors are added or dropped affects the explained variance increments substantially, when the predictors are intercorrelated. In particular, the first predictor variable has an "unfair" advantage: to it accrues not only its increment over all the other predictors, but also all the variance explained jointly by *all* the predictor variables in the regression.

Thus, unless there is a temporal and/or causal ordering among these variables, there

[1] Robert A. Gordon, "Issues in the Ecological Study of Delinquency," *American Sociological Review*, XXXII (December, 1967), 875–90; Gordon, "Issues in Multiple Regression," *American Journal of Sociology*, LXXIII (March, 1968), 592–616; Peter M. Blau and Otis Dudley Duncan, *The American Occupational Structure* (New York: John Wiley & Sons, 1967); James Fennessey, "The General Linear Model: A New Perspective on Some Familiar Topics," *American Journal of Sociology*, LXXIV (July, 1968), 1–27.

[2] Gordon, "Issues in the Ecological Study of Delinquency"; and "Issues in Multiple Regression."

[3] Otis Dudley Duncan, David L. Featherman, and Beverly Duncan, "Socioeconomic Background and Occupational Achievement: Extensions of a Basic Model" (report to U.S. Office of Education, Project No. 5-0074 [EO-191], 1968).

is no substantive reason for selecting any of them as the first predictor; the choice is then arbitrary. So long as one does not *draw* causal inferences from the results based on any one of these orderings of the predictor variables, there is no problem. But, unfortunately, all too many sociologists confuse simple prediction with causation—whence cometh the need for path analysis.[4]

How then can O'Lessker's problem be solved? If he wanted to find the best predictor of the shift in the Nazi vote, he could just as well have used the zero-order correlation coefficients between each of the predictor variables and the change in Nazi votes. The highest coefficient would, by definition, select the "best predictor" (under this criterion, the DNVP vote change).

Alternatively, he could make use of the technique of path analysis[5] to make explicit his "causal" model. When this is done, several flaws in his reasoning become apparent. At the outset, it becomes clear that none of the predictor variables has any temporal priority over others, so that the conclusions he draws from Table 2 are fallacious, based as they are on one particular order of the predictors (and not even the most reasonable, given the β weights in Table 3). As a path diagram based on the data in Table 3 and some statements on page 67 would indicate, the increased turnout had the greatest *direct* impact on the change in Nazi votes (since it had the largest path coefficient). But all the remaining five predictors had rather substantial direct impacts on Nazi voting, so that to select only the turnout and the change in nationalist votes as *the* "combination . . . that transformed the Nazi party into a true mass movement" (p. 67) is rather misleading.

TABLE 1

PARTITIONING OF TOTAL CORRELATIONS

Factor	Portion of Zero-Order Correlation Deriving From: Net Effect ("Direct")	Joint Effects ("Indirect"): Joint with Turnout	Joint Effects ("Indirect"): Joint with All Other Parties	Total Correlation with Nazi Increase
Turnout	+1.08	....	−.76	+.32
Nationalists	−0.82	+.11	+.08	−.63
Non-Catholic middle ("other")	−0.60	+.26	+.18	−.16
Socialists	−0.39	+.30	−.30	−.39
Communists	−0.36	+.68	+.06	+.38
Catholic center	−0.31	+.69	−.43	−.05

Using the intercorrelation matrix kindly supplied me by O'Lessker, I have used some simple path computations to clarify some of the issues. All of the six predictors are treated as causally prior to the Nazi vote, but none is treated as prior to any other. Given this "model," one can partition the observed correlations with the Nazi change as in Table 1. The "direct" or net effect is simply the β coefficient given in O'Lessker's Table 3. All the remaining "indirect" or joint effects in Table 1 were multiple paths computed from the matrix he provided me.

[4] Otis Dudley Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, LXXII (July, 1966), 1-16; Blau and Duncan, *op. cit.*

[5] Duncan, "Path Analysis"; Duncan *et al.*, *op. cit.*

What does this summary indicate? First, as regards the change in turnout, we find that, although the increased turnout had a large positive net (or direct) effect on the Nazi gain, it also had a substantial negative joint effect (−.76). The latter derives from the large positive relationships between increased turnout and increased votes for the other parties. This strongly suggests that a substantial proportion of the increased turnout went to non-Nazi parties, particularly the Communist and Catholic center (*Zentrum*) groups. The over-all correlation between turnout and the Nazi vote is thus reduced to +0.32, in contrast to the β coefficient of +1.08. Although O'Lessker notes the effects of increased turnout on some parties other than the Nazis (p. 67), he fails to integrate this observation into his analysis of the 1930 Nazi successes. Furthermore, he fails to take note of the modest increase in non-Catholic middle-class and Socialist votes that also appears to have resulted from the increased turnout.

Turning to the changes in the nationalist (DNVP) vote, the 1928 supporters appear to have shifted largely to the Nazi party, along with previous supporters of the non-Catholic middle-class parties. That is, the high negative zero-order correlation (−.63) between DNVP and Nazi changes is a result mainly of their strong *direct* negative relation (−.82), *independent* of the change in turnout (see row 2 of my Table 1). Thus Lipset appears to have been on sound ground in changing Bendix's view that the Nazi increase was due principally to new voters. But he does not appear to have been correct in assuming that only the non-Catholic middle-class parties' supporters were defecting to nazism: the DNVP also appears to have been a rather large contributor to the Nazi success. Indeed, all the other parties appear to have shared in the defection to the Nazis, as indicated by the direct effects listed in the first column of my Table 1.

As is usually the case, Bendix, Lipset, and O'Lessker all have made some valid observations. A full understanding of all the causal processes at work here is not possible from these data and these statistical techniques, owing in part to the multicollinearity problem, which is not yet resolved[6] for the general linear model.[7] Put most simply, if two factors tend to occur simultaneously and are highly intercorrelated, is is rather difficult to choose between the two as "causes" of subsequent events. All six predictors had some direct and indirect impact on the Nazi sweep of the 1930 election, and it is patently incorrect to focus on any one predictor or combination of predictors alone, ignoring the rest.

In addition to all the above methodological problems, there remains one further nebulous area which O'Lessker has not touched on. This is the area of correlation among change variables. The problem arises from the fact that a given correlation coefficient for two change variables actually involves combinations of four variables: each of the basic factors being studied, measured at two points of time. In this situation, problems arise because of "serial correlation," or the high correlations arising between the two measures of the same factor, that is, at two points in time.[8] Whether this condition is exacerbated or meliorated by the fact that the dependent variable is a perfect linear function of the predictor variables is not clear, but this should be explored further. However, my primary concern is with the basic misapplication of the multiple regression procedures, as stated above.

There are several printing or computa-

[6] Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964); Blalock, "The Identification Problem and Theory Building: The Case of Status Inconsistency," *American Sociological Review*, XXXI (February, 1966), 52–61; Blalock, "Status Inconsistency, Social Mobility, Status Integration, and Structural Effects," *American Sociological Review*, XXXII (October, 1967), 790–801.

[7] Fennessey, *op. cit.*

[8] Otis Dudley Duncan, Ray P. Cuzzort, and Beverly Duncan, *Statistical Geography* (Glencoe, Ill.: Free Press, 1961); Blau and Duncan, *op. cit.*

tional errors appearing in the article, which is also irritating—for example, the square of 0.802 is .643, not .701 (Table 2); and the first three independent variables explain 93 per cent, not 98 per cent, of the variance (n. 8).

If there is a moral to be found here, it is this: sophisticated techniques provide more information to the analyst—but only at the cost of increased restrictions on "casual inferences" and a great deal more of painstaking effort on the part of the analyst. "Ex nihilo, nihil fit."

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The Author Replies

Mr. Schnaiberg is very likely correct in many of his criticisms of my statistical methods. The reason is that I am only indifferently trained in that science, never having got beyond a second-level textbook. But there are a couple of comments worth making by way of rejoinder, for it is the substantive issue—*who voted for Hitler?*—that concerns me and not the procedural one of statistical refinement.

Most importantly, nothing in Schnaiberg's criticism really challenges my conclusions on the substantive issue, namely, that in 1930 it was a combination of defecting Nationalists and previous non-voters that transformed the Nazis from a noisy fringe group into the second largest party in the state. And, contrary to Lipset, wholesale defections to nazism on the part of non-Catholic middle-class voters did not occur until 1932—and then almost certainly as a result of the electoral events of 1930.

Schnaiberg scores my drawing of causal inferences on the basis of a stepwise regression analysis based on an "arbitrary" ordering of the predictor variables. Although he had no way of knowing this, the computer program I used (one prepared by the Health Sciences Computing Facility of UCLA) does in fact select that variable which independently explains the most variance and enters it into the equation first. But he is quite correct in pointing to the problem of multicollinearity, which makes it difficult to estimate the relative effect of any single remaining variable. We can, however, look at the zero-order r's and see that increase in turnout and in the Nazi vote correlate at .32 while decrease in the non-Catholic middle-class vote and increase in the Nazi vote correlate at only .17, which surely tends to support my substantive conclusions. (For decrease in the Nationalist vote and increase in the Nazi vote, $r =$ .62.)

Schnaiberg's own analysis, presented in his Table 1, indicates "that the effect of the influx of new voters in a district was to raise both Left *and* Right votes, as well as to increase the Catholic centrist vote." Let me point out that I said just that in my article (p. 67) in reporting the zero-order correlations; and again on page 68 I commented that "this fact serves as support for Bendix's hypothesis about the 'radicalization of the electorate'—a development not confined to radicalization on the Right."

The upshot of all this would seem to be that, while my statistical procedures were evidently inadequate as charged, the inferences I drew from my findings were by no means incorrect. At the very least they seem to constitute a better account of what actually happened in the Germany of the early 1930's than Lipset's contention that the vast increase in turnout had nothing whatever to do with the sharp upsurge in Nazi voting strength in September, 1930.

That, after all, was what I set out to demonstrate. And if other scholars are now motivated to give still closer attention to the problem, I for one will be delighted.

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BOOK REVIEWS

Power and Discontent. By WILLIAM A. GAMSON. Homewood, Ill.: Dorsey Press, 1968. Pp. xi+208. $3.25.

This volume synthesizes the literature on power from two conflicting standpoints and for two distinctive audiences: those of "authorities" and "potential partisans." As a moderately priced paperback, it no doubt will provide a useful service for both audiences, but in the process of dichotomizing power relationships into conflicts between students and administrators, workers and managers, restless insurgents and governments, it neglects dimensions of power that are not highlighted by this particular focus. Gamson brings together some of the best ideas of Schattschneider, Easton, Lasswell and Kaplan, Dahl, Dahrendorf, and Parsons and combines them in a lucid and coherent presentation. But in casting so wide a net, the result is that many sections have no theoretical integration with others. The concept of trust is introduced in an effort to bridge the two fundamental perspectives, but the linkage is by no means fully satisfying. Adaptations of exchange theory have been used in dealing with many of these same problems to achieve, to my mind, a more satisfactory theoretical integration.

But there are many useful points in the book. The discussion of subjective probabilities (esp. pp. 66 ff.) is provocative. Inducements are touched upon suggestively (pp. 77 ff.), if not developed as systematically as would be possible with exchange theory. The discussions of a backlash and redistribution of resources are helpful (pp. 99, 106), and the classification of types and decisions and issue areas is used to develop several interesting propositions (pp. 147–62).

The sections aimed at "potential participants" seeking to exercise their influence focus (largely in Parsonian fashion) on the sorts of tactics which derive from constraint resources, inducement resources, and persuasion resources. The "authorities" are in turn advised on such tactics as insulation, persuasion, co-optation, and the use of sanctions against "potential participants."

Chapter vii on "Predicting Attempts at Influence" is probably the most original in its outlining of such aspects of decision making as group solidarity, liquidity of resources, and types of decisions which lead to more or less likely attempts at influence by various actors. The discussion of liquidity of resources builds nicely on Parsons but does not go as far as the sections by myself or Scheuch, Gordon, and Nuttall in Terry N. Clark, editor, *Community Structure and Decision-making* (San Francisco: Chandler Publishing Co., 1968). In particular, Gamson's (earlier) provocative reinterpretation of the results of reputational surveys as information about the allocation of resources in a community seems to be lost in a confusion of levels of generality (pp. 103 ff.). Surely control of communication media and skill in writing and speaking are on a lower level of generality than the reputed power of an actor. The interrelationships among such varied "resources" require more precise and extended treatment.

Although this may not have been primarily intended as a theoretical work suggesting propositions that could be tested, there are a number of interesting ideas for empirical research in the last two chapters. Most discussions of operationalization, however, remain rather vague.

In a period of continual confrontation of such groups as Gamson discusses, this is a social science manual that the several confronting parties will certainly find, to use their argot, "relevant." It should become a best seller.

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The Social Order of the Slum: Ethnicity and Territory in the Inner City. By GERALD D. SUTTLES. Chicago: University of Chicago Press, 1968. Pp. xv+243. $8.95.

This is a book that would have delighted Robert E. Park. It is about a slum in the near west side of Chicago and exemplifies most of what is at once scholarly, thorough, and insightful in the research tradition Park began. It is also a book from which social scientists, urban planners, or policy makers for the poor

can learn a great deal. With all the thinking and research that has been going on in and about the slums, it seems scarcely possible that this should be so. Haven't we heard enough about street corner groups, delinquency, the shortcomings of public housing, and the rest of the unhappy story? Before reading Suttles' book I might have said so; after reading it I am refreshed by a rare conviction for readers of sociology: I now understand much better.

The chief reasons for this include Suttles' use of two important research approaches often neglected in studies of urban conditions. In the first place, Suttles' approach is holistic; he tries to present the slum as a whole rather than to answer restricted (if urgent) questions about some aspect of it. In the second place, his view of the slum results from a meticulous effort to merge his understanding of it as a sociologist with the knowledge its people have of it. He sees through *their* eyes and addresses himself to the problems *they* perceive and try to solve in their daily lives. Both devices enhance credibility. Presentation of a working model of the whole allows the reader to judge for himself whether anything central to an understanding of the slum has been omitted. Presentation of residents' perspectives assures us that this model is not merely a neat construction of the sociological imagination.

For this is in no sense a journalistic account. It is almost overcompact—so full of carefully organized data that it is not always easy to read. It is not a narrative; the researcher seldom appears except as participant in a few of the sparingly used quotations from his field notes. Instead, it is organized in five sections, each of which elaborates on some aspect of what Suttles terms the segmental moral order of the slum. In part 1, he provides the ecology of the area and its history. We learn not only on what blocks each of four ethnic groups—Italians, Mexicans, Puerto Ricans, and Negroes—lives but how people name the areas and which areas they regard as their territory. Within each ethnic territory people divide into same-sex age groups. Particularly among boys, if there is more than one group, they are assumed to be enemies. The slum is an area where people "carry out their anatagonisms face-to-face" (p. 35) without middle-class forms of impersonal interchange.

Part 2 presents the institutions of the area and its communication. These, too, Suttles sees as a pattern of "ordered segmentation" (p. 41) preserved in the ethnic differences of religious congregations and local businesses. The latter must often host "guests" from the less numerous ethnic groups (Puerto Rican, Negro, and Mexican) who do not run most of their commercial establishments as the Italians do. Negroes in public housing are nearly always guests. The schools, among institutions from the outside, belong to the numerically dominant ethnic group and thus also sustain antagonisms.

Gesture, language, clothing, and style of interaction further differentiate the ethnic groups and breed suspicion. The Negroes, with few other means of self-expression, are as flamboyantly dressed up as the Italians are conservatively comfortable. Enforced intimacy in crowded living spaces promotes personal disclosure which people pass on in street life and clubs in order to understand each other. In emphasizing the "historical and individuated social order" of the slum as both the source of trust and an explanation of the antagonism slum residents feel about other ethnic groups (and rival groups among themselves), Suttles once again suggests that the slum lacks the impersonal interchanges of middle-class life made possible by norms and role expectations.

Part 3, which presents each ethnic group in greater detail, and part 4, which talks about boys' street groups, re-enforce this contrast between the moral order of the larger world or middle class and that of the slum. It is a point, if one of the few, where I must part company with him. The literary device of contrasting the group you are studying with one you have not is always risky unless you can cite comparable studies of the contrasted group. Up to this point I am willing to accept the near absence of discussion of relevant research and theory (Herbert Gans and Walter B. Miller, for example, are not footnoted). The social psychology of C. W. Mills, Everett Hughes, Herbert Blumer, Howard S. Becker, and G. H. Mead, much of whose work is conceptually relevant, is not mentioned. Are we to assume that Suttles has not read these authors and made no use of their work in pursuing his study? Nearly every detail of his field methods, his holistic approach, his emphasis upon peoples' shared understandings as the basis of an emergent moral order, and his presentation of data in quotations, tabulations, and extended argument relating particular findings to more general considerations have long been prominent characteristics of the work of the students of Robert E. Park and their

students. I do not mean to take credit from Suttles' excellent book; I do feel that sociologists now studying groups in the Mead-Park tradition have an obligation to make their indebtedness clear.

BLANCHE GEER

Northeastern University

The Decline of the German Mandarins: The German Academic Community, 1890–1933. By FRITZ K. RINGER. Cambridge, Mass.: Harvard University Press, 1969. Pp. 528. $13.50.

This is an outstanding study linking intellectual and social history with the sociology of knowledge and culture. The "academic community" is defined to include most centrally university professors but also such professionals as clergymen, lawyers, doctors, and others steeped in fundamentally the same academic culture. Ringer documents the decline of this social group from a position of eminence enjoyed only briefly and analyzes the changing intellectual outlook and forms of creativity associated with the decline. As a historical investigation of the political and cultural style of the declining free professions, the study is similar in many ways to Richard Hofstadter's important chapter iv of *The Age of Reform;* it is an excellent case study of the correlates of status deprivation.

But it is also more. In many respects, the American graduate university is a creation of these Mandarins, and many problems they faced are still with us. Students at this reviewer's university occupied the administration building and conducted a vigorous revolutionary campaign during the week while he read the book, and seemed only too close to the social and cultural attacks on the German university near the end of its period of brilliance. The need for a separate set of institutions to synthesize and disseminate the findings of basic research is directly relevant to what is currently termed the crisis of general education.

The term "Mandarin" was used to refer to the general social eminence of the great German professors, an eminence founded on a basic set of cultural beliefs which they developed and which became shared by a large part of the society around them. Their position would have been impossible under the earlier feudal society which was more clearly dominated by aristocrats and warriors. Ringer argues that the large-scale industrial organizations of the machine age demanded entreprenurial and organizational skills which in turn displaced the Mandarin ideology. The Mandarins only emerged in the early nineteenth century through a sort of compromise with the aristocratically controlled bureaucracies in which loyalty to the state was granted in return for generous material support and intellectual autonomy. Near the turn of the century, however, German academics began to write of a crisis in their special fields and in the society around them; aspects of it were analyzed by such gifted observers at the time as Max and Alfred Weber, Meinecke, and Troeltsch; but Ringer brings together these materials with a coherence and lucidity that become possible with the perspectives of time and distance. The crisis was never more than vaguely defined but was related to the increasing differentiation of knowledge that could no longer be integrated with the German idealistic tradition. University learning thus provided no longer as convenient a *Weltanschauung* as in the time of Hegel, and for many critics the universities seemed increasingly to resemble the factories around them. Anxious students, school teachers, and others cried for synthesis and a religion in which they could have total faith. But the professors sought to remain "above the parties" and despised the narrow ideological bases of the politics of interest group compromise. The nationalism of some led to enthusiastic support for World War I; searching self-examination by others produced the economic *Methodenstreit*, the debate about historicism, and *Wissensoziologie.* But relativism and exact knowledge were meager fare for those seeking Faith. *Volkisch*, nationalistic, and socialist themes outside the universities grew increasingly strong until, in conjunction with economic chaos, they became united in the National Socialist party.

Ringer's strongest sections are those analyzing the reactions of different groups of Mandarins to these various developments. He focuses several illuminating incidents. The controversy in which Weber produced his famous essay—most appropriately translated as "Scholarship as an Occupation"—was crucial and deservingly merits Ringer's detailed analysis. The proposed, enacted, and rejected legislation dealing with educational reorganization at different stages all nicely illustrate the range of opinions debated. Key terms and distinctions in the Mandarin vocabulary are searched for their clarifying and obfuscating functions: objective and

subjective *Geist, Bildung, Kultur, Zivilization*, idealism, synthesis. Such ideological defenses as the symphonic analogy (pp. 100 ff.), the technique of negative definition (p. 394), and the technique of the higher third (perhaps better labeled "non-creative synthesis") (pp. 395 ff.) are suggestively presented.

If analyses of these general themes are the strongest points of the book, its weakest sections are those dealing with specific disciplines. There are capsule summaries of history, psychology, sociology, economics, and pedagogy during these years, but they are treated too much as separate entities. Ringer's technical knowledge of these many fields is necessarily insufficient to clarify what was most or least fruitful for later developments, although his modesty is refreshing (p. 372). A more satisfactory format would have resulted from organizing the discussions of these disciplines around several central themes, such as the elements of the Mandarin ideology. Cross-national comparisons are almost entirely absent, and a few would have been useful. The Index is incomplete. The author's and reader's psychic distance from the Mandarins would not have been unduly decreased by following their habit of placing the footnotes at the foot of each page. The price is high.

TERRY N. CLARK

University of Chicago

American Piety: The Nature of Religious Commitment. By RODNEY STARK and CHARLES Y. GLOCK. Berkeley and Los Angeles: University of California Press, 1968. Pp. x+230. $6.75.

This is the first of a three-volume study of contemporary religious commitment in America. The study is based on two sets of data: first, an elaborate questionnaire survey of adult church members in northern California, and second, a national sample of adult Americans, who answered some of the most important questions from the church-member survey. The two surveys took place in 1963 and 1964, respectively.

The first volume sets itself the task of reducing the ambiguity which attaches to the notion of religious commitment, and this is done by conceptualizing and measuring the different aspects of individual religious commitment along the dimensions suggested by the authors in their earlier book, *Religion and Society in Tension* (Chicago: Rand McNally & Co., 1965). They distinguish five dimensions: belief, practice, knowledge, experience, and consequences. The last dimension, concerned as it is with effects to some extent, occupies a special position and is not taken up in this first report. Apparently Volume III will be devoted to investigating the social and psychological consequences of commitment.

Although enough of this data appeared in the earlier *Religion and Society in Tension* to whet the appetite, the first installment of the finished product suggests that it will constitute the most impressive and exhaustive body of empirical material on American religious behavior yet to be compiled. One important effect of these findings will be to refine the concept of religious commitment in such a way that it will no longer be possible to use such crude categories as Protestant, Catholic, and Jew, in relation to the "religious factor." The data presented by Stark and Glock demonstrate the sharp distinctions that can be drawn among different groups with regard to the varieties of religious commitment.

Thus, on the belief dimension there is some indication that ethicalism and doctrinal orthodoxy can be mutually exclusive roots of religious identity, especially for Protestants. The proportion scoring high on an ethicalism index falls steadily from the more liberal denominations such as the Congregationalists (where 52 per cent scored high) to the theologically conservative southern Baptists (33 per cent scored high). An interesting exception to this is provided by the sects. Whereas on all other indexes the sects are grouped with the conservative denominations, on the ethicalism index they score more highly than members of even the most liberal denominations. The reason is to be found in the fact that the measure of ethicalism was constructed from a battery of questions asking about criteria for salvation. Apparently the sect member differs from members of even the most conservative denomination in that he regards almost everything as having some bearing on salvation. This could well be a useful lead for further research into the differentiating factors at the root of the church-sect dichotomy.

Over-all, the picture that emerges of American religion is one in which almost everyone belongs to a denomination, but only a minority fully accepts the traditional, orthodox doctrines

Protestant-Catholic contrasts are inconsequential compared with differences among the Protestant groups, and in some of the latter a substantial minority have a demythologized faith that might surprise their unsuspecting pastors. Some light is shed also on the discrepancy between high membership figures and low religious knowledge by the finding that church attenders are more active "joiners" than the general public. This characteristic is obviously related to the above-average affluence of church attenders, as revealed by the data, although this is not the whole story. However, the more liberal (and more affluent) denomination members spend less of their time in church activities, and have fewer close friends there, than members of conservative denominations. Émile Durkheim's conception of the church as a moral community is thus shown to be more applicable to some denominations than to others.

Finally, Stark and Glock provide some evidence to show a conservative-to-liberal trend in church switching which suggests a definite cycle in American church affiliation. Persons moving up into the middle class are recruited into congregational membership by the theologically conservative denominations, while the more established middle and upper classes are moving into moderate and liberal denominations. Many of those who begin life in liberal denominations eventually drop any congregational membership, even though they still claim a liberal denominational affiliation when asked. If this is the case, then it will go some way toward explaining why the steady rise in church attendance, which went along with the upward trend in the economy after World War II, turned into a downward trend in the late 1950's.

Kenneth A. Thompson

Smith College

Comparative Research across Cultures and Nations. Edited by Stein Rokkan. Paris: Mouton, 1968. Pp. 239.

The International Social Science Council organized a "Round Table Conference on Comparative Research" in Paris during the spring of 1965. Five themes were to be discussed by a group of experts from different countries: the cross-cultural method; comparative analysis of historical change; comparative analysis of processes of modernization; aggregate statistical comparisons of nations; and comparative sample surveys. To focus the discussions, sixteen recent monographs were to be analyzed critically as models for further comparative endeavors. Apart from some indecisive polemics (Why do academics rarely respond to their critics? Don't they believe in cumulative knowledge?), most papers in the book express their authors' preoccupations rather than the conference's intentions. It, therefore, does not come as a surprise that these papers vary greatly in quality, in sophistication (fourfold frequency tables versus linear stochastic models as paradigms of social reality), in length, in style (including various modes of footnoting), in topical content and metatheoretical assumptions. And, while such diversity is a congress' delight, it is a source of despair for cataloguers and reviewers.

However diverse, the authors share some common beliefs and assumptions. First, it is believed throughout that there are laws which govern and/or describe regularities in the social order and its development. Second, these laws are causal. Third, because experimentation is not feasible, comparison across cultures and societies is the only method left to substantiate, falsify, modify, or even discover laws of society. Fourth, the methodological problems of intersocietal comparisons frustrate the scientific enterprise; solve these problems and the body of laws will ensue.

These methodological problems all center on matters of identity and equivalence. For example, is "feudalism" in prerevolutionary France identical, and therefore comparable, to "feudalism" in Japan of the last century, or for that matter in present day Central America? Furthermore, if we define the phenomenon in its essential components, then the exposition readily digresses into metaphysics. If we define the phenomenon in terms of its causes or consequences, then causal laws become mere tautologies. Similar problems occur on the operational level. Considering the different levels of inequality, is the per capita gross national product of Kuwait identical, and therefore comparable, to this product in the United States? Can one therefore compare such aggregate national statistics? In regard to interviewing, does the "same" question, translated in various languages, measure the same thing? These and related issues are extensively discussed in various chapters.

Methodological problems of this kind, however, are not unique to intersocietal research,

since very similar problems arise in intra-societal investigations. After all, feudalism was not the same in all parts of France, and it varied over time; the minimum wage buys less in New York City than in Drexel, North Carolina; and interviews concerning crabmeat must elicit different responses in babes, students, and gourmets. Ethnomethodologists feast on these insights, and neither they nor anyone else have resolved them. Nonetheless, correlations proceed and stochastic laws proliferate. Across cultures, however, the *scale* of these methodological problems is much larger, and it is for this reason that their resolution will require organizational rather than methodological innovation.

The lone professor and his assistants are not equipped to traverse the earth while raising theoretical questions, collecting, and analyzing data. Also, such efforts do not pay off, considering required publication schedules. To secure continuity, institutionalization is inevitable. Thus, we find such things as the Human Relations Area Files, assorted data banks, and regional and national census offices. For cross-national research, however, such enterprises will have to become cross-national as well, escalating problems of research politics and philosophy, introducing problems of international conflict, national politics and ideologies, creating imperialisms of all kinds. The Introduction mentions efforts by UNESCO and the International Social Science Council which were designed to rectify institutional dilemmas, but little came of them. In his essay, Erwin K. Scheuch deals perceptively with some administrative and diplomatic problems. His treatment, however, is an afterthought, and few of the other contributors seem so concerned. Why? Methodological talk is more prestigious; social analysis of concrete research failure is bad form; and finally, social science training proves inimical to organizational creativity. To conclude, because administrative machinery, rather than methodological expertise, is the issue in faltering cross-cultural comparative research, the book probably misses the point.

J. Zvi Namenwirth

Yale University

Symbols in Society. By Hugh Dalziel Duncan. New York: Oxford University Press, 1968. Pp. x+262. $6.75.

This book, dedicated to Kenneth Burke and George Herbert Mead, aims at a systematic dramatistic theory of communication and society. It is divided into axioms, theoretical propositions (e.g., "disorder in society originates in disorder in communication," p. 130), and methodological propositions (e.g., "sociological explanations of symbols must be grounded in the analysis of social drama as a drama of hierarchy," p. 155). Using such elements, Duncan aims to construct a "symbolic model of sociation" which is based on society and language, not man, nature, or God (p. 117).

The book is polemical in spirit, taking issue with "mechanists," especially those of Harvard; and castigating sociologists for thinking poorly about communication, castigating Marx and Pareto for reducing symbols to epiphenomena, semanticists for reducing symbols to "referents," anthropologists for reducing function to structure, and system theorists for using "equilibrium" as a mystifying ultimate.

The critique of mechanism is perhaps the most important argument of the book. Regarding interaction: "If we 'gear', 'mesh' or 'cathect' roles, we cannot talk about the symbolic *meaning* of roles.... In mechanical models of interaction, men signal, they do not communicate" (p. 67). Regarding determination of social systems: "Purely 'environmental' explanations of social order are like explanations of a play by its stage setting.... As we symbolize nature, we make it a scene or stage upon which we enact our drama of social order" (p. 70).

Duncan takes the methodological bull of objectivity by the horns and throws it: symbols are not elusive, subjective data requiring other kinds of data for confirmation, but the "most directly observable 'facts' in human relationships." We know directly what people say they do and only by inference what they actually did (p. 152). He recommends a "sociolinguistic" methodology to search out linkages of symbols to each other and to meanings in various action contexts. For example, "In the name of what principles of social order" is a victim sacrificed? (p. 157) or in what ways is rank symbolized in communication of social order? (p. 165). Seven basic forms of social drama, and ten types of linkage, are distinguished for empirical investigation. But, from these methodological suggestions, we are a long way from having a manual which can be used by graduate students. As Duncan says, in interpretation of symbols such as the grotesque and absurd, we "must wait

until some great mind lights a path to understanding" (p. 159).

I cannot avoid some criticism of the style and organization of the book. Some readers may be put off by Emersonian generalizing, sweeping statements of "all" and "every," especially when one can think of exceptions (e.g., "only peers are honored," p. 57). At his best writing about topics like sociability, Duncan is like Simmel; but at worst he is wordy and repetitious. The importance of rules and umpires to democracy, for example, hardly needs lengthy explanation to Americans. He belabors certain favorite topics, such as hierarchy, and guilt and victimage, while ignoring areas of symbolic interaction to which contemporary theorists, such as Goffman, and Lindesmith and Strauss, have contributed—which seems odd for a book aiming at a comprehensive theory of symbols and society. The same essay style continues in "Methodological Propositions," where we might expect to get closer to the nitty-gritty. For example, in Proposition 11, although it runs to two pages, I could not find a word which tells about methods for studying the social manners discussed. Indeed, it would not be easy to tell, if some of the propositions of the book were mixed in a heap, which are "theoretical" and which "methodological."

On the issue with mechanists, I personally do not see the conflict between the idea of equilibrium and symbolism that Duncan envisages. It seems to me that, if carefully specified, we can distinguish a symbiotic system of cause and effect beneath a symbolic one of meanings and their functions. Both sets of causes work together in a status quo which, partly because it is in physical equilibrium and partly because it is a situation symbolically defined and maintained, is in equilibrium. If this is so, the problem is to analyze a combined symbolic-nonsymbolic equilibrium. In other words, to reject general system theory, just because at present it is too mechanical, is to toss out a very promising baby along with the bath water.

But these are mostly minor faults, and the substantive effort of the book can be applauded. We who wish for the development of symbolic interaction theory can be glad that Duncan is applying the riches of his knowledge of literature and symbolism to the data of sociology, to try to bring to fruition the promise of the writers to whom his book is dedicated.

Orrin E. Klapp

San Diego State College

Community Structure and Decision Making: Comparative Analyses. Edited by Terry N. Clark. San Francisco: Chandler Publishing Co., 1968. Pp. xi+498.

This is the first full-length volume to be devoted fully to the analysis of community decision making in large numbers of communities. As such, it synthesizes the most important findings emerging from research based on a comparative perspective. The synthesis leaves something to be desired, since the book is an assembly of the work of twenty-four researchers, most of whom have previously published their research. But their work is excellent, and it is a broad coverage of community power research by political scientists and sociologists. There are such well-known researchers as Peter Rossi, Amos Hawley, William V. D'Antonio, William Gamson, and Raymond Wolfinger among others. Besides, Terry Clark has pulled the disparate sections together with five introductory chapters on community structure, power, and decision making, and a concluding chapter on present and future research on community decision making.

The volume stresses the new search for comparative designs and attempts to specify as precisely as possible the types of community structure, under varying conditions, which give rise to different patterns of decision making, and how these patterns in turn influence the actual outcomes of community decisions in one direction or another.

There are many features of the book that reflect the nature of community power research today. The attempt to define power goes on. Terry Clark says that *power* is the potential ability of actors to select and change, and to attain the goals of a social system. Influence, on the other hand, is social action within the general normative framework of a power structure. Power, seen as a distribution of resources, may be converted into influence. *Influence*, then, is the exercise of power that brings about changes in a social system (p. 54). This distinction is vital in understanding the difference between the study of persons and organizations that wield power and the study of issues as a means of understanding decision-making processes. It suggests that researchers should be careful in what they call "power structures" and "influence patterns." Distinctions between manifest leaders, potential leaders, and reputed leaders also reveal important differences as R. L. Nuttall, E. K. Scheuch, and C. Gordon point

out in their chapter, "On the Structure of Influence."

The appreciation of the varieties of power structures has marked the more mature phase of research in this area. There is now a recognition of the possibility that a community system may be composed of numerous power and influence hierarchies, each with a different shape. Power structures may be composed of multiple hierarchies of which some may be monolithic and others polylithic. Even the same structure may respond autocratically or democratically, depending on issues and resource mobilization. There is every indication that researchers will depict relatively complex patterns to delineate ongoing power structures of local communities in the future. Interrelated, overlapping, intermittent, semi-autonomous hierarchies will illustrate the sociometry of personal and organizational power and influence structures.

Important as these matters are, probably the major characteristic of the book is the array of efforts to find the structured features of the community which have the greatest impact on its political functioning. These include absentee ownership, economic resources, city type, competitive political parties, degree of industrialization, community integration, life-style values, ratio of managers, proprietors, and officials as well as the employed labor force, form of government, ethnicity, city size, region, type of ballot, etc. The full range of these variables can be seen in chapter v in which thirty-eight general propositions are set forth for testing.

I would nominate the article "Reputation and Resources in Community Politics," by William A. Gamson, as the neatest research design with a significant contribution. Gamson studied fifty-four issues in eighteen New England communities. He identified the reputational leaders associated with the issues and analyzed issue outcomes to find out whether the issues had reputational leader support, divided or inactive reputational leaders, or reputational leader opposition. His finding: reputational leaders are, with few exceptions, active on at least one of three issues studied in their respective communities. However, they are frequently active on opposite sides; when they are both active and united, they are on the winning side about three-fourths of the time. Nor is their success gained from supporting the status quo—they achieve their success against this advantage. They are united and successful in support of change two-thirds of the time (p. 347).

I would nominate the article, "Political Ethos and the Structure of City Governments," by Raymond E. Wolfinger and John Osgood Field, for the best demonstration of how ugly fact can destroy the beautiful theory. Students can take heart that thesis sponsors really mean it when they say a lack of correlation is an acceptable outcome for a research thesis if the research is well done.

Wolfinger and Field tested Banfield and Wilson's theory of a "public regardingness and a private regardingness" ethos as orientations related to the style, structure, and outcome of local politics. They utilized a research design including 309 cities in excess of 50,000. Twenty-six pages later we learn that the ethos is "irrelevant to the South"; "inapplicable to the West"; "fares badly in the Northeast"; "the differences in the Midwest cities are small and uneven"; "in short the ethos theory clearly needs a good deal of modification" (p. 195).

This book is essential for the student of community power. It is well suited to graduate study and should be in the library of every researcher in political sociology.

DELBERT C. MILLER

Indiana University

Patterns of Local Community Leadership. By LINTON C. FREEMAN. Indianapolis: Bobbs-Merrill Co., 1968. Pp. v+138. $2.95.

Since the publication of Floyd Hunter's classic study of Atlanta, *Community Power Structure*, there has been a continuing controversy concerning the best method of studying community leadership. While numerous comparisons have been made between various approaches to identifying leadership, Freeman's *Patterns of Local Community Leadership* presents a systematic and comprehensive comparison of these procedures. Of particular concern is the degree to which various techniques agree or disagree in identifying community leaders. Also investigated is the degree to which leadership is concentrated in the city studied (Syracuse, New York), as well as the factors affecting differential rates of leadership by various segments of the population.

Not surprisingly, it was found that the approach used had an influence on which indi-

viduals were identified as leaders. However, Freeman suggests that these results may, at least in part, be dependent upon the city studied and the time of the investigation (1959–61). Perhaps two or more decades ago, when Syracuse was smaller and less diversified, there would have been greater agreement among the different methods in identifying the leaders of the community.

It is strongly suggested that the conflict between the monolithic and the dispersed conceptions of community power is essentially artificial. The apparently conflicting findings of various researchers may largely reflect the fact that they examined different cities at different periods in their growth and development. In their historical study of leadership in Syracuse, for example, Roscoe Martin and his associates concluded that a single ruling elite probably dominated decision making during the 1930's. However, as the city grew in size and complexity, decision making became decentralized. Needless to say, many of Freeman's readers will not so easily be convinced.

Patterns of Local Community Leadership is a brief (82 pages, excluding appendixes) and well-written report of research which was conducted by Freeman and his collaborators, Stephen Koff, Morris Sunshine, Thomas Fararo, and Warner Bloomberg, Jr. Preliminary reports, such as "Locating Leaders in Local Communities: A Comparison of Some Alternative Approaches" (*American Sociological Review*, October, 1963), have been appearing for several years.

In spite of the competence of the investigators, Freeman is not able to deliver all that he promises. The temporal and spatial specificity of the research alone precludes this accomplishment. Nevertheless, the book represents a significant contribution to our understanding of community leadership and the problems involved in its study.

Freeman has the rare ability to explain statistical concepts and procedures in a lucid and interesting manner (see, e.g., his *Elementary Applied Statistics*). While his methodology is sophisticated, he carefully explains what specific techniques achieve and why he employs them in given instances. Consequently, while it is part of the Bobbs-Merrill "Advanced Studies in Sociology" series, the book is also suitable for use even with undergraduates.

DAVID J. HANSON

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Old People in Three Industrial Societies. By ETHEL SHANAS, PETER TOWNSEND, DOROTHY WEDDERBURN, HENNING FRIIS, POUL MILHOJ, and JAN STEHOUWER. New York: Atherton Press, 1968. Pp. x+478. $15.00.

This excellent comparative cross-national survey of those old people living at home in Denmark, Britain, and the United States contains a wealth of new data. Professional interviewers in each country conducted structured interviews with a random sample of 2,500 persons (males and females) aged sixty-five or more. Careful controls led to good uniformity in coding. The major problem in the data lies not in the design but in the differences in non-response rates which in Denmark and Great Britain were 14 per cent and in the United States, 7 per cent.

We follow the life of the old through chapters on health, incapacity, medical services, welfare services, family relations, work and retirement, income receivers, pensioners, and charitable cases. The three societies differ in the structure of their institutions and in their attitudes toward the old, but these differences among Danish, British, and American societies seem to have less effect on the data than the common experience of aging in an advanced industrial society. Old people as a group are beginning to pose as many problems for advanced industrial societies as children do for those in process of development.

The 4 per cent or 5 per cent of the old and frail who are in institutions are excluded from the surveys. However, in Denmark 10 per cent, in Great Britain 14 per cent, and in the United States 8 per cent of the old *at home* are either bedfast or housebound. It becomes evident that in all three societies, in spite of the wide variety of institutions for the care of the aged and the differing concepts of social concern that these represent, the old in institutions are never more than 5 per cent of the age cohort. This confirms reports from more general social surveys of communities where about 3 per cent of the population of all ages depend wholly on the

state welfare services for their survival. So, 95 per cent of the old who survive do so because they have families to aid them, not because the state is welfare-oriented.

Notice that more than twice as many of the frail and bedfast old people are cared for at home than in all hospitals or nursing homes of any single country. About 80 per cent of the old have brothers or sisters who can sometimes help them, but the most frequent providers are children or grandchildren. Those old people who had two or more children had one of them living within thirty minutes or closer when they were aged seventy or over. So, *family planners, please notice* that in all these three developed societies the best social provision for old age is to make sure you have grandchildren around. In fact, of all the welfare services, only housing seemed to alter noticeably the behavior of the people. Denmark standardized its house size at too small a room count, so instead of living in the same house as their children, Danish old people live nearby more often than in Great Britain or the United States.

The principal contribution to theory is that the old as a group are surprisingly well integrated into their society. Approximately 90 per cent are able to walk around, and almost as many say they can go outside without difficulty. Of the elderly men, about 39 per cent in all three countries are still employed. Although social class comparisons are troublesome because of differences in national usages and statistics, more white-collar workers than blue-collar workers are still employed at age seventy or over in all three countries.

The conventional topics of retirement, income maintenance, public provision for health care in community clinics and in hospitals, which are relevant to social policy rather than to sociological theory, are fully discussed. The theoretical meaning of the emergence in all industrial societies of as much as 10 per cent of the population above the conventional working age is mentioned, but further exploration to discover generalizations about social structures that are theoretically interesting will be needed. In spite of the interest of this research team in comparative demography, in income, and in social policy, I was naturally impressed by the repeated iteration of family data. These family data are the basis for the discussion of theories of integration and disengagement; these same data provide the infrastructure that allows bureaucracies in all countries to pretend they have an advanced social welfare system when they look after only 3-5 per cent of the population entitled to support. Children, and more specifically grandchildren, are essential to a comfortable old age; great grandchildren are even better. If this is so in Denmark, the arguments for birth control in India can hardly be convincing.

For sociologists interested in good data about the similarities among urban-industrial societies, in cross-national comparative research and particularly for family sociologists, this book marks the beginning of a new stage in sociological research and publication. This appears to be the report of a harmonious international professional team. It should be widely read as a clear exposition of survey data.

John Mogey

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A Decade of Health Services: Social Survey Trends in Use and Expenditures. By Ronald Andersen and Odin W. Anderson. Chicago: University of Chicago Press, 1967. Pp. xix+244. $13.00.

This book reflects the extent to which social research has contributed to the store of information in medical care and public health and, more specifically, the degree to which current knowledge of use and costs of health services is anchored in the now classic Health Information Foundation–National Opinion Research Center studies. The volume is based on three comparable national studies conducted in 1953, 1958, and 1964. In the last survey, home interviews were carried out with 2,367 families, providing information on 7,803 individuals. This constitutes an area probability sample of the non-institutionalized civilian population of the United States. The co-author of the volume, Dr. Odin W. Anderson, was the senior investigator of the other two studies, and the major questions which he formulated in his earlier works provide the main basis for organizing the data of this volume. The book is divided into five main chapters: "Use of Health Services"; "Personal Expenditures for Health Services"; "Methods of Paying for Personal Health Services"; and "Relationship of Use, Expenditure, and Method of Payment." The final chapter summarizes the main findings of the study and considers their implications. A de-

tailed description of the methodology and a copy of the interview schedule are included in the Appendix.

The volume is lucid and concise and contains a mass of rich data. While many of the findings will not surprise students of the field and, for the most part, point in the same direction as those reported in earlier studies, the book offers hard and substantial documentation of vital areas of concern and provides benchmarks of use and expenditures over a ten-year period. Income continues to be an overriding determinant of the use of health services. Higher-income groups are more likely to see physicians and specialists and to obtain dental care, although there appears to be a "bimodal distribution of admission rates by income level . . . with the higher rates existing for the lowest and upper middle income groups and lower rates for the lower middle and upper income groups" (p. 38–39). Happily, we learn that people in the lower-income brackets are increasing their use of health services and that the gap between different socioeconomic groups in the use of some services appears to be narrowing. However, there is considerable variation among different regions of the country.

Expenditures for personal health services in the United States have increased markedly, from 10 billion in 1953 to 21 billion in 1963, reflecting a faster growth in the health sector than in the rest of the economy. "The most significant shift in the proportion of expenditures has been the increasing proportion flowing to hospitals and the decreasing proportion to physicians" (p. 152). The proportion spent for drugs and medications has also increased, although this change appears attributable more to the growing amount of the use of drugs than to price changes. Except for dental care, those sixty-five and over spent more than other age groups for each type of health service; and females spent more than males for every type of health service.

The proportion of total expenditures for personal health services covered by insurance shows a steady increase over the years—from 15 per cent in 1953 to 27 per cent in 1963. A constant increase in benefits is also evident for each type of service, although benefits provided were primarily for hospital care and physician care. "Thus, by 1963, 69 per cent of all hospital expenditures were covered by voluntary health insurance" (p. 92). One interesting development is the increase in employer participation in the payment of insurance premiums. Approximately four-fifths of the families enrolled through their work groups had part of their premiums paid by the employer in 1963.

The voluntary health insurance movement seems to be approaching saturation, with three-fourths of all American families now possessing some form of health insurance coverage. Those still outside the scope of the system are largely the aged, rural, low income, self-employed, or other elements of the so-called non-group which has long posed actuarial problems for the insurance industry and particularly for Blue Cross. The benefit structure for those enrolled on a non-group basis continues to be appreciably lower than that for persons enrolled through their place of work. In this respect, it is worth noting that the dreams of some early Blue Cross leaders to achieve a standard community rate for all those insured has virtually been abandoned. This points up the limitations of the private insurance system to provide adequate benefits for special segments of the population.

It is difficult to summarize the wealth of highly condensed data contained in this volume. Those who are interested in studying the operation of our complex and pluralistic health system, especially with regard to use and expenditures, will find the book a valuable reference. It will be most interesting to observe the impact of recent federal legislation on the use-and-cost picture when subsequent studies are undertaken.

Sol Levine

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Social Stratification: The Forms and Functions of Inequality. By Melvin M. Tumin. Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1967. Pp. ix+112. $4.50.

This short text, one of its publisher's "Foundations of Modern Sociology" series, presents a brief and relatively advanced review of stratification. Not every aspect is covered, but most aspects covered are covered well. And Melvin Tumin offers us aspects of an original essay as well as a text. The essay advances a view of stratification which (whatever may be Tumin's private motives) is (*a*) consistent with the liberal-left vision of a good society and (*b*) refutes the Davis-Moore theory of stratification.

Tumin calls no special attention to his involvement in debates on that theory and remarks only that "the political and moral implications of social inequality are issues on which my personal predilections are reasonably well-known" (p. vi). This is hardly candid enough for innocent undergraduates. But balanced by the book's other merits—including a careful and clear prose style—Tumin's bias is not too objectionable to me, but I am more inclined to assent to the conception of a possible though stratified society adumbrated by Davis and Moore than to share the hope for a good and unstratified society held by most of their critics.

Tumin opens with skilful summaries of such past theorists as Aristotle, Marx, and Weber. Two chapters (nearly a quarter of the book) deal with the concepts of "evaluation" and "reward." I think Tumin would have accomplished more here had he tied his analysis to some more general and systematic notion of "reward" (e.g., that of the psychological "learning theorists"). Thus, Tumin labors to set off the value of a status from the reward it receives but notes that this is not always possible, since social approbation (evaluation) is itself a reward. My own suspicion is that, while evaluation and reward diverge in the short run (because the honorific symbols of status change more slowly than the rewards of status), a more thorough analysis of concepts will show that in the long run, and with all due respect to the Weberian catechism, this is a distinction without a difference.

The chapter on method deals well with issues of multiple criteria for stratum definition and their corollary that the enumeration of strata in a given society depends mostly on how they are defined. Tumin iterates the point that particular research conclusions (such as those of users of the "Warner approach") rest on particular concepts and methods. The highly condensed chapters on mobility are outstanding and reach a literary level above the rest of the book. Tumin stresses that the principal cause of stratum mobility is neither aspiration (e.g., "N achievement") nor its societal valuation but, rather, disequilibria of supply and demand for occupants of stratified positions, that is, class-specific fertility rates (supply) and changes in the economy and occupational structure (demand). Insofar as classes do not nicely fit their fertility to the need for personnel in their level of positions (although there is evidence that in time and in part they do so adapt), then even a caste society with a static economy will experience some degree of stratum mobility. "Value orientations" may not matter at all.

Finally, we come to Tumin's exorcism of the pernicious Davis-Moore theory (chap. xii, "Some Theoretical Problems"; nowhere does Tumin name the enemy). I was disappointed; the argument read better when Tumin first gave it in 1953. The criticism is inconclusive and somewhat sentimental, and suggests that Tumin may be less sure now that stratification can be ended. Today we know how hard and costly it is to sustain egalitarian communities, largely through the study of the radical *kibbutzim*, but Tumin does not mention them. In fact, some argument for inherited inequality is nearly unavoidable. The Davis-Moore theory is only one such argument. In his own treatise on stratification, Gerhard Lenski takes several swipes at Davis and Moore but reaches the same conclusion on the persistence of stratification (*Power and Privilege* [New York: McGraw-Hill Book Co., 1966], pp. 63, 441–42). Note too that the Davis-Moore argument can be sustained on their assumption of differential scarcity of activities alone. A premise of their *differential* functional importance of activities is not essential, although this assumption, however difficult to prove, is still less outlandish than its opposite—that all activities are equal in functional importance. If human activities are operants, and unless those which a system of equal reward would render scarce are expendable (which violates any assumption that activities are not differentially important), then activities must be unequally rewarded, which inequality attaches to positions through the division of labor. There are many roads to the conclusion that he who says social organization says inequality.

For Tumin (and for many others) the ideological and theoretical objection is not so much to inequality as to its inheritance. He writes graphically of the injustice and "dysfunctions" of inherited inequality but fails to account for its ubiquity and persistence. The problem is the relationship of stratification and the family. Most arrangements to fix responsibility for reproduction and socialization (which have always required strong institutionalized reinforcement) do so by identifying parents and children in all major statuses (i.e., those of class, religion, "ethnicity") not necessarily set off within the family (i.e., those of age and sex). This unity of status gives the occasion, and the responsibilities assigned to parents the

motive, for ascription of stratified and other statuses. Who, therefore, would relieve inherited inequality must very profoundly modify the family. Small wonder the ideologues (most of whom are busy giving their own children an elite education) have avoided discussing this aspect of stratification.

Tumin does save some of his original concern by stressing the costs of any stratification system in the alienation and low productivity of persons at the bottom. This shows that Tumin is no Marxist (or at least no consistent Marxist), for he implies that the proletariat are not high producers whom the parasitical bourgeoisie can exploit. Viewed as a text, his book is generally successful. But as an essay it seems to me to fail to show that inherited inequality will not persist in human society.

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Law and the Balance of Power: The Automobile Manufacturers and Their Dealers. By Stuart Macaulay. New York: Russell Sage Foundation, 1966. Pp. xix+224.

In this penetrating case study Professor Macaulay enriches our understanding of the legal order by showing how legal rules are related to the doings of those purportedly regulated by these rules. The gap between the law as promulgated by higher authorities and the law on the ground has been discovered many times. From the high vantage point of traditional legal scholarship, this gap has been viewed as an exceptional, marginal, and transient phenomenon, not as one of the central and pervasive features of our legal system. The legal scholar, as Jerome Frank put it, "shut his eyes to the usualness of what he desired to think the unusual." (*Courts on Trial* [New York, 1963], p. 198.) Only recently have we begun to get sophisticated empirical studies of the relationship between the higher law and the vast stretches beneath.

Macaulay's "text" is the story of the franchised new-car dealers' efforts to better their position in dealing with the major automobile manufacturers. (In passing we get a flavorful sketch of the dealers and the manufacturers' road men.) From the starting point of widespread grievance over insecurity, arbitrariness, and high-pressure tactics, the author traces five "stages" of the dealers' uses of the law. First, the use by individuals of existing legal resources: dealers bring lawsuits against manufacturers, which, when they do not lose, they find the costs of winning prohibitive. Second, the aggrieved organize for attempts at "collective bargaining." Third, they take collective action to change the legal rules: dealer lobbies lead to a scatter of statutes at the state level, and, finally, after extensive public hearings, Congress enacts a Good Faith Act to curb management abuses. Fourth, their opponents respond. The manufacturers manage to water down the federal legislation and, to head off drastic changes, liberalize the franchise contracts and create internal review systems. Finally, there are new battles over the implementation of the new rules.

Macaulay concludes that the dealers were partially successful in elevating their status from that of subordinate and disposable employees to that of more or less tenured appointees. He finds that this cannot be attributed to the direct operation of the new legal rules. Dealers are wholly unsuccessful in suits under the federal Good Faith Act, since the manufacturers occupy the strategic heights in the litigation battle. State penal statutes are entirely ineffective. Dealers enjoy greater success where there are administrative licensing provisions, but here the major impact is not through the licensing system but through informal settlement systems that grow up around administrative officials. Beyond all this there is the serendipitous result of the diversion of management attention to dealer relations, which led to a more efficient system stressing rewards rather than punishments and due process rather than fiat. Thus the relationship between the changes in the rules and the changes in the situation of the parties is indirect. The new rules have their major impact by promoting and supporting the manufacterers' internal review systems and the state administrators' informal grievance procedures.

The law of the courts, while it has an important deflecting influence, is not uniquely determinative of the outcomes produced by the system, nor does it supply explanatory principles pointing to crucial factors in the process. Macaulay concludes that, not only are the upper reaches of the legal system operating in the dark, but so are legal scholars who attempt

to assess the consequences of rules by examining the appellate law. He shows that such assessment requires more than overcoming our preoccupation with appellate courts to examine the lower reaches of the formal system. It is necessary to go outside the formal system to trace the connection of formal rules with low-visibility informal mechanisms and private governments. Macaulay demonstrates how this can be done by going beyond reported cases and legislative history to extensive interviewing of manufacturers, dealers, administrators, and lawyers.

Only when we have other explorations of the linkage between law and practice will we know how typical or atypical is the function of legal rules in the instance reported here. And only then will it be possible to specify the variables which account for the relation of law and practice in various settings. In the meantime, Macaulay's richly suggestive study enables us to look beneath the normative surface of the law without sliding into a corrosive reductionism that brings us clear through to the other side.

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India and Ceylon: Unity and Diversity. Edited by PHILIP MASON. New York: Oxford University Press, 1967. Pp. viii+311. $6.50.

This volume consists of thirteen essays by a dozen scholars. One essay sketches group relations in Ceylon since Independence; the other twelve take up various aspects of the unity/diversity theme in India. The editor's "Introductory Review" summarizes the papers and attempts to supply a common focus that is not evident from the papers themselves. As he puts it, the book's subject is "the diversities of Indian society, particularly those that resemble the cleavages associated with race, and how far they are likely to affect political stability and unity" (p. 5). The agenda consists of the familiar catalogue of cleavages in Indian society: differences of religion, of caste and tribe, of language and region, between elites and masses.

Except for Owen Lynch's fascinating account of urban social organization, these papers do not present much new material of a kind unfamiliar to partakers in such discussions. Nor do they embody or move toward any overarching theory. As might be expected of a "symposium" in which the authors neither met nor saw each other's work in progress, there is little sense of interchange and challenge among them. The virtue of this collection lies in assembling in convenient form a set of stimulating interpretive essays which, at their best, integrate a wide range of materials with thoughtful analysis. Except in the area of Hindu-Muslim relations—which are treated here only in a rather cursory historical sketch containing little reference to recent developments—this collection provides an excellent introduction to the social complexities of Indian politics and policy.

The general tone is one of qualified but emphatic optimism about the unity and stability of India. Finding that India's political resources are equal to the centripetal pressures of language and region, W. H. Morris-Jones observes that "It is India's fate to attract prophets of gloom.... It is her achievement to disappoint them" (p. 57). The authors assemble a list of unifying forces that offset or overcome the divisions. Pre-eminent among them are: Hinduism with its tolerance and compromise, its powerful ideological basis for maintenance of diversity, and its unifying process of Sanskritization; the political structure, manned by a heterogeneous and cohesive elite—and especially the Congress party with its Hinduism-like capacity for accommodating the incompatible.

Most of the contributors seem to concur that the multiplicity of cross-cutting divisions is itself an asset. T. H. Bottomore concludes that "It is partly... the existence of so many different kinds of division, cutting across one another, which explains the cohesion of the political elite" (p. 258). This multiplicity prevents any set of differences from being the single dominant focus of political organization and conflict, as Adrian Mayer neatly shows in his discussion of the role of caste at different levels of political activity.

A Gandhian countercurrent to this sanguine pluralism is faintly discernible. Hugh Tinker subscribes to a Gandhian kind of participatory democracy as ideal but unlikely to be realized. Dennis Dalton, discerning a higher consistency in Gandhi's shifting views on caste, sees the Gandhian dream of social harmony undermined by caste politics which he equates with divisiveness, although others stress that caste not only

divides but propels into a unifying search for allies.

The editor concludes that "the overwhelming impression conveyed to me by the essays in this book is that the tough network of fibres binding Indian *society* into a whole will continue to hold. The divisive forces that simulate racial barriers are in retreat" (p. 26). Since "divisive forces" are an asset as well as a liability, it is not clear that the lesson need be wholly optimistic—as is suggested by C. von Furer-Haimendorf's account of the disintegrative absorption of tribals.

The editor tends to view India over the shoulder of the government: he is worried about "fissions which are liable to disturb the State" (p. 26). Thus the essays tend to focus on demands upon the government and the ability of the government to accommodate these demands. Except for Professor von Furer-Haimendorf, who stresses the gap between policies and their implementation as a major problem, there is less emphasis on the supply side, that is, the capacity of the government to respond to these demands and to implement its policies effectively. Here, compromise, tolerance, and diffidence—the very qualities that make government successful in accommodating demands—may impair its ability to deliver the goods effectively. As the editor concludes, "It is famine, not the internal tensions of society, that is the enemy" (p. 29).

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